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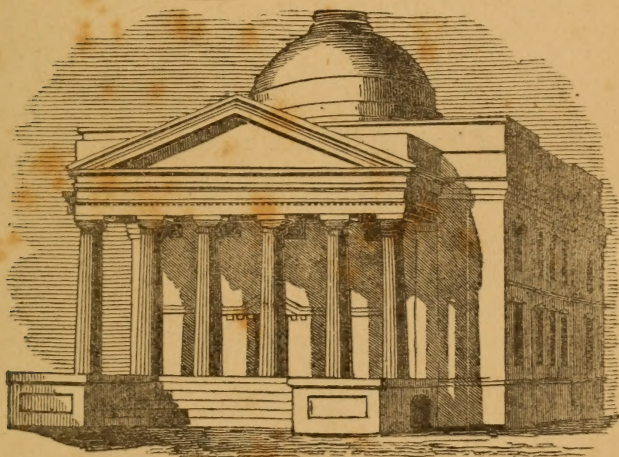
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MEDICAL AND SURGICAL JOURNAL.

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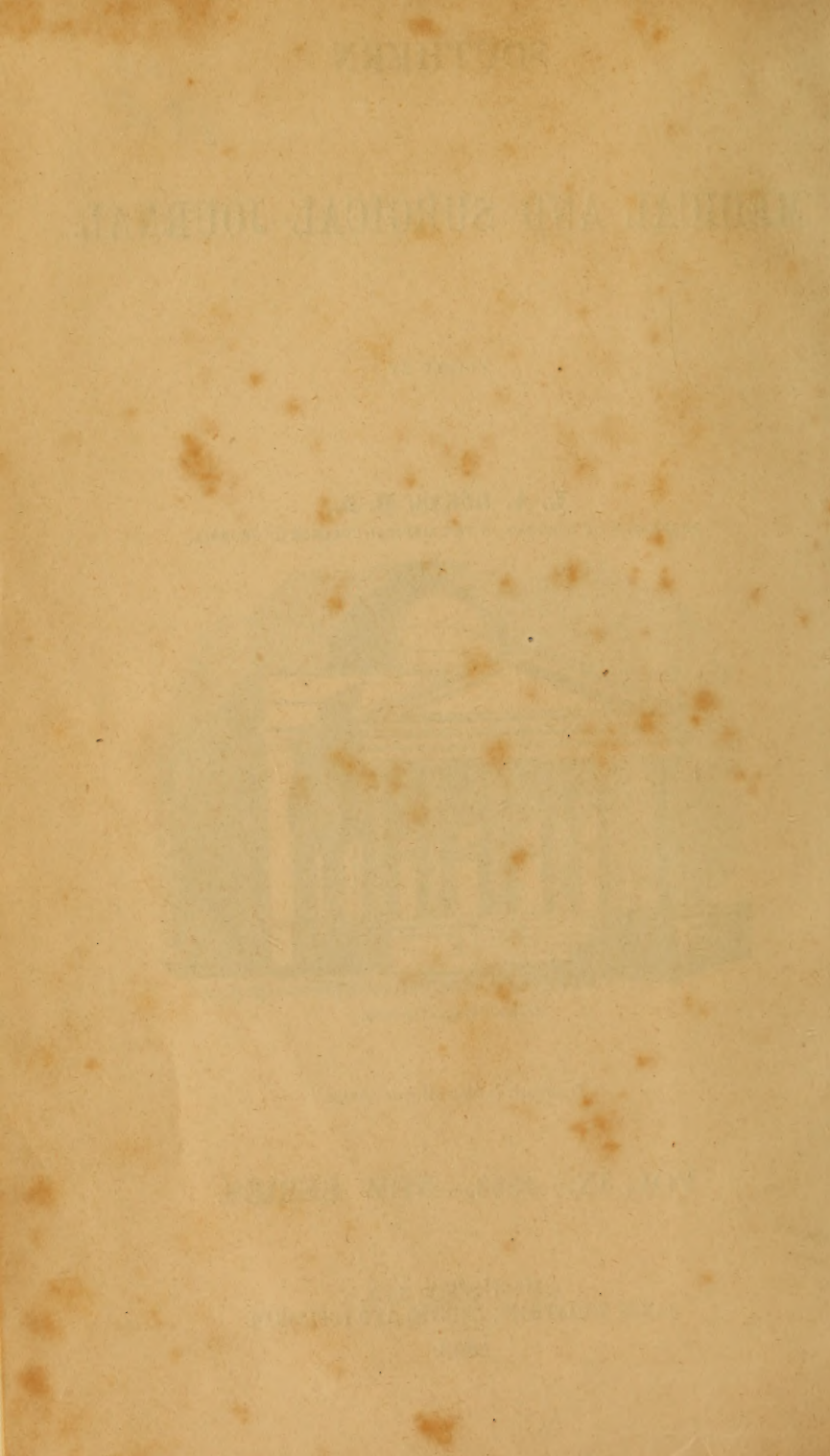
"Je prends le bien où je le trouve."

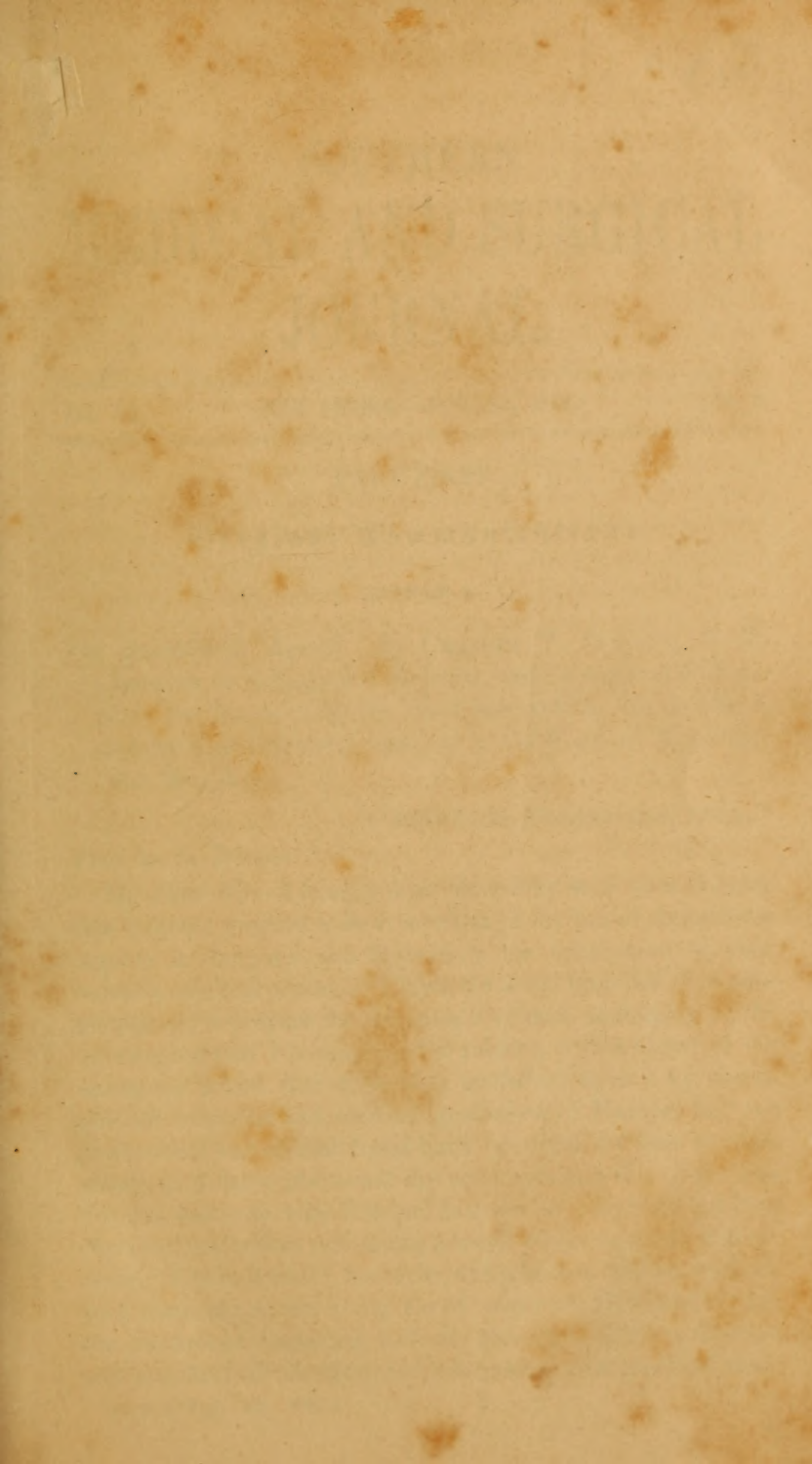
VOL. IX.—1853.—NEW SERIES.

AUGUSTA, GA.

JAMES McCafferty, PRINTER AND PUBLISHER.

1853.







1894

SOUTHERN MEDICAL AND SURGICAL JOURNAL.

Vol. 9.]

NEW SERIES.—JANUARY, 1853.

[No. 1.]

PART FIRST.

Original Communications.

ARTICLE I.

On Anæsthetic Agents. By CHARLES T. JACKSON, M. D.,
Chevalier de la Légion d'Honneur, late Geologist and Chem-
ist to the States of Maine, New Hampshire, Rhode Island,
and the United States; Assayer to the State of Massachu-
setts, &c., &c.

BOSTON, October 23d, 1852.

Prof. L. A. DUGAS:

My Dear Sir—I comply most cheerfully with your request, that I should prepare a short account of the use of Anæsthetic Agents in Surgery and Midwifery for publication in your valuable medical journal. I have for some time had it in contemplation to prepare for the press a volume, containing all the information that I possess on the subject, with a digest of the numerous works that have been published upon it by distinguished scientific and medical gentlemen of Europe, but my professional and official duties have occupied my time so completely as to have prevented my accomplishing this task; and I should desire to visit Europe, in order to collect all the best observations, before publishing a work of so important a character. I have decided, however, to begin here, and to work up such materials as are in my hands; after which, I hope to have the pleasure of consulting with my generous friends in Europe, and to collect all the scattered information of a practical char-

acter that may be needed to complete a work worthy of the subject, and sufficient to meet the wants of the medical world. This, the public may expect from my hands.

I have now before me a number of books and pamphlets, on the use of anæsthetic agents, which have emanated from the presses of Europe—twenty-one of them are in the German language, one in Latin, three in French, and one in Italian, while only two or three pamphlets have been published in England, on Chloroform, and only one regular work on the use of Ether and Chloroform in Child-birth, and a pamphlet on Ether, have, thus far, appeared in America. Unfortunately, the public mind has been more occupied with the disputes which have taken place here as to the origin of the discovery, than with the rational investigation of the principles and practice of Etherization. This, I trust, will cease to agitate us much longer; for the question of discovery has been passed upon by the highest of scientific tribunals—the Institute of France—and it is no longer open for discussion, excepting so far as concerns the appropriation of some reward for the discovery by the Congress of the United States, and even that I have not sought, preferring that the act should emanate spontaneously from the councils of the nation. France and Sweden have already shewn, by liberal acts, their approbation of this discovery, and it is to be hoped that our own country will not long delay that justice which is expected from her hands.

Leaving this question, permit me now to enter upon the subject of the practical administration of anæsthetic agents; and let us enquire, first, what are the means that have been proposed and employed for the production of anæsthesia?

Mesmeric influence, whatever may be its nature, has certainly acted as a sedative to nervous sensibility, and to such an extent as to prevent the perception of pain during severe surgical operations. Of this fact, we have ample evidence on record in this country, in Europe, and in Asia; and though I have not myself seen any successful case in the practice of Boston surgeons, and am not aware of any having occurred in our hospital, I am satisfied, from the evidence of those whose statements may be fully relied upon, that anæsthesia has been really

superinduced in surgical patients by operations of a mesmeric character. I may be allowed to allude to the striking and well attested cases, published in this journal, by Dr. L. A. Dugas, in 1845, pp. 122 and 508, and in 1846, p. 72.

The objection which surgeons make to any reliance on this mysterious nervous agency is, that there are very few persons who are capable of being put into a mesmeric state, even when not alarmed by the terror of a severe surgical operation, and that even facile subjects may be so agitated as to prevent any effectual influence being exerted upon them—therefore, this power, whatever it may be, is not generally available.

If we look back, to learn what other means have been proposed to render persons insensible to pain, we find that Humphry Davy, in the year 1800, published a work entitled, "*Researches, Chemical and Philosophical, chiefly concerning Nitrous Oxide, or Dephlogistigated Nitrous Air and its Respiration*. By Humphry Davy, superintendant of the Medical Pneumatic Institution. London." In this work, Davy gives an account of his experiments on the effects of inhalation of protoxide of nitrogen, and in one of his experiments, he says, while suffering from the pain attendant upon the cutting of a wisdom tooth, "on the day when the inflammation was most troublesome, I breathed three large doses of nitrous oxide. The pain always diminished after the first four or five inspirations; the thrilling came on, as usual, and uneasiness was for a few moments swallowed up in pleasure. As the former state of the mind returned, the state of the organ returned with it; and I once imagined that the pain was more severe after the experiment than before." (Page 465.)

Again, on page 464, he says, after breathing the nitrous oxide, "I imagined that I had increased sensibility of touch: my fingers were pained by anything rough, and the tooth edge produced from slighter causes than usual. I was certainly more irritable, and felt more acutely from trifling circumstances."

This increased irritability produced by inhalation of protoxide of nitrogen, is confirmed by the statement of Dr. Beddoes, the employer of Davy, on page 543, of the same work, who says, "Several times I have found that a cut which had ceased to be painful has smarted afresh, and on taking two doses in succession, the smarting ceased in the interval, and returned during the second respiration."

Thus, it appears from the researches of the discoverer of exhilarating gas himself, and from the experience of his medical instructor and employer, Dr. Beddoes, that protoxide of nitrogen has no anæsthetic properties, and the above quoted experiments were obviously made with an express view to the discovery of some pain-subduing properties in this gas. Davy has the credit of having made early attempts to the discovery of anæsthesia—but he did not find it. Horace Wells, a dentist, verified Davy's failure before the public in Boston, in 1844, and in 1847, he again failed in the same way in the hospitals of New York, thus giving as good evidence as was needed that protoxide of nitrogen is not an anæsthetic agent, in spite of all the depositions which he has published to prove that it was equal or superior to ether.

We shall not consume the time of our readers by recounting the various absurd proposals which were made by a Dr. Hickman, of England—such as the administration of carbonic acid gas by inhalation, so as to produce asphyxia, and of compressing the trachea so as to strangle the patient, and then to operate upon him while in that half dead state; or the method proposed by Morgagni, of compressing the jugular veins until a sort of apoplexy was brought on; for these methods were at once rejected by the French Academy of Medicine, as too absurd for trial and wholly unjustifiable. And we cannot but marvel, that any one should claim for Doctor Hickman the discovery of anæsthesia on account of these barbarous proposals, which, thanks to the humanity of the Academy, were never inflicted upon any surgical patient. Nor will it be necessary for me to look over ancient Persian, Hindoo, Chinese, Greek, or Roman books, to learn that means were sought for and proposed, for preventing pain in surgical operations.* It is enough for us to know that they never discovered any such means, and it is no novelty to us that mankind have always wished for such a discovery. It is well known, that the use of ether vapor for the production of insensibility to pain in surgical operations was made by me, and that the Institute of France so decided, after

* Those who feel curious to learn the opinions of the ancients on this subject, are referred to the work of M. Louis Figuier. *Exposition et Histoire Universelles des decouvertes Scientifiques et Modernes*. PARIS, 1852.

a full examination of the claims of numerous contestants for the honor of this discovery.

The method of operating with Ether Vapour and with Chloroform I shall now proceed to detail, premising the necessary precautions in judging of the purity of the ether to be employed, and shewing how it may be purified from alcohol and acids which are frequently contained in the ether of commerce. Pure sulphuric ether is represented by the formula $C_4 H_5 O$. Its specific gravity from 0.724 at $54^{\circ} F.$, or 0.715 at $68^{\circ} F.$ It is a light, highly volatile fluid, transparent, highly limpid and colourless. Its odour is fragrant and peculiar. It boils at from 96° to $98^{\circ} F.$, and evaporates with great rapidity when exposed to the air, leaving nothing behind if it is pure. Owing to its rapid volatilization it abstracts heat from bodies with great rapidity. It burns with a white flame, and produces carbonic acid gas and water.

Ether unites with alcohol readily, and is completely dissolved by it, but may be separated from it by agitation with water, for water combines rapidly with the alcohol and abstracts it from the ether, which is but slightly soluble in water, and being lighter it floats upon its surface. Owing to this property, we can readily wash out those impurities in ether which are soluble in water, and for that purpose I employ a very simple apparatus, called a washing bottle, which is represented in the wood cut below.



- a* glass jar.
- b* air tube.
- c* tube for drawing off the ether.
- d* division line between the ether and the water.

A stop cork may be affixed to the tube *c*, which may be made of metal, if desirable. These tubes are inserted through a cork.

Take the ether, and evaporate a portion of it to dryness on a cloth, and if there is much odour remaining on the cloth the

ether is impure. Smell of it as it evaporates to ascertain if any sulphurous acid is present. Test it also with a slip of blue litmus paper to discover if it is acid; for if it is, it will turn the blue paper red.

To test it for alcohol. After washing the ether, distill the alcohol from the water, by means of a retort and Leibig's condenser, and the alcohol will be obtained, and may be rectified, as usual, by re-distillation from dry carbonate of potash, or from dry chloride of calcium.

For practical purposes, if the ether has no smell of sulphurous acid, we need only to wash it with about twice its bulk of water. For this, we use the washing bottle above figured. Set the bottle upright, and fill it one-third full of ether, and then nearly fill it with water, and shake it up for a few minutes, and then invert the bottle, stopping the tube *c* before inverting it. Hold it in this position for a short time, until the line of separation between the ether and the water is well defined; then open the tube *c*, and let the water gently flow out, as it will do, on account of the ready passage of air up the tube *b*, to the top of the liquor. As soon as the ether reaches the tube *c*, stop it, and bringing it over the bottle in which you intend to preserve the ether, open the tube *c* again, and allow the ether to flow into it.

This ether, without any re-distillation, is suitable for inhalation; but if it is to be rendered perfectly dry, add a quantity of dried chloride of calcium, and after agitation for some time, distill off the ether, condensing the fluid by means of Leibig's condenser and ice water.

I sometimes have rendered ether less offensive to the smell by agitating it with lime-water, or still better, with bleaching powder, hyperchlorite of lime, which destroys the aroma of some of the volatile oils which it may accidentally contain. This ether is to be again washed with water, and it is then fit for use.

Many of the accidents which have happened in the early use of ether as an anæsthetic agent, (though none of them have proved fatal,) arose from the use of impure alcoholic ether of commerce, and ether containing sulphurous acid; this acid being not unfrequently present in the ether of commerce,

which is not always prepared with sufficient care, organic matter, such as straw, cork, or other things that decompose sulphuric acid, and generate sulphurous acid being present.

The utmost care is required in the manufacture of ether for inhalation, and none but colourless oil of vitriol and purified alcohol should be employed in its preparation.

Mode of administration of Ether Vapour for the production of Anæsthesia.—No other apparatus is required than a large sponge, or a porous towel. A good sponge is the most convenient, and should not be smaller than the two fists of a man—viz., from four to six inches in diameter. The form of the sponge is of no consequence, provided it is large enough.

Let the patient recline in an easy chair, or if on the operating couch, let his head and shoulders be elevated, if he reclines upon his back; but if he lies upon his side, he may remain pro-cumbent. Soften the sponge in water, so as to open its texture, and squeeze it as dry as convenient—pour it full of the ether, and squeeze out enough to prevent its dropping—bring the sponge close to the open mouth of the patient, and let him breathe fully and boldly. The first impression of the ether upon the respiratory organs produces coughing at first, but is soon borne without trouble, and the patient inhales the ether with great eagerness. Let him inhale it as freely as possible, for the best effects are those which are soonest brought about, as no excitement arises if the vapour is given freely, while it is likely to take place if given too gradually. The patient in a few minutes is etherized—his eyes close, and he falls back, as if asleep. If you open his eyelids you will find the eye rolled up, as in sound sleep. The pupil is at first contracted, but soon dilates under a full dose. The pulse, at first quickened, is, under full etherization rendered slower than natural, and generally may be diminished about ten beats a minute less than the natural pulse. Under chloroform, it is not unfrequently reduced from 72 to 30 beats per minute, and it gradually rises as the patient recovers from the effects of the anæsthetic agent.

There is no need of rendering all patients unconscious; for loss of all feeling takes place before the unconscious state commences, and there is no difficulty in keeping the patient for

hours just on the borders of unconsciousness, without passing into that state. The surgeon should charge an assistant with the duty of administering the ether, in order that he should be able to devote his undivided attention to the operation, and the etherizer should devote his attention to his particular duty.

In case the patient has become deeply unconscious, and it is desirable to awaken him, we have only to sponge his face and head quickly with ice water, when he will generally be aroused—at least this method hastens the recovery in a remarkable manner.

It is very important for the surgeon to know that the patient should not be etherized while his stomach is full of food, for he will be made to vomit in most cases, and he is much more refractory towards the etherization. Make it a rule, then, that the patient is to be kept fasting for some hours before he is to be operated upon by ether; also, that he drinks no ardent spirits, wine, or beer, which oppose the anæsthetic effects of ether. Men who are habitually temperate always are the best subjects for etherization, and habitual intemperance often prevents persons from being affected favorably by anæsthetic agents. It is a great mistake to suppose, that etherization is equivalent to drunkenness from alcohol, or from exhilarating gas: it is directly opposed to that state. Alcohol excites to violence, and so does the protoxide of nitrogen—while ether vapour, properly administered, acts very quickly as a sedative, and there is no disposition shewn to violent actions.

It is wholly unnecessary to detail cases, for the world has been flooded with them, and three or four well-observed ones are as good as a thousand. I have made direct experiments upon habitual inebriates, and upon men who never drank any intoxicating liquors in their lives, and I know, from these trials, that ardent spirits act directly against the true and proper influence of ether.

In Parturition, there is rarely any need of rendering the female unconscious. She can, by a little instruction, be taught how to prevent the sensation of pain in each muscular contraction of the uterus, by anticipating it with the ether, a bottle of which she may have in one hand, while she holds her sponge, handkerchief, or towel in the other, and applies the ether over

her mouth as she needs it. Since the introduction of pure chloroform, I have been in the habit of making a mixture of it with ether, and thus avoid the peculiar dangers of chloroform. One measure of pure chloroform, made as I shall presently describe, and four measures of pure washed sulphuric ether, mixed, forms the most convenient anæsthetic agent for the practitioner, since a four ounce phial will contain enough for any case of labour, and twice as much as is required for any surgical operation. This mixture, which should be freshly made for each case, I have found to be perfectly safe, and I have used it for more than five years, in my family, and on numerous patients who have required my surgical aid, without having witnessed a single unfavorable symptom. I have kept an American Indian four hours in an insensible and unconscious state with it, and have administered it to many furious maniacs, in McLean Asylum, with great advantage; the patients gaining rest and sleep for some time, were found to be much relieved, and one furious maniac was cured, for the time at least.

In Midwifery this mixture of ether with pure chloroform operates admirably, and the *accoucheur* may, after instructing his patient in the mode of inhaling it, allow her to take the phial into her own hands, and to shake a little of the mixture upon her handkerchief, just as cologne spirits is used, and then to inhale it from the handkerchief as she feels a pain coming on. If unconsciousness is desirable, as well as insensibility to pain, another person must be charged with the administration of the anæsthetic agent, under the directions of the *accoucheur*. This is always required when instruments are employed for the delivery of the child. I have used this mixture in my own family in cases of parturition, and, without producing unconsciousness, except for three minutes in one instance, have always prevented all sensation of pain, without a single unfavorable symptom accompanying or following its administration. I have also used it in every case when my children required their teeth to be extracted, and with the most agreeable effects.

Pure chloroform is too powerful an agent to be used generally, and since it is never required to be used alone and we have other and safer agents, its farther employment in its concentrated state should be abandoned. Its vapour is so heavy

as to nearly drown the patient, if he happens to draw into his lungs the pure vapour, and one inhalation too much may prove an over-dose and put the patient into a state of stupor beyond the wishes of the operator, and to the no small alarm of the friends of the patient.

There are also poisonous kinds of chloroform in the market, which produce immediate death if breathed in a concentrated form, and the physician and surgeon may be so unfortunate as to procure a bad variety.

The fousel oil, or oil of whiskey, is not unfrequently present in the alcohol used for the manufacture of chloroform, and as I have ascertained by direct experiment, a poisonous chloride of amyle is present in most of the chloroform of commerce, Mr. Atwood and myself having separated it from such chloroform. I have also made the poisonous product directly from a mixture of fousel oil, hyperchlorite of lime and water, and have proved that it destroys life.

It is not, however, certainly ascertained, that this compound of amyle alone produces those sudden deaths, which under the hands of skilful practitioners have so quickly taken place, on the inhalation of impure chloroform, though the fluidity of the blood in these fatal cases seems to indicate that some such body as I have described must have been present, for I found that such results were produced by the amyle compound, when inhaled by animals; but, in my experiments, the death of the animals, a rat and a turtle (*testudo picta*), took place too slowly to resemble the known effects of the poisonous chloroform. I have not yet, however, tried this compound mixed of chloroform, which may cause its more rapid action.

I know that there is a kind of chloroform sold in this market that quickly decomposes and produces some remarkable and very disagreeable products and free chlorine; and I am informed by an eminent chemical manufacturer, Mr. Luther Atwood, that such chloroform, in his experiments, usually results from following the rules printed in the U. S. Dispensatory, 9th edition, page 841—1851; while, by following another method, similar to that which has been published by Prof. Simpson, good chloroform results, but it must be made from very pure alcohol, properly diluted with water. The chloroform which

I use, was manufactured by Mr. Atwood, for Messrs. Philbrick, Carpenter & Co., of Boston, and it was made from alcohol purified by a process peculiar to his own works. This alcohol has no trace of fousel oil in it, and has not the odour of any of the materials from which it is extracted. The most highly rectified and pure alcohol alone is fit for the manufacture of chloroform. The process for making chloroform, as published by Simpson, is as follows:

“℞ Bleaching powder, Hyperchlorite of Lime, lbs. iv.

Water, - - - - - lbs. xii.

Alcohol, - - - - - oz. xii.

Mix in a capacious retort or still, and distill so long as a dense liquid, which sinks in the water with which it comes over, is produced.”

This chloroform must be washed in the washing bottle, before described, with a sufficiency of water, to remove any alcohol or acids it may contain. The chloroform settles to the bottom and may be drawn off by the tube c, of that apparatus. There is no need of a re-distillation of this chloroform from sulphuric acid or from chloride of calcicum—a thorough washing with water is all that is required. This chloroform is to be used with ether, as before described.

There is a mixture of chloroform and alcohol sold under the improper name of Chloric Ether, which, when administered on a wet sponge, is decomposed, the alcohol all leaving the chloroform and uniting with the water, so that nothing but chloroform is really administered—not a particle of the alcohol rising in the vapor inhaled by the patient. The only advantage which can possibly arise from the use of this preparation, is the division of the chloroform into fine globules in the wet sponge; but it is an uncertain mixture, and is as objectionable as pure chloroform—the use of it in a case at the Marine Hospital in Chelsea having produced death in a short space of time, in the same manner and with the same morbid appearances resulting, as are known to characterize the fatal administration of bad chloroform.

I have seen cases of violent and very dangerous convulsions continuing for three days, from the administration of chloroform, for the prevention of pain in the extraction of a tooth. These

convulsions were found to be best controlled by the administration of proper doses of morphine, and the patient recovered in the course of a week.

The first chloroform ever manufactured in the United States, for anæsthetic use, was made by me in my private laboratory, and was, after being tried upon myself, administered to my pupils. I then furnished physicians and surgeons with a sufficient quantity to prove its efficacy, and made an exhibition of it at a scientific club, where I administered it to our celebrated surgeon, Dr. John C. Warren, and to Mr. Joseph M. Wightman. I subsequently administered it to physicians and to others in public lectures, and have employed it to some extent in various petty surgical cases which fell into my hands while I was engaged in public geological surveys. I have therefore had some experience in the use of this agent, as well as of ether, and although I have met with no accidents in practice with it, I must confess that I prefer my original anæsthetic agent, pure ether, or its admixture with pure chloroform.

Dr. Simpson, who substituted chloroform for ether, at the suggestion of Mr. Waldie, the chemist, still adheres to the administration of chloroform, but he is extremely careful to procure the purest kind, and will not employ that which is sold in the market unless he knows who made it, and that it is pure. I am not aware of any fatal case having happened under the management of Simpson, and this must be owing to his great care in selecting pure chloroform. If ether has not caused death in a single instance, and I am not aware of one having yet taken place in consequence of its use, why should we not adhere to its employment in all cases? It is objected that its odour is not so agreeable as that of chloroform, and that it is more bulky and must therefore encumber the practitioner. These objections are in a great measure removed, by the substitution of the mixture of ether and chloroform, as employed by me, and with due care little danger is to be apprehended from the use of this mixture.

For philosophical experiments, we may wish to employ other substitutes for ether, and therefore I shall give a list of those which possess anæsthetic properties, with some remarks upon their peculiar effects upon man and animals.

They are, as given in M. Bouisson's work on Anæsthesia—
1st. Hydrochloric Ether, (first employed with Sulphuric Ether, by M. Flourens, of Paris, in 1847.)

2d. Acetic Ether. (M. Figuier.) See *Comptes Rendus* of the French Academy of Science, 1848, and M. Bouisson's *Traité*, 1850. Paris.

3d. Nitrous Ether. (M. Flourens.)

4th. Nitric Ether. (Dr. Simpson, on animals.)

5th. Aldehyde. (M. Poggiale.)

6th. Chloride of Hydrocarbon. (Dr. Nunnally.)

7th. Formomethylæ. (M. Bouisson.)

8th. Benzine. (Drs. Simpson and Snow.)

9th. Bi-sulphide of Carbon. (Name of experimenter not known.) Norway Journal.

Of these, I have tried Nos. 1, 5, 6, and 9, upon my own person, and No. 9, I have administered to my pupils, with rather alarming results, and have also tried it upon myself. It is too powerful, and is dangerous. I have tried, too, chloride of methyle, and found it to act precisely like chloroform, or the ter-chloride of formyle. I have tried formic ether, and cænanthic ether, with unpleasant effects, and without anæsthesia being produced by them. Benzole has been breathed by me, but its effects were not satisfactory, and it produced considerable irritation in the fauces and larynx. I do not regard these agents as susceptible of being substituted advantageously for pure sulphuric ether.

I will give here the results stated in M. Bouisson's admirable *Traité de la Method Anesthésique*, (Paris, 1850,) respecting those agents that have not been tried by me, remarking that M. Bouisson himself is decidedly in favor of adhering to the use of ether vapour, as originally proposed and employed by me.

Chloride of ethyle, or hydrochloric ether, is made by saturating alcohol with chlohydric acid gas, and by distilling the liquid in a water bath, and condensing the ether in a flask of water surrounded with ice and salt. Hydrochloric ether boils at 11° centigrade or 42° F. M. Flourens first employed this ether in the place of sulphuric ether, and for comparison with it, and found that it produced the same effects. It is so volatile that it is difficult to keep, and can only be used in cold weather;

hence, is not employed to any extent in actual practice, for it would be necessary to keep it constantly surrounded by ice, to prevent its escape by evaporation, as it boils at 42° F.

Acetic ether, or acetate of the oxide of ethyle, is a colourless liquid, having an aromatic odour, somewhat between ether and acetic acid. Its density is 0·86, and boiling point 74° cent. or 95° F. This ether being less volatile than oxide of ethyle, is not so readily administered by inhalation, but, according to Figuier, it produces effects analogous to those of sulphuric ether. Tried on a puppy of 2½ months, by M. Figuier, it produced insensibility in five minutes, so that no pain was occasioned by amputation and cauterization of his tail, but it took ten minutes to render an adult dog insensible to pain. M. Bouisson has tried acetic ether in surgical operations on man, but did not obtain such prompt results as with sulphuric ether. It required twenty minutes to produce insensibility in a patient from whose cheek a small cancerous tumour was excised by that surgeon. He thinks that this ether is suitable for persons who are easily affected by anæsthetic agents, and that it produces less coughing than the sulphuric ether. M. Chambert agrees with M. Bouisson in this opinion.

Nitrous ether, (nitrite of oxide of ethyle,) density 0·95, boils at 16° cent. or 51° F. This ether is a *deadly poison*, and its use must be carefully avoided. M. Flourens has verified its poisonous properties by experiments on animals, and a servant maid of a druggist was killed by sleeping in a room where a carboy of it was broken under her bed.—(Ed. Med. and Surg. Journal, t. xxxv., p. 452.) This ethereal fluid, of course, is to be thrown out of the list of anæsthetic agents.

Nitric ether (nitrate of ethyle) has been used in experiments on man and animals, by Dr. Simpson, who declares that it produces anæsthetic effects on man when fifty or sixty drops of it are inhaled from a handkerchief. He regards it as free from danger. But this is not a reliable opinion, for severe headache and vertigo follow its inhalation, as I know, and from my experience I would not recommend its trial. In case it were breathed too freely, I have no doubt it would produce fatal results. I therefore reject it.

Aldehyde (hydrate of the oxide of acetylene) was proposed by

M. Poggiale, Professor of Chemistry in the Val-de-Grace in 1848, as a substitute for ether and chloroform. He had tried it on animals, with what he regarded as successful results; but experiments made by others, on men, proved that it was a very disagreeable, if not a dangerous substance. Dr. Simpson found that it produced a severe cough, and great dyspnoea and headache. The same results were obtained by me, in experiments on my own person, in 1847, and I at once denounced aldelyde, as not being a safe or proper anæsthetic agent.

Chloride of hydrocarbon (or the Dutch oil) was proposed by Dr. Nunnally, of Leeds, as an anæsthetic agent; but experience has proved that it produces a severe irritation of the throat, and is not so good an anæsthetic agent as chloroform.

Formomethylæ is mentioned, by M. Bouisson, as an anæsthetic agent somewhat between ether and chloroform. It is obtained by distilling wood naphtha (*esprit de bois*) with a mixture of diluted sulphurous acid, and per-oxide of manganese. It is an agreeable aromatic liquid, boils at 42° cent. or 107° F. It has been tried upon dogs with success, but never upon man. Its value cannot be superior to that of sulphuric ether, though regarded by M. Bouisson as between ether and chloroform; furthermore, it is a rare and costly substance, and is not known in commerce.

Benzine, produced by the distillation of benzoic acid with lime, has been proposed as a substitute for ether and chloroform, but it has proved unsuccessful in the hands of Drs. Simpson and Snow, who first proposed to employ it, and it should not be used in operations on man.

Bi-sulphuret of carbon is obtained by passing the vapour of sulphur over red hot charcoal, and collecting the volatile product in pounded ice, and then re-distilling it, and condensing it again in ice and water. This very disagreeable smelling liquid was proposed by some one in Christiania, in Norway, as a substitute for ether. I do not know the name of the writer, but the article was published in a Norwegian newspaper, called the *Morgenblad*, in 1848. I immediately undertook a series of experiments on this volatile liquid, and found that it did possess anæsthetic properties of greater power than chloroform, but that its use was not only disagreeable but also quite dangerous,

owing to the remarkable effects of the bi-sulphuret of carbon vapour on the circulation, and on the respiratory organs. Its effect is very sudden, and a person under its influence is as pale as ashes, and his lips are of a blue colour—hence there is partial asphyxia. After trying it upon my own person, and repeating the trial on one of my pupils, I concluded that it would be dangerous to allow the introduction of such a powerful agent into general practice. Its use cannot be safe in any but the most skillful hands, and even then I should fear that fatal results would occasionally take place. It is not recommended by those who have tried it in France.

From the foregoing *resumé* of all that is known of the proposed substitutes for ether, I think the reader will come to the conclusion which we have long since arrived at in Boston, that none of the substitutes that have thus far been proposed, have on trial been found to be equal in value, so far as efficiency and *safety* are considered, to the preparation originally employed by me in my first experiments, viz., the *oxide of ethyle*, or pure sulphuric ether vapor mingled with air.

We are in no way opposed to improvements upon my method; but the improvements must be proved to be such, before we can admit them; and we are convinced that there is little reason to believe that any substitute will be found that will, in all the essential requisites, surpass that for which I have expressed my preference.

ARTICLE II.

Veratrum Viride, or American Hellebore. By W. C. NORWOOD, M. D., of Cokesbury, South Carolina. (Continued from p. 653 of Vol. VIII., N. S.)

We gave in the No. of this Journal for November last, an account of what was known in reference to the effects of *veratrum viride* previous to 1850, of the manner in which we used it and of the cases treated with it. This paper might be said to contain the experimental process adopted to determine its powers in controlling arterial action, and the effects witnessed during these inquiries. But, to proceed: We soon

discovered, to our surprise, that in almost every instance, so soon as nausea or vomiting was excited the pulse became slow, full and distinct, the skin cool and often soft and moist, and in some cases bathed in a most profuse perspiration, with entire relief of pain in a number of cases and materially mitigated in others. The cases in which there was no abatement of pain were very few. The mouth and tongue grew moist, breathing and expectoration more free and easy, and by continuing the remedy, in doses short of the nauseating point, from one to three days longer, there would be no return of the symptoms in a large majority of the cases in which the disease was subjected to early treatment. In a small number of cases, if not continued longer, the symptoms would return on a suspension of the remedy. In very few cases we have had to continue the tincture from five to twelve days. These cases are exceedingly rare, and were often treated with other remedies or suffered to run some time without any treatment. We had, by a continued series of experiments and observations, arrived at the fact that, in nearly every case, we could reduce the pulse to any point we wished; that by putting the patient under its influence, we could predict with certainty that the pulse would range between 56 and 85 beats per minute.

Before going any further, we beg leave to present the following two cases of pneumonia—the one on account of the interest of the occasion, and the other because of the peculiarity of the effect:

In 1846, we were called to see Mr. E., in consultation with Dr. J. A. Stewart. Mr. E. had been laboring under a severe attack of pneumonia for several days. The remedies prescribed were entirely approved of and continued for a time, but failed to relieve. The threatening aspect of the case was such, that it was thought prudent to inform his parents, at a distance, of his perilous condition. At this critical juncture, we observed to Dr. S. that we had been using an article in a number of cases of pneumonia, with a success and peculiarity of effect we had never been able to obtain from any other remedy, and proposed to use it in the present case. We immediately put Mr. E. on the use of the *veratrum viride*, to be given every three hours—the quantity to be increased one drop

at each dose until nausea or vomiting occurred. At 8 o'clock, A. M., commenced with seven drops. The third portion excited severe nausea and free vomiting, with great paleness, coolness and moisture of the surface. During the occurrence of these interesting and striking effects, we were notified that Mr. E. was vomiting freely, was much worse and was thought to be dying. We found, however, that what had caused so much alarm to the patient and his friends, was to us a source of gratification; for, after the effort at vomiting was over and nausea relieved, the pulse was reduced to 63 beats and the pain relieved.

In this case, a pulse of from 120 to 130 beats, was reduced in twelve hours, to 63, and all the febrile and inflammatory symptoms were relieved. This was to us an occasion of thrilling and exciting interest. Dr. S. was the first physician to whom we had stated our belief in its powers, and he now stood before us witnessing the most commanding demonstration of the powers of the agent over a disease of acknowledged fatality and under the most unpromising circumstances. Who would charge us with wanton enthusiasm? or who would fail to be enthusiastic on such an occasion? The portion was reduced to one half and continued several days without any return of the symptoms, and the patient rapidly convalesced.

We were at one time impressed with the belief that nausea or vomiting, one or both, was essential to the control of the heart. Called, in February, 1847, to see a son of Mrs. T., labouring under a violent attack of pneumonia, we put him on the use of *veratrum viride* every three hours. Although 12 years of age, his general slender health and deformed chest, having been severely afflicted with asthma, induced us to commence with a very small dose, that we might avoid any drastic effect of the remedy. The first portion given was two drops, to be increased one drop every portion until the slightest nausea was experienced, then to lessen or discontinue the remedy, as the case might require. On taking the third or fourth portion, Mrs. T. discovered that he was getting very pale, that the skin was cool and moist, and the pain scarcely felt only on taking a full inspiration. The slowness of the pulse, and the palor and coolness of the surface alarmed her, and she sent for us.

We found him pale, cool, moist, and with a pulse beating 35, full and distinct. When put on the tincture, in the morning, his pulse was 120 to 125, skin hot and dry, frequent and labored breathing, pain severe, great thirst. In the short space of twelve or fifteen hours the symptoms were subdued, and by continuing the tincture in doses of from two to three and four drops, there was no renewal of the symptoms.

Since the above, we have been able, in a number of cases, to succeed in reducing the action of the heart and arteries, without exciting the least nausea or vomiting, by commencing with a very small dose. Nausea and vomiting are therefore not absolutely necessary. We might enumerate many other cases of like success, but deem it unnecessary to detail any more at present.

In 1850, we determined to announce to the world the fact, that *the great desideratum had been discovered: an agent by which we could emphatically say to the heart and arteries, thus fast shalt thou beat, and no faster.* Aware of the fate of many remedial agents urged upon the attention of the profession, and which have proved valueless, we withheld our notice until we had, by the utmost care and observation, acquired the conviction of its being as much a specific in pneumonia typhodes, as quinine is in intermittent fevers. We leave it to an enlightened profession to judge whether or not the agent has failed to answer or equal the representations made.

We now began to reflect upon the fact, that in a very large majority, if not in every disease of violence, a frequent pulse is manifest, and that we judge, in a great measure, of their intensity, by its frequency and the condition of the vascular system. We asked ourselves the question: if *veratrum viride* will control arterial excitement, break up and arrest pneumonia typhodes, why should it not succeed in arresting other fevers and inflammations? Believing, as we did, that the altered and vitiated condition of the secretions were the consequence of increased and perverted circulation, and that the degree of their morbid condition might be measured by that of the vascular system, we concluded that the *veratrum viride* would cure other febrile and inflammatory affections by its specific action on the heart. We were therefore led to test the *veratrum viride* in a number

of diseases, and the result of a limited portion of our experience was given in the January numbers of this Journal, for 1851 and 1852.

In nearly all, if not in every acute disease, especially of a febrile and inflammatory character, we find the frequency of the pulse and the derangement of the vascular system in proportion to the force and severity of the case. There is scarcely an exception to the rule. Why this is so we do not know. The fact cannot be denied; and in order to restore health, we must, of necessity, control the circulation, directly or indirectly. Now, *veratrum viride* will almost invariably effect this, whatever may have been the disturbing cause. The how and the why, we do not understand. We look upon the universality of its application to be exactly defined by the universality of the occurrence of increased cardiac action. In testing its powers, we did not confine our experiments to febrile and inflammatory diseases of an idiopathic character, but extended them to traumatic lesions in which fever and inflammation had supervened, and our labors were crowned with a success that we little dreamed of realizing. Its power of controlling arterial action, in febrile and inflammatory diseases and in traumatic lesions, we consider established beyond doubt. We gave the statement of a case of convulsions, treated with the *veratrum viride*, in the January No. (1851) of this Journal; since which time we have treated a number of others, with great success. We have not used it in epileptic convulsions sufficiently to enable us to speak with confidence and certainty in that disease. In the case of a Mr. S., whom we commenced treating in January last, and still have under treatment, there has been no return of the paroxysms since then, which is a much longer interval than he has enjoyed for years, and his general health is much improved. It stands unrivalled in palpitations of the heart, for promptness and certainty of relief. It is a specific in the painful affection of the testicle consequent upon the mumps. We have not failed, in a single case, to obtain relief from the pain and fever in twelve hours, and prevented a return of the symptoms, by perfect rest and a continuance of the tincture for three or four days. How far it will succeed in orchitis, from other causes, we are not prepared to say. It affords us no ordin-

any pleasure, to record its value in the treatment of the inflamed mamma of lying-in females. If taken in time, in these cases, it may be relied on to control the fever, pain and inflammation of the gland, so as to prevent suppuration in almost every instance. It is valuable in inflammation of the brain. In whooping-cough, accompanied with high febrile excitement, it has no equal. In convulsions generally, it is highly valuable. In asthma and rheumatism its effects are peculiarly striking, especially in the acute forms. In chronic rheumatism we have not used it. In puerperal fever our experience is limited, but the few cases in which it was used stamps it a reliable agent in that disease. We have found it of great value in the treatment of typhoid dysentery, and would feel unable to combat that disease without it or some other remedy of equal power. Its effects on the system are in perfect antagonism to those of scarlet fever. Combined with the diuretic treatment, we do not believe it can be equalled by any other plan of treatment that has ever been adopted in scarlet fever. We know it to be valuable of itself, but its powers are greatly increased by the above combination.

When we reflect upon the power of *veratrum viride* to allay pain, irritability and irritation, and more especially irritative mobility, in connection with its influence over the heart's action and deranged secretions, it is truly difficult properly to appreciate its value. We know of no untried agent that we would venture to rely on with more certainty in the treatment of yellow fever, and we look with interest for the results of its trial in this disease. From its direct influence over the vascular system, we believe it will prove valuable in the treatment of small-pox, and by keeping the excitement down and the surface cool and pale, it will perhaps prevent the unsightly pitting which often takes place in that disease. Our remarks will be concluded in the next, which will embrace the treatment of typhoid fever.

ABBEVILLE C. H., S. C., November, 1852.

Our last contained a statement of the valuable results obtained in the treatment of Pneumonitis, and a brief notice of the fact that the reduction and control of the heart and arter-

ies were not the consequence or result of the nausea and vomiting, but were independent of either of these; also a notice of a number of diseases in which we had tested its value. We must confess, that notwithstanding the time and space already occupied, that we have scarcely entered the threshold, much less exhausted the subject. It would take a volume to unfold the powers and effects of *veratrum viride*, and the almost innumerable cases to which it is peculiarly applicable. The powers and properties of *veratrum viride*, when fully known and understood, will open new fields for thought and investigation, and give greater scope for practical research in all that relates to the pathology and treatment of disease, than any agent that has ever enlisted the attention of the medical world; and we are persuaded that it will completely change many of the existing views of pathology, and simplify the treatment of disease to an extent unparalleled in the history of medicine. Then, as a matter of course, my feeble pen is not equal to the task of embodying in a few short essays, written at numerous and interrupted periods, snatched from professional exactions, all that might be necessary to a full knowledge and understanding of its powers. The articles will therefore be clogged with numerous repetitions and unavoidable tautology.

We now enter on the most important and interesting part of our subject, viz—its value in the treatment and cure of Typhoid Fever—a disease whose fatality renders big with interest any thing proposed for its cure. The treatment of typhoid fever is a matter in which every individual is deeply interested. Might we not ask with emphasis, what country, what community, has not felt and heard of the destructive mortality following in its wake? and has not the cry been echoed back by every tongue and breeze—a remedy to stay the fell destroyer's progress! When we have presented as much of facts and evidence as we deem sufficient on the occasion, you will be able to judge and others can determine whether a cure has been discovered and the destroyer stayed or merely checked; when the value of *veratrum viride* in pneumonia typhoides and other malignant and fatal diseases, is embraced in the subject, it becomes doubly interesting and important. In 1850 we first entered on a trial of the tincture

of *veratrum viride* in the treatment of typhoid fever. It was due to our patients and to justice that we should proceed with caution. We accordingly, at first, gave it in mild and moderately severe cases, avoiding its use at first in all cases of unusual severity and malignancy. We first used it in the case of a negro boy of Mrs. W., which was uncomplicated and yielded readily. When called, on the third day of the disease, the bowels had been moved sufficiently by a cathartic of calomel, followed by repeated portions of camphorated Dovers powder, without abatement of the symptoms. The skin was hot and dry, great thirst, severe pain in the forehead; the eyes dull, heavy and ecchymosed; tongue covered in the centre with a dark, thin fur, tip and edges very red and dry; pulse 127, small, soft and with a quickness in the stroke, that indicated greater frequency than really existed. The patient was ordered a six drop dose, to be increased till nausea or vomiting occurred. By mistake, the dose was not increased. After continuing the treatment twelve hours, there being no abatement in the symptoms, we were notified of the fact and wrote to increase until an impression was made and that we would see the patient in twelve hours. During the absence of the messenger, Mrs. W. discovered that the dose was to be increased, and did so, and when this reached eight drops there was free vomiting, with a subsidence of all febrile symptoms, the severe pain in the head excepted. At the expiration of twelve hours, we found the boy with a skin cool and moist, thirst materially abated, and the pulse reduced to fifty-six beats. A blister was applied to relieve the unmitigated pain in the head, and the *veratrum viride* was continued four days without any return of the symptoms.

Other mild cases were treated with the same rapidly favorable and successful results. We were thus emboldened and warranted in extending it to the treatment of cases much more severe and malignant, as were those of Mr. R., the son of Mr. W., the two at Dr. Q.'s, and that at Dr. T.'s—all of which, except the first, were published at length in the January No. of this Journal in 1851.

While on a visit to Georgia, in July, 1851, we were asked by Dr. M. to look at a negro woman of Mr. T.'s. She had been sick a number of days, with no abatement of the symp-

toms. Pulse 116, skin hot and dry, tongue red and dry, great thirst, more or less delirium, and a peculiar nervous motion, or more properly, a tremor and inability to hold the head still or to take a drink of any thing out of a cup or tumbler with her own hand. The owner was exceedingly uneasy about the condition of his negro, as a great many had fallen victims to that disease. We might have noticed the gurgling noise, sickness at the stomach, and spinal tenderness, which had resisted the use of blisters to the stomach and spine, as well as cupping of the same, together with an alterative treatment of calomel. On being asked our opinion, we observed to Dr. M., we thought the fever could be cooled and the pulse reduced. By request, we remained five hours, put her on the use of the tincture of *veratrum viride*—gave her seven drops at 12, eight at 2, P. M., and nine at 4, P. M. In half an hour after the the third portion nausea and vomiting were excited moderately. The pulse was reduced to 80 beats per minute, the skin became cool and moist, and the nervous tremor or motion very much relieved. The Doctor observed, that the pulse was reduced as low as he wished it; the dose was consequently reduced to four drops, to be given every three hours. A son of Mr. T. was also sick of typhoid fever. His case was mild, the pulse at the highest numbering but a very few beats over one hundred. When the effects on the negro woman were known, he was quite anxious to take it also. Accordingly, he was ordered it every three hours, beginning with seven drops; to be increased one drop. The third dose excited severe nausea and free emesis, producing cool and moist skin, and reducing the pulse to 58 beats per minute. The portion was then reduced to four drops, at intervals of three hours. The next morning, at 9 o'clock, found the negro's pulse 80; delirium entirely gone, and full relief of all nervous tremor; skin cool and moist; tongue moist, and little or no thirst. The son's pulse was from 58 to 60, other symptoms in unison.

The above was an occasion of interest and solicitude to us, for the time, and our feelings can be much better imagined than expressed. Dr. M. dismissed the cases within thirty hours after we first saw them, the medicine to be kept up for a few days, and he to be notified in case of change for the worse.

Dr. W. saw them, and Dr. S., then a student, was also present. Dr. W. has since used the *veratrum viride* extensively, and with great success. This circumstance led to its introduction into that region, Coweta, Troup and Heard, as Dr. M. practiced in the three counties. The letters of Dr. M., Dr. Ridley, and Dr. Renwick, are testimonials of their opinions of its value and beneficial effects in the treatment of typhoid fever, &c.

We were called, with Dr. P., to see a negro girl of Judge B.'s, on whom he was attending. The girl was severely sick with typhoid fever, which had been unusually fatal in that region. The pulse was from 120 to 130, when at the highest; tongue dry, and red on edges and tip, dark brown or black in the centre; great tenderness of the abdomen; gurgling or rumbling and tympanitic abdomen; decubitus on the back; feet drawn up; knees separated; muttering and delirious while inclining to sleep, especially during the night; tendency to diarrhœa; tip of the nose peculiarly sharp or pointed—had been treated with calomel, turpentine, and camphorated Dover's powder. It was on the eighth day we saw her, and, with desire of Dr. P., commenced giving the tincture every three hours. The patient being ten or eleven years of age, we commenced with two drops, and increased each dose one drop. In thirty hours the pulse was reduced from 120 to 90 beats per minute, surface became cool, and mouth and tongue moist. In fifty hours the pulse was reduced to 70, at which time she was nauseated and vomited—it was kept at between 75 and 85, till she was fully convalescent, and did not exceed that point, unless suspended, or given at too great intervals.

The Judge observed, after we had been in attendance for a few days—"you say she is better, and will get well. I have had two to die in my family already, and she will die also." We are pleased to say that we were correct in our prognosis, and that when we left, the patient was sitting up and able to walk about, had a good appetite and regular bowels. This was a case of no ordinary interest, as in that immediate section many had denied the efficacy and the powers of the *veratrum viride*, but had witnessed the mortality of the disease under every other mode of treatment. This closes the history of three cases we assisted in treating in Georgia. We will again

turn to our own, and an adjoining district, and give the cases of most interest.

On the 14th July, 1852, we saw, in consultation with Dr. C., Mr. C. It was the sixth day of his relapse. Pulse 120, small, soft and weak; gurgling on pressure, and tenderness in the right iliac region; bowels flatulent and slightly tympanitic; burning in the palm of the right hand; edges and tip of tongue dry and red—slight white fur on the tongue, which we attributed to calomel; preternatural wakefulness. Had been treated with alterative doses of calomel and Dover's powder—had taken an emetic. Gums slightly distended from calomel, fetid or mercurial breath, and a number of small ulcers on the cheeks and tongue; skin dry; bowels inclining to diarrhœa, but readily controlled. By consent, was put on the tinct. of *veratrum viride* every three hours, to be increased slowly, and to avoid emesis, as he was opposed to taking it till it produced this effect. This is a great error; for those who take it till free emesis is excited, and the liver properly aroused, convalesce much faster. Commenced with three drops, and increased one drop every portion given, till six were taken, and slight nausea produced. It was then reduced to three drops, or more, according to effect. On reaching six drops, the pulse was reduced to 80, and kept from 80 to 85. By continuing this treatment for a number of days, the pulse was reduced as low as 70 in the morning, with the skin rather cooler than ordinary, and towards sun-set it would get up to 80 or 85, and the skin would be rather warm, and accompanied with more or less restlessness till midnight, and then pass off. It was suggested to try a few portions of quinine. The morning on which he took the quinine, the pulse was 70, skin cool, mouth and tongue moist. A portion of quinine was given at 9 and 11. Before 1, his pulse was from 130 to 135, the skin hot and dry, and a general aggravation of all the febrile symptoms. The *veratrum viride* was resumed in full portions for a few doses, which soon subdued the excitement, and was continued. Convalescence was slow but perfect. It is an error, not to reduce the pulse as low as sixty in many cases. There is as much febrile excitement in some, with a pulse of 80, as there is in others with a pulse of 90 or 100; consequently, when this is the case, the convales-

cence will be extremely slow. In such cases, the *veratrum viride* should be given till free emesis is excited, and the pulse should be kept at 60 or under.

On the 19th July, 1852, we were called into an adjoining district, to see a negro woman of Mrs. G.'s, in consultation with Drs. T. and McD. We saw her at 8, A. M., on the 20th, the twelfth day of the disease. She had been treated with all the remedies usually resorted to, without relief. She was slightly mercurialized; supposed to be three months advanced in pregnancy; pulse 130, extremely quick and weak, so much so that it was difficult to count; tongue dry and red on the tip and edges, with a thick dark fur in the centre. The papillæ were not covered with fur, were elevated, enlarged and flattened at top; thirst extreme; great heat in the region of the stomach, and complaining of internal heat and burning; extremities cold, with general coolness of the surface, except over the region of the stomach; answered questions in a quick and hurried manner—would invariably change some part of the body before giving an answer. Discharges from the bowels dark and muddy, mixed with slime; more or less tenderness and gurgling on pressure in the right iliac region; tendency to diarrhœa slight. On the ninth day from the attack, there was a sudden and decided change for the worse, and brandy and quinine were freely given to sustain the action of the heart and arteries, and the surface was thoroughly rubbed to keep up external warmth.

We have given such a description of the treatment and condition of the patient, at the time of our first visit, as will be fully endorsed by the physicians in attendance. Two cases had just terminated fatally in the same family, and two others in a family not more than six hundred yards distant. We could not complain of the reputation that had preceded us; but the standing of the medicine was anything but favorable in that region of country. The previous and threatening mortality, the severity of the case, the new remedy, the unfavorable prognosis of the physicians in attendance, naturally excited the deepest interest, and curiosity was wrought up to the highest point as to what course would be pursued. By consent, every remedy was discontinued, both internal and external, and the

tincture of *veratrum viride* ordered every three hours, to be increased *pro re nata*, which we superintended in person from 9, A. M. till 5 P. M. Three drops were given at 9, which nauseated and vomited pretty freely before 12. The first matter thrown up was a large quantity of mucus and slime, followed by a quantity of dark, thick bile, or bitterish fluid, on the ejection of which she expressed considerable relief from the unusual burning or heat in the region of the stomach. Four drops were given at 12, which excited free emesis in from thirty to fifty minutes, bringing up an abundance of thick, yellow bile. After this paroxysm of vomiting had subsided, the extremities and surface generally became warm, or, in other words, there was a general diffusion and equal distribution of heat. She expressed perfect relief from internal heat or burning, followed by a general feeling of agreeable coolness; but three drops were given at 3 o'clock, which excited slight nausea, and perhaps a slight but single paroxysm of vomiting. What we had achieved when we left (at 5, P. M.) was the relief from unusual heat in the stomach, severe thirst, general restlessness, an equable diffusion of heat, and greater fulness and distinctness of the pulse. Instructions were left to continue the *veratrum viride* in three or four drop doses, as she might be able to bear it, avoiding too much nausea and vomiting, if possible. After leaving, we sent a message back to give twenty or thirty drops of laudanum, one hour before the next portion, to prevent nausea or vomiting, if possible.

That night, as matter of course, was passed by us with more or less anxiety and interest. On reaching the patient the next morning, the *viride* was exciting very little nausea, the pulse was reduced to 120, more full and distinct, and all the other symptoms were slightly improved. We were not satisfied with the small quantity of the *veratrum viride* we were using; we therefore ordered an enema of four ounces of cold water and six drops of the tinct. of *veratrum viride* every six hours, and the three drop doses, every three hours, to be continued, thus making, in all, forty-eight drops in the twenty-four hours. The enemata were ordered to be given between the portions by mouth. The nausea and vomiting were kept up for a time after each enema, but not to an extent that required them to be

suspended, and which subsided after a few repetitions of the enema.

The morning following, which was the fourteenth day of the disease, the pulse was down to 100, and with a like improvement in all the symptoms. The morning following, the pulse was reduced to 85, and all the other symptoms were greatly mitigated, so much so that we were not to see her for the next forty-eight hours. On Sunday morning, at 9, A. M., (the seventeenth day of the disease,) we were at our post, with our pleasing anticipations disappointed, blasted, and, for the time, scattered to the winds,—but to fight the battle at far greater hazard. Found her flooding; pains severe and frequent. Requested Dr. T. to examine the condition of the uterus; found the os tincæ soft and dilated, so that he could discover a substance or body presenting; gave her a portion of ergot; the fetus was thrown off within a half hour, and flooding ceased. By this time the pulse had reached 135 beats per minute, was peculiarly quick and feeble; number of respirations 63 per minute; skin hot and dry, the heat of that peculiar acrid kind called “calor mordax;” thirst greatly aggravated. The *veratrum viride* was increased to five drops every three hours; spirits of turpentine to be given every six hours, in fifteen drop doses, in a little warm sweet milk to cover the taste, which excels any vehicle we ever tried. The enema of cold water to be continued every six hours, and the *viride* increased to eight drops. When we left, at 4 in the afternoon, there was slight moisture of the surface; the pulse was 130, more full and distinct; breathing a little less frequent and hurried. On the day following it was reduced to 95 beats per minute; on the following day it was reduced as low as 85, with a like improvement of all the symptoms. The remedies were continued, and she rapidly and perfectly convalesced. It did appear that Providence brought us safely through the most critical of all the cases we have met. It also appeared, that so soon as the foetus was thrown off, she was much less susceptible to the impression of the *veratrum viride*.

There are many points of interest in the above case, which are well worthy of particular notice. In the first place, it had been treated by two skillful physicians, with all the ordinary

remedies. On the ninth day, the stage of collapse or exhaustion set in so rapidly and to such an extent, as to render brandy, quinine and rubefacient frictions necessary, to keep up the actions of the heart and arteries as well as the external warmth. After the free use of the above, from Saturday till Tuesday, we find there was no relief, but rather a continuance and aggravation of the symptoms. On Tuesday there was a withdrawal of all remedial agents in use—was put on a few drops of the tincture of *veratrum viride*, at no time for the first 24 hours exceeding four drops. This was attended with relief from internal heat and burning, a general distribution of heat on the surface, and the pulse rendered slower, fuller, and more distinct, &c. The only change made which seemed to add to the good effects, were enemata of cold water, containing six drops of the tinct. of *veratrum viride*. In the meantime she aborts with a renewal and aggravation of all the symptoms; to meet which, there is added to the treatment 15 drops of *spts. turpentine*; the dose of *veratrum viride* increased, by mouth, to five drops, and by enemata to 8 drops. Again, the lessened susceptibility after the abortion, whereas, under ordinary circumstances, bleeding increases this susceptibility: true, the loss of blood was comparatively small, yet, taking into account the length of time she had been sick, it might be said to have been relatively large. These are facts and circumstances for reflection and investigation.

We had intended to report three severe and interesting cases treated in the same family, but the continual appearance of articles from other and abler pens, induces us to lay aside much that we intended for the press. We will simply state that, with the prompt and efficient assistance of the two physicians in attendance, we were able to bring all the cases to a happy termination. We will add, for the sake of brevity, that all the cases were treated previous to January, 1852, except a very few. Since the first of June, 1852, we have been called to see seven cases of typhoid fever, in consultation, all of which were cured. We had noted down eleven consecutive cases of this fever, eight of which we saw in consultation, one had been seen by another physician and two were seen by no other physician. We notice these on account of their great severity,

and more especially from the fact of a large majority of them being patients under treatment by other physicians. We are in possession of a large number of cases of typhoid and other diseases, which have been treated with unvarying success with the *veratrum viride*. Dr. B. treated 23 cases, without the loss of a single one—all in the same family.

Veratrum viride, green hellebore, American hellebore, is not our common Poke-root or *Phytolacca Decandra*, but is the poke weed, *veratrum viride*, and is entirely different in its appearance and properties. Again—it is called white hellebore, by the shakers, and those ordering the *veratrum viride* often get the white hellebore proper, or European, for it, by not being specific in the correction of the error in name. The properties and powers of *veratrum viride* are the following: 1st, acrid—This property is very limited and confined to the fauces. 2d. It is adanagic, deobstruent or alterative: this property it possesses in a marked and very high degree; not equalled by calomel or iodine in this particular, which will adapt it to the relief and cure of many diseases hitherto beyond the reach of any remedy. Of this class of diseases, those which we think will be much benefitted by it, are, cancer and consumption. 3d. It is actively and decidedly expectorant, so much so that we rarely add any other article. 4th. It is one of the most certain diaphoretics belonging to the *materia medica*: it often excites great coolness or coldness of the surface; in some cases the skin is rendered merely soft and moist; in other instances, the perspiration is free, and at other times it is most abundant; but, notwithstanding its profuseness, it does not reduce or exhaust the system, as many diaphoretics do when in excess, and therefore need not excite alarm nor be suspended on that account. 5th. It is nervine, not narcotic, under any circumstances; as since our first article, we have taken it more than twenty times to test its varied powers, and we have taken it in all quantities, from the production of free emesis down to the minimum dose. This property renders it of great value in the treatment of painful diseases and such as are accompanied with convulsions, morbid irritability and irritative mobility. For example—pneumonia, rheumatism, puerperal fever, convulsions generally, and palpitation of the heart, &c.

6th. It is one of the most certain and efficient emetics known, and is peculiarly adapted to meet that indication in hooping cough, asthma, croup, scarlet fever, and in all cases where there is much febrile and inflammatory action. It often excites severe nausea and frequent vomiting, which, taken in connection with great paleness, often alarms the patient and by-standers; but these effects, when in excess, are readily relieved by one or two full portions of morphine and tinct. of ginger, or of laudanum and brandy. One grand and leading feature is, that the exhaustion which follows it, is not excessive and permanent, but confined merely to the effort. Again, the matter, first ejected, is a large quantity of thick, slimy mucus, and soon after, the liver is called on to pour forth its own fluid in abundance. 7th. The seventh property is its most valuable and interesting, and for which it stands unparalleled and unequaled as a therapeutic agent. So much has already been written on what we call the sedative—arterial sedative—properties of the agent, or the power it possesses of controlling and regulating arterial action, that we shall not again run over the amount of evidence on this part of the subject. By virtue of this and other powers, the treatment of disease has been much simplified, and when the effects, recorded in the case of Mr. G.'s negro woman, shall have been fully considered, we may bid adieu to much of the supposed necessity for stimulants in the treatment of atonic or asthenic cases. We challenge the medical world to produce its equal, as a therapeutic agent, for certainty of effect, for extent of effect, or for peculiarity of effect, and the ease and safety with which it may be administered to small and great. In small portions, we have found nothing to equal it in exciting and promoting appetite.

The formula we use is the following:—

℞. Root of *veratrum viride*, dried, . . . oz. 8

Alcohol, of the shops, undiluted, . . . “ 16

Let it stand from ten days to two weeks. Medium dose for an adult male, eight drops, to be increased one or two drops every portion, until nausea or vomiting, or a reduction in the frequency of the pulse takes place; then reduce one-half in all cases. Females, and persons from 14 to 18 years of age, should commence with six drops, and increase as above. Children, from

one to two years of age, to commence with one drop ; from two to five years of age, two drops, and increase one drop. The usual interval with us is three hours between the portions. In ordinary cases of pneumonia, we usually continue it three days after the symptoms are subsided. In typhoid fever, and many other diseases, it requires to be continued much longer. For the satisfaction and information of the profession, we would state that it may be continued indefinitely, or any length of time, in moderate doses, or short of nausea, without the least inconvenience. The only objection that could be urged, is the increase of appetite, or desire for food. It is not cathartic—it is like all other remedial agents, subject to the same rules and regulations, making it out of the question for a person to lay down any but general directions for regulating the dose. We are better pleased with the method adopted for getting its first impression by Dr. Welburn, of Farmville, Alabama, than with our own. We allude to the short interval between the first three portions he administers: He gives “six drops, in ten minutes seven drops, in ten minutes more eight or ten drops ; and then suspends the dose till vomiting occurs,” which will be sure to take place in a large majority of cases. In the outset of many cases, we would recommend Dr. Welburn’s manner of using it. In a male, twenty-five drops is the largest quantity we have known to be required to excite emesis, and sixteen drops in the female when given in the manner and at the intervals we have directed. There need be no danger apprehended of its exciting inflammation of the stomach—we have given special attention to that particular. It is peculiar and at the same time interesting in its effects. The fact of its acting as a sedative on almost every other portion of the system, diminishing the vascular and muscular action and motion of every other part, and increasing that of the stomach. We have seen it produce emesis in very susceptible persons, and the contractions of the stomach were so rapid as to be almost continuous and uninterrupted ; but a strong alcoholic tincture of ginger and morphine would afford more prompt and immediate relief than any other articles that we have ever used. We have never seen a case that failed to be relieved by the above remedies in thirty minutes. The great advantage of the rem-

edy is that it does not exhaust longer than the effort to vomit is concerned. A great many remedies leave the patient in an exhausted and enfeebled condition, aside from the effort or immediate action—not so with the *veratrum viride*. Again, tartar emetic should never be given with it, in any form or manner. The only cases in which we have seen the tincture of *veratrum viride* purge, were when given in combination with tartar emetic, or with Coxe's hive syrup. In most of these cases it excited a violent cholera-morbus. We would not think of giving the tincture of *veratrum viride* where tartar emetic had been used, without preceding it with a full dose of morphine or laudanum at least one hour. We have known many fall out with the *veratrum viride* when it was not at fault. Again, venesection, when a large quantity of blood is drawn, increases materially its effects, whereas opium and morphine lessens or diminishes them. If a patient had been bled freely, preceded or followed by a liberal use of tartar emetic, and then followed up with medium portions of the tincture of *veratrum viride*, we should anticipate and prepare for drastic, if not hazardous effects.

The length of our article warns us of the necessity of concluding. We will, for the benefit of many who have written us, state the diseases in which we have used it with success, and leave the matter with the profession for further experiment and application from analogy. We do not hazard any thing of opinion or reputation, when we assert that it is a specific in pneumonia, in the qualified terms we have stated; we say the same of convulsions accompanied with high febrile excitement, also of palpitation of the heart. In typhoid fever it has more than answered our most sanguine anticipations; we assert the same in puerperal fever, scarlet fever, rheumatism and asthma. In the spring of 1851, we were called in consultation with Dr. Stewart, to Dr. G.'s child, who was well nigh run down with hooping cough, fever and diarrhœa. We advised the tincture of *veratrum viride*, which acted like a charm; since which time, Dr. S. has written us a letter highly extolling it as unparalleled in the treatment of hooping cough. We have seen no case of metastasis to the testicle in mumps, that was not relieved of pain and fever in twelve hours. It may be styled *the remedy* in croup, when there is great vascular derangement.

We have used it with great success in inflammation of the brain, also in typhoid dysentery. It is a valuable emmenagogue. In the inflamed breast, we give it with a confidence bordering on a certainty of success. In epileptic convulsions we have confidence of obtaining great relief from it. We look with confidence to being able to cure consumption, by a timely and judicious use of it. We trust even cancer will be robbed of its terrors. We are anxious to test its powers in yellow fever and in phlegmasia dolens, &c. In conclusion, we will state to the profession at large, that we have endeavored to give a faithful and unexaggerated account of a portion of the cases in which we have used it, with a statement of its powers and properties. We know that we have like passions with other men, and that we are liable to be mistaken, that we are liable to be carried beyond the bounds of truth and soberness, as well as others, in our great desire to advance and consummate as far as we may, the honor and perfection of our science. But we feel confident, that when all is cool and calm—when every property and power is put to the test of fair and proper trial—that every effect and power claimed by us as belonging to and possessed by *veratrum viride*, will be emphatically confirmed and established by the profession. We have not made any effort to distinguish between its primary or direct and its secondary or indirect effects. If we have succeeded in getting the profession awakened to its properties and enlisted in the investigation of its application and adaptation to the treatment of disease, we congratulate ourselves that we have achieved much with our feeble efforts, and that ere long we shall see embodied, by much abler pens, all the inestimable and unparalleled powers belonging to *veratrum viride*.

COLUMBUS, GA., March 23, 1852.

Dr. W. C. NORWOOD:

Dear Sir—I left home, for Macon, a few days after you. On my return, found Dr. Boswell in full blast with the *veratrum viride*: visited his cases with him—have used it in a few cases myself, and in every case it was *sure to reduce the pulse*. I am as *well pleased with it as I anticipated*, and as much so as *any one remedy I ever used*. I have no idea that you claim for it as much credit as it is entitled to. Four out of the six ounces have been used, and we will soon need more. I design extending its use in every case where there is too

much arterial action, until I have fully tested whether there be a difference in its effects in different diseases.

S. A. BILLING, M. D.

[We regret, that we mislaid a letter of later date from Dr. B., which was much more full and specific than the above, which was written in haste and in answer to other inquiries.]

FLAT SHOALS, GA., Feb. 10th, 1852.

Dr. NORWOOD:

Sir—I avail myself of this opportunity for communicating to you the result of my experience in the use of the preparation of *veratrum viride*. * * * But I assure you that the *v. viride*, as a curative, far exceeds any thing with which I am acquainted. I have given it in only two cases, and if it proves as successful in the future, I pronounce it a specific in the fullest acceptance of the term.

Case 1st. I was called to visit Mr. A. on 5th inst. Found him laboring under a deep-seated attack of pneumonia typhodes: he had great difficulty in breathing, intense headache, *irritable* stomach, skin hot and dry, pulse 165 to the minute. Commenced giving him the *v. viride* in five drop doses, until he had taken four portions; then increased it to ten drops. By the next day, at noon, his pulse was reduced to 85 per minute, skin moist and pleasant, cerebral disturbance removed. His recovery from this time forward was rapidly attained.

Case 2d. I was called to Mrs. J., whom I found laboring under palpitation of the heart; pulse 130; great anxiety manifested by the countenance, and, using her own language, a "sense of suffocation" experienced. Ordered the *v. viride* given in ten drop doses, every three hours. The second dose produced free emesis, and with it an entire abatement of all distressing symptoms.

These two cases were treated with nothing else, save the *v. viride*, in order to test its powers in controlling the circulation. I would not take \$20 for the remnant left, (one ounce,) placing the medicine out of my reach. I think, sir, you may safely stake your reputation, as a medical man, upon the virtues of your *bantling*. It is, indeed, the "philosopher's stone," and the "blessing of him that is ready to perish," will be bestowed upon you for the discovery.

Trusting that you may live long, to see the success of your medicine placed beyond the reach of envy or malice, I am, my dear sir, your ob't ser'vt,

J. J. C. BLACKBURN, M. D.

MILLEDGEVILLE, GA., Jan. 12, 1852.

Dr. NORWOOD:

Dear Sir, * * * I feel perfect freedom in assuring you, that I do not know of any article of medicine which manifests itself as a reliable remedial agent in any kind of specific action, on particular parts of the human system, with half the certainty as your preparation

of veratrum does, in controlling inordinate action of the heart, under the varied forms of febrile excitement. Calomel does not act with half the certainty, in emulging the liver—nor does aloes, in irritating the lower part of the rectum—nor does ergot of rye, in increasing parturient effort in labor, and, indeed, I might say, nor is tartrate of antimony more certain to produce emesis, nor is castor oil, nor croton oil, nor any other purgative, more certain to produce catharsis.

How far your preparation acts, as a remedial agent, beyond its unparalleled and unequalled control of the sanguiferous system, in the management of fevers of different types and at different stages—or what is its *modus operandi* in producing such effects as are distinctly evident to any observer of common sense, (physician or otherwise,) and in seeming to produce very beneficial collateral effects from its use in very dangerous cases, at critical times, *I am not prepared to say*. My testimony, as a medical man of some experience, may, however, be briefly stated to be decidedly favorable to the use of said article, believing, as I do, that no physician can use it without regarding it as a very efficient article, and such a one as in its specific control of the action of the heart, in feverish excitements, fully meets our heretofore earnest desideratum. *Digitalis* succeeds in one case out of twenty perhaps—this preparation, in nineteen cases out of twenty, more certainly.

So much, at present, in reference to my appreciation of the use of your preparation of *veratrum viride*. I know that I am sincere, and do not think that I am enthusiastic beyond a reliable matter of fact. *

* * *

JOHN F. MORLAND, M. D.

[The Doctor resides at Corinth, Ga. He was acting member of the legislature when the above was written.—W. C. N.]

OGLETHORPE, GA., 19th Feb., 1852.

My Dear Sir—Please pardon me for not giving you, earlier, the results of my experience in the use of the *veratrum viride*. So soon as I received the package you sent me, I made the saturated tincture of alcohol, and have used it with more certainty of success, in controlling arterial action, than any other known remedy. I can truly say, that I have never found any remedy that produced its specific effects so certainly in my hands—it has never failed. I have used it in scarlet fever, pneumonia, typhoid pneumonia, typhoid fever, inflammatory rheumatism, and in all cases where I wanted to lessen the frequency of the heart's action, and in no instance has it failed. In your published articles you have claimed much for it, but not more, nor as much as it really merits; for if there is any thing in nature entitled to confidence, to such a degree as to amount to a certainty, it is most undoubtedly the article. I hope the profession will universally adopt its use, and thereby secure for the science a triumph it so justly merits, in saving thousands from an untimely grave; and for you, I am certain the prayers of thousands will arise to a throne of grace, that you may be abundantly blessed in your labors.

Most respectfully, yours,

WILLIAM ELLIS, M. D.

NEW YORK, Sept. 22d, 1851.

At the request of Dr. Norwood, of S. Carolina, in order to test the effects of the internal administration of the *veratrum viride* upon the circulation, I selected four cases in my wards, and ordered the tincture of the root to be prescribed as follows :

Case 1st. Adult female : extensive fissure of the anus and rectum, spasmodic contraction of the sphincter ani, with excessive pain ; pulse 130. Dose, five drops every three hours. Pulse reduced to 68 in fifteen hours.

Case 2d. Adult male : morbus coxarius ; pulse 99. Dose, from five to eight drops every three hours. Pulse reduced in twelve hours to 50 beats.

Case 3d. Adult male : articular rheumatism ; pulse 120. Dose, from five to eight drops every three hours. Pulse reduced to 80 in fifteen hours.

Case 4th. Adult male : the effects of the operation by excision for large sarcolated hydroceles of the tunica vaginalis on both sides ; pulse 102. Dose, five drops every three hours. Reduction in ten hours to 60 beats.

I should have much confidence in the salutary action of the *veratrum viride* in cases of acceleration of the pulse in traumatic lesion of any of the vital organs, in patients of a robust constitution, or with sthenic diathesis.

J. M. CARNOCHAN, M. D.,

*Surgeon of the New York Emigrant's Hospital ;
Prof. of Surgery in the N. York Med. College.*

ARTICLE III.

Extirpation of a Cervical Tumor. By L. A. DUGAS, M.D., &c.

Elick, a negro (blacksmith), about 40 years of age, belonging to Mr. James McDowel, of Talbot county, Ga., was sent to me in November last, when I obtained the following history of his case :—About thirteen years ago he perceived a small tumor upon the right side of his neck, a little above the clavicle, which, although somewhat tender to the touch, remained without giving him any trouble until nine years ago, when he began to feel some uneasiness in the right arm, which would be increased upon pressing or handling the tumor, then apparently not larger than the end of his thumb. The pain of the limb gradually became more troublesome until four years ago, since when he has been more or less incapacitated for the duties of his trade, his sufferings being very much aggravated by working at the anvil. For the last six or eight months his entire limb has

been the seat of incessantly excruciating pain, depriving him of sleep, and utterly unfitting him for any kind of work. Several years ago his right hand and arm became very tremulous, and this state of things extended to the left arm, and subsequently to the whole muscular system. Several physicians were consulted, and pronounced the tumor to be aneurismal.

Upon examining the case I found it to be as follows:—A tumor exists in the triangular space above the clavicle, and between the sterno-cleido-mastoideus and trapezius of the right side. It appears to be beneath the platysma myoides, presents a surface about the size of a half-dollar coin, but evidently extends down to the brachial plexus, the subclavian artery and the clavicle. It is extremely sensitive to the touch, or rather, when touched, intense pain is induced in the brachial plexus and down the arm to the fingers. It is very slightly moveable, yields a sense of obscure fluctuation, and pulsates visibly as well as to the touch, synchronously with the arteries. The pulsation is arrested by compressing the subclavian artery nearer the heart, but the size of the tumor remains unchanged—nor can this be diminished by direct pressure upon the tumor. Auscultation reveals no aneurismal sound. General health pretty good, with the exception of the muscular tremor, which was much increased by the examination. He carries the limb in a sling, or holds it up with the other hand, when walking or standing; but when seated, draws the affected arm around the back of the chair or bed-post with the left hand, so as to effect as much pressure as possible and to keep it steady, by which means he says he is partially relieved of the dreadful pain. Can use his hand, but cannot elevate the arm—probably from protracted disuse added to the nervous affection. He is so nervous that he perhaps exaggerates his sufferings.

Being satisfied that the case was one of diseased lymphatic gland, which derived its pulsations from contact with the subclavian artery, and which irritated, by pressure, the subjacent brachial plexus, I proceeded to extirpate the tumor on the 27th November, in presence of the Medical Class and others.

This was effected in the usual manner. The tumor was found to be about the size of a large hen's egg, in contact with the subclavian artery, and resting upon the brachial plexus, to

which it was connected by strong areolar tissue. It proved to be a lymphatic gland, whose investing membrane was thick and resisting, and whose structure, broken up by chronic disease, presented a mass of shreds and small cavities filled with a caseous and grumous matter.* Subjected to microscopical examination by Dr. Juriah Harriss, no carcinomatous cells were detected.

The great impairment of the patient's nervous functions, his extraordinary timidity and reluctance to submit to what he thought would in all probability kill him, and the necessity of inducing, for such an operation, the most complete or comatose anæsthesia for a greater length of time than might be safe under the circumstances, induced me to omit the use of chloroform or ether. He bore the knife very badly, was extremely unruly, and thus protracted the completion of the operation much beyond what was necessary. A slight suppuration ensued, but the wound was perfectly healed on the 12th December, and he returned home on the 16th.

The patient felt no more pain in the right arm after the day of the operation, but said there was still some numbness and weakness in the limb. When he left here he had so far recovered the use of his arm as to be able to cut wood with an axe, or with a saw, the left hand, however, being much the stronger of the two. He could not yet elevate the whole limb—but will doubtless regain its full powers by use.

The rare occurrence of such serious effects from so small a tumor in the cervical region, together with the interest attached to the case in consequence of a difference of opinion with regard to the diagnosis, have induced the writer to put it on record.

Augusta, Dec. 1852.

* An account of diseased Cervical Glands was published by the writer in this Journal, in 1846, vol. 2, new series, p. 513.

PART II.

Eclectic Department.

What is the best time to amputate?

[We extract from one of Mr. Guthrie's lectures, published in the London Lancet, the following aphorisms in reference to the time at which amputations should be performed after wounds. The eminent abilities and extensive experience of Mr. G. entitle his conclusions to great respect.—EDITOR.]

40. When the wound of an extremity is of so serious a nature as to preclude all hope of saving it by scientific treatment, that limb should be amputated as soon as possible.

41. An amputation of the upper extremity may almost always be done from the shoulder-joint downwards, without much risk to life, and when necessary, the sooner it is done the better.

42. An amputation of any part of the lower extremity below the knee downwards may be done forthwith, with nearly an equal chance of freedom from any immediate danger, as of the upper extremity at or near the shoulder-joint.

43. It is otherwise with amputations above the middle of the thigh, and up to the hip-joint. They are always attended by considerable danger.

44. There can be no doubt that if the knife of the surgeon could in all cases follow the ball of the enemy, or the wheel of a railway carriage, and make a clean good stump instead of leaving a contused and ragged wound, it would be greatly to the advantage of the sufferer; but as this cannot be, and an approach to it even can rarely take place, the question naturally recurs,—At what distance of time after the receipt of the injury or accident can the operation be performed most advantageously for the patient?

45. In order to answer this question distinctly, it should be considered with reference to two distinct states of injury:—

1st. When injuries require amputation of the arm below the shoulder-joint, or of the leg below the knee, these operations may be done at any time from the moment of infliction until the expiration of twelve or twenty-four hours, without any detriment being sustained by the sufferer with regard to his recovery; although every one, under such circumstances, must be desirous to have the operation over. The surgeon having several equally serious cases of injury of the head or trunk brought to him at the same time as two requiring amputation, may defer them more safely perhaps than the assistance he is

also called upon to give to the other cases, the postponement of which may be attended with greater danger.

2nd. This state embraces those great injuries, in which the shoulder is carried away with some injury to the trunk, or the thigh is torn off at or above its middle, rendering an amputation of the upper third or at the hip-joint necessary; and it is this or nearly this state which alone implies a doubt as to the propriety of immediate amputation, and demands further investigation. It is the state to which your attention is earnestly drawn for future observation.

46. It has been implied, if not actually maintained, that a man could have his thigh carried away by a cannon-shot without being fully aware of it, or, if aware of it, that it did not cause much alarm—in fact, that it did not materially signify as to his apprehension, whether the ball took off his limb or the tail of his coat, or only grazed his breeches. An instance of this kind has not fallen under my observation.

47. A surgeon on a field of battle can rarely have a patient brought to him, requiring amputation, under less time than from a quarter to half an hour; a surgeon in a ship may see his patient in less than five minutes after the receipt of the injury; and to the surgeons of the navy we must hereafter defer for their testimony as to the absence or presence of any constitutional alarm and shock; and if they occur, to what degree do they follow immediately after the receipt of such injury. The question must not be encumbered and mystified by a reference to all sorts of amputations after all sorts of injuries, but to the one especial injury—viz., that of the *upper half of the thigh*.

48. My experience, which may be erroneous, like every thing human, has taught me, that when a thigh is torn or nearly torn off, by a cannon-shot, there is always more or less loss of blood, suddenly discharged, and which soon ceases in death, or in a state approaching to syncope. When the great artery is torn, this fainting saves life, for an artery of the magnitude of the common femoral does not close its canal by retracting and contracting, as a smaller vessel does; it can only diminish it; and the formation of an external coagulum is necessary to preserve life, which the shock, alarm, and fainting, by taking off the force of the circulation, aid in forming; and without which the patient would bleed to death. An amputation, in this state of extreme depression, might destroy life.

49. If the cannon-shot, or other instrument capable of crushing the upper part of a thigh, should not divide the principal artery, and the sufferer should not bleed, it is possible he may be in the state alluded to, in which the patient, for he may not

be called sufferer, is said to be just as composed as if he had only lost a portion of his breeches. Nevertheless few have seen a man lose even a piece of his breeches by a cannon-shot without perceiving that he was indisputably frightened.

50. Whilst some persons, under the loss of a limb high up, are reduced to a state of syncope, or nearly approaching to it, which renders them almost, or even entirely speechless; others suffer extreme pain, and earnestly entreat assistance, under which circumstances, amputation should be performed forthwith. In the former, the administration of stimulants may render the operation less immediately dangerous.

51. Chloroform, or other similar remedies, may produce an effect in such cases yet unknown. Its careful administration may not destroy the ebbing powers of life, and may render an amputation practicable, which could not otherwise be performed without the greatest danger. It may be otherwise; the point, however, is to be ascertained, although in all cases of great suffering its use should be unhesitatingly adopted.

52. When the sufferer is brought to the surgeon at the end of half an hour, having lost a limb below the thigh or shoulder, by a cannon-shot, he will often be found in a state of such great depression as to be likely to be destroyed by the infliction of a serious and painful operation like amputation. This has occurred to me so often as to induce me to recommend delay for five, six, or even eight hours, if the unfortunate person did not suffer much, and appeared likely to be revived by the proper use of stimulants.

53. This recommendation originated from the fact, that as one seriously wounded man has as much claim as another to the attention of the surgeon, all could not be attended to at the same time; and the success following the deferred cases of amputation was as great, if not greater, than in those on which the operation was more immediately performed.

54. The advantageous results of *primary* amputations, or those done within the first twenty-four, or at most forty-eight hours, over *secondary* amputations, or those done at the end of several days, or three or four weeks, has been so firmly and fully established as to admit no longer of dispute.

55. When an amputation is deferred to the secondary period, a joint is often lost. A leg which might have been cut off below the knee in the first instance, is frequently obliged to be removed above the knee when done in the second.

56. In the secondary period after great injuries, the areolar and muscular textures near the part injured are often unhealthy, the bones are in many instances inflamed internally, and their periosteal membranes deposit on the surrounding parts so much

new ossific matter as frequently to envelope in a few days the ligatures on the vessels, and render them immovable, necrosis of the extremity of the bone following as a necessary consequence, protracting the cure for months.

57. Sloughing of the stump, accompanied by inflammation of the vein or veins leading to the cava, frequently take place. This state of stump is often followed by purulent deposits in and upon the different viscera, and principally in the cavities of the chest. Where febrile diseases are endemic, they often prevail; the constitutional irritation is great; the stumps do not unite, or open out is apparently united and slough, and frequently after a few days implicate the veins.—[*London Lancet*.

Preparations of Arsenic.

Arsenic, the therapeutic use of which appears to have been from the earliest ages diffused generally throughout India and China, has been, since the sixteenth century, much employed by the physicians of Germany, England, France and Italy.

The therapeutic history of this heroic remedy is one of the most interesting on record. Thousands of facts had long attested the efficacy of arsenical preparations in the treatment of intermittent fevers, when experience proved that they might be also advantageously employed in diseases of the skin; it was, moreover, known that arsenic had been extolled in India as a remedy in the most serious affections, and particularly in elephantiasis.

In 1817, Biett, repeating at the Hospital St. Louis the experiments he had just witnessed in the London Hospitals, introduced into France the employment of arsenic in the treatment of cutaneous affections, and from the first laid down rules for its use, which it has not since been found necessary to modify.

The preparations of arsenic, notwithstanding their incontestible value in the treatment of many diseases of the skin, are not applicable to all. They are principally useful in the essentially chronic affections; in the dry forms, such as psoriasis and lepra; and in eruptions of another class, which, after having resisted rational treatment, and having become, as it were, established, show an obstinacy which seems dependent on local, idiopathic conditions of the skin, as in chronic-lichen, and especially in certain forms of eczema.

They afford us the most effectual remedies in the treatment of the elephantiasis of the Greeks.

Good effects have also been obtained from the employment

of arsenical preparations in the treatment of venereal diseases, and especially in the tuberculous and scaly forms of syphilitic eruptions.

I have already mentioned that the efficacy of certain decoctions (de Felz. d'Arnault, &c.) in the composition of which more or less antimony is employed, is generally attributed to the presence of arsenic.

IODIDE OF ARSENIC—*Thompson's Pills*.—Iodide of arsenic, three-fourths of a grain; extract of hemlock, one scruple: make into ten pills—one to be taken every eight hours. This very active formula has been principally employed in lepra.

Mr. Donovan has recommended the double iodide of arsenic and mercury, a compound of equal parts of iodide of arsenic and bin-iodide of mercury, in the treatment of lepra, psoriasis, lupus, and of syphilitic affections. *Solution of double iodide of arsenic and mercury modified by Soubeiron*.—Iodide of arsenic, iodide of mercury, of each, one part; distilled water, ninety-eight parts. This solution contains an hundredth part of each iodide.

ARSENIOUS ACID.—*Liquid Arsenious Acid*.—A solution of three-quarters of a grain of arsenious acid in eight ounces of distilled water is employed under this name in the German hospitals. The dose is one table-spoonful, gradually increased to six, and is to be taken in the morning on an empty stomach (Foy.) *Dr. Gilbert's formula*: Arsenious acid, three-quarters of a grain; distilled water, sixteen ounces; dissolve by the aid of heat; divide into five phials, the contents of each of which are to be taken in the morning of either one or two days, according to circumstances. This formula is generally preferred by M. Gilbert in the treatment of diseases of the skin, and especially of psoriasis. *Asiatic Pills*.—Arsenious acid, three-quarters of a grain; black pepper, ten grains; gum Arabic, one-sixth of a grain; water, as much as sufficient to make twelve pills—one to be taken daily. This preparation is very active; it is the one I usually prefer, varying the dose from one to two, but more frequently diminishing it in the following manner:—Take the mass of Asiatic pills, eight grains; extract of taraxicum, twenty-three grains; mix and divide into twenty-three pills, of which one or two are to be given daily.

ARSENITE.—But one arsenite is employed in therapeutics, namely, the arsenite of potash, forming the basis of Fowler's solution, which is a very energetic medicine, and requires to be used with caution. Biett, who frequently prescribed it, recommends that the daily dose should at first be only two or three drops, and never exceed twelve, administered in two equal portions.

ARSENATES.—*Pearson's Arsenical Solution*.—Arseniate of soda, three-quarters of a grain; distilled water, one fluid ounce; dissolve and filter (Cordex.) Dose: a scruple, gradually increased to half a drachm. This preparation is milder and more manageable than Fowler's. I prefer it with women and children, in cutaneous diseases. *Biett's Arsenical Liquor*.—Arseniate of Ammonia, three grains; distilled water, four fluid ounces. The uses and dose of this are the same as those of the preceding preparation. *Pills of Arseniate of Soda*.—Arseniate of soda, three-quarters of a grain; extract of taraxicum, half a drachm; divide into thirty pills—one or two to be taken for a dose. Use, the same as of the preceding preparation. I often employ this formule with advantage. *Pills of Arseniate of Iron* (Biett).—Arseniate of iron, two and a half grains; extract of hops, one drachm; syrup orange flower, a sufficient quantity; make into forty-eight pills. Biett employed these pills particularly in scaly affections and in lupus—the dose is one daily.—[*Dublin Quarterly Journal*.]

Remedy for Intussusception of the Bowels. By A. S. BALDWIN, of Jacksonville, Florida. (Extract from a letter to the Editor.)

Having recently seen in the medical periodicals, several reports of *post-mortem* examinations, in cases of intussusception of the bowels, I am induced to send you an account of a rather simple, but what appeared to me a very effectual remedy for this complaint, in a case which came under my care about four years ago, when I had despaired of affording relief by the ordinary remedies. If it shall seem to you to have sufficient merit, it may, perhaps, by giving it publicity in your journal, induce other members of the profession to give it a trial. During the last fifteen years, several cases of this complaint have come under my observation, most of which terminated fatally. The various remedies recommended were applied; among them the long elastic tube, for the purpose of throwing fluids high up into the bowels, with the hope of distending them so that the vaginated folds of the intestine might be drawn out. In some instances, at least, its use appeared to be productive of mischief, from passing through the constricted part of the intestines, so that the fluid injected was lodged in the pouch or sack existing above the point of obstruction; to ineffectually increase the distension already existing there; for, when the tube was withdrawn, no fluid was returned, and the distension was increased without having the effect to remove the difficulty. Injections administered by the common syringe were returned immedi-

ately, even while giving them, and had no effect to distend the lower bowel, so as to aid in overcoming the obstruction, and the conclusion arrived at was, that adhesions existed between the folds of the invaginated parts of the bowel, and the obstruction was irremediable.

Circumstances which have occurred to me, dispose me to think that these adhesions do not take place so early, or so frequently, as many are disposed to believe. About three years ago, a case of this kind occurred in my practice, and for several days all the appliances which had been recommended had been used to overcome the obstruction, but without avail. There was a circumscribed spot, to the left of the umbilicus, and a little below it, which was painful; there was considerable distension of the abdomen, with a sensation of soreness across, but which did not amount to pain (being what is called in this country "*misery*"). Twice in this case the long tube was used, and injections thrown high up into the bowels, which, however, did not return upon the withdrawal of the tube, but added to the swelling previously existing above the point of obstruction. I was apprehensive that adhesion had by this time taken place, and as I despaired of relief by the ordinary methods, and the patient had arranged his temporal affairs and given himself up to die, I determined to distend the lower bowels, to their utmost capacity, by the injection of warm water.

An ivory tube, having a shield around it, was introduced and passed up until the shield was pressed up against the sphincter ani, a cloth was wrapped around this and pressed up firmly; the tube was now connected by an elastic tube with the pump, which was placed in a wash-basin of warm water, which was slowly injected into the bowels, pressure being kept up to prevent its return. Another basin of water was brought, half of which was thrown up. The abdomen was, of course, much distended by this quantity of fluid, and considerable rumbling and commotion of the bowels were produced, the pain at the point of obstruction was, for a moment, acute, causing the patient to cry out. The pressure and tube were removed, and we found he had the power to retain the injection until he could be helped to the chair, when about five quarts of the injection was passed; becoming faint, he was laid upon the bed, and brandy and water administered; he soon rallied, and passed as much more, colored by fecal matter; soon after, a copious and regular, but very offensive stool was had, in which the oil, taken several days before, could be distinguished. After this, he had no farther difficulty, except debility, and a sensation of soreness at the point of obstruction, which lasted for a few days, when he returned to his work, that of a carriage-maker,

and up to the present time has had no return of the complaint. Since that time I have not had so severe a case of this complaint, but in every case which shows a disposition to be obstinate, I resort to this mode of injection, with uniform and immediate success. Perhaps some of these would have been as obstinate as the one above detailed, if the former mode of treatment had been pursued; but I am fully impressed with the belief that, had this remedy been used with those cases which had proved fatal, some of them, at least, might have been saved. The case in which I first tried it was an unpromising one, on account of the long time which had elapsed since the attack before the remedy was used, sufficient for adhesive inflammation to have agglutinated together the folds of intestine involved in the intussusception.—[*Am. Journ. Med. Sc.*

External use of the Nitrate of Lead.

Dr. Ogier Ward was induced to make use of this preparation in cases accompanied by fetid discharges, observing its advantages as a disinfectant under other circumstances.

The first case in which he used it with this intention was that of a lady, whose lochia were so offensive as to scent the whole house, and nauseate even the nurse. It was used as an injection; and the third application effected the complete removal of the fetor.

He has also used it with success as a lotion for sore legs when in a sloughy and indolent condition, and finds that it soon restores them to a healthy state, inducing a proper secretion of pus, with firm granulations on the surface of the ulcer.

He has not used the nitrate in acute gonorrhœa, but states that it acts admirably as an astringent in gleet discharges, as well as in those of cancer uteri, whether sanious or purulent. In short, as a lotion it is as extensively useful as the diacetate of lead, while it is superior to that preparation by its disinfecting property.

In chronic cutaneous diseases he has seen the most remarkable instance of its efficacy in a case of eruption, of a kind of rupia or impetigo of five years' standing. The complaint broke out on the vertex of a woman, æt. 50, and, leaving its original site, it has gradually crept down over the forehead, nose and cheeks, to the level of the mouth. The primary form of the eruption consisted of inflamed, flattened pustules, slightly elevated above the surrounding skin, which, discharging their contents, formed thick, rough, yellowish crusts, or scabs, fissured in all directions, like those of crusta lactea, which, firmly adhering and growing from their base, like rupia, for a longer

or shorter time, fell off at last, leaving cicatrices of various shapes, exactly like the seams and pits of smallpox. The patient came under Dr. Ward's care some months ago, when he tried many remedies, both internal and external, in vain, quinine being the only one that produced any good effect, and this not permanently. As the skin around the sores and where they had healed seemed in a state of hypertrophy, the papillæ projecting in many places as in elephantiasis, it occurred to him that the best way to check the progress of the disease would be to apply some penetrating astringent to the surface; and with this view he ordered a lotion of the nitrate of lead, with quinine, internally. In the course of a few days, the eruption ceased to make any progress, the crusts began to fall off, and the skin to lose its redness and swelling; and in a fortnight every sore was healed, though the face and forehead remained still seamed with scars. There has been no fresh breaking out for some weeks, though, as the nose is still red and swelled, he has ordered the continuance of the lotion.

The formula he generally uses is the following:—*R.* Plumbi carb. scr. j; Acid. nitr. dil. q.s. ad solvendum; Aquæ dist. ℥j. *M.* Fiat lotio bis terve indies assidue utenda.—[*Prov. Med. and Surg. Journal.*

On the use of Manganese as an adjuvant to Iron. By M. PETREQUIN.

M. Petrequin quotes various authors to prove that manganese is a normal constituent of the animal and vegetable tissues; and believes that wherever iron is present in appreciable quantity, manganese co-exists with it. Hence iron alone will not always succeed in blood-diseases. M. Petrequin has observed many cases of chlorosis, which have resisted iron as obstinately as anæmia connected with cancer or organic degeneration. Other cases again, after deriving a certain amount of benefit from iron, remain stationary. Others again appear cured by iron, but the cure is not permanent. The remedy required in this case, M. Petrequin finds to be manganese. He does not give it or iron alone, but combines them.

It is especially in *diseases of the blood* that ferro-manganic medicines are useful. They have a special action on the vascular apparatus, on the formation of the blood, and on the circulating fluid itself. They do not act as tonics or astringents, but as regenerators of the blood. They have succeeded admirably in anæmia following hæmorrhage, operations, polypi, metrorrhagia, etc.; also, in chlorosis attending puberty, which

is a more common disease than is generally supposed, and occurs even in males. M. Petrequin has also frequently found the combinations of iron of benefit in the diseases of women at the critical period. He has often seen in these subjects, *metrorrhagia*, accompanied with an aspect of the surface which would lead to the suspicion of organic uterine disease: the hæmorrhage, however, was but a complication, and the patients, apparently in a hopeless state, have recovered under the use of ferro-manganic preparations, conjoined with tonics and ergotine.

In *amenorrhœa* and *dysmenorrhœa*, the patients often imagine that they require to be bled; but care must generally be taken not to comply with this request. M. Petrequin has more than once seen cases of *amenorrhœa* with severe chlorosis, in which it has not been desirable to hasten the appearance of the catamenia—the consequent loss of blood aggravating the disease. The general state of health must here be attended to. Œdema of the lower limbs sometimes occurs in these cases; but it is a less severe complication than when it attends *metrorrhagia*. It often disappears, as the patient recovers, under the use of iron and manganese.

These medicines are no less efficacious in the treatment of *anæmia* resulting from prolonged intermittent fevers, prolonged suppuration, strumous, syphilitic, or cancerous affections, phthisis, etc. Pills and the syrup of the iodide of manganese and iron are preferable in these cases.

In all these cases, the ferro-manganic preparations do not merely act on the stomach and nervous system, but they are absorbed, and assist in the formation of hæmatosine and new blood-globules, so as to restore the blood to its normal condition. Their effect in this way is greater than that of iron alone.

In the *functional affections of the heart* connected with chlorosis and *anæmia*, and which must not be mistaken for organic disease, a combination of iron and manganese with *digitalis* and other moderators of the heart's action is advantageous. The same remark applies to the *functional disorders of the lungs*, attending the same constitutional states.

Disordered states of the nervous system are intimately connected with those of the blood. He, as well as M. Gubian, has observed that iron is here better tolerated when combined with manganese. He has also seen benefit from the use of iron with manganese in many cases of *dyspepsia*, *gastralgia*, and *gastro-enteralgia*. Nervous affections of the digestive organs are often the result of chlorosis; and, where stomachics and cinchona have failed, iron has often been found (especially the carbonate, by some English physicians) to be of service. Gas-

trodynia complicating chlorosis has often yielded to the use of ferro-manganiferous water, and to pills of carbonate of iron and manganese.

In *nervous affections connected with exhaustion* from venereal excesses, onanism, rapid growth, &c., as well as in leucorrhœa, diabetis, &c., M. Petrequin has a high opinion of these medicines. He is continuing his researches on their action in certain cases of sterility from asthenia, and in some hyposthenic affections of the scalp, such as early baldness, alopecia, &c.

M. Petrequin has confined his observations to a limited number of the ferro-manganic preparations; and has made many observations before publishing the formulæ which he finds most useful. Having found, even at an early period, that the medicines were liable to adulteration, he has availed himself of the assistance of competent pharmacutists. Since the publication of his first memoir, in 1849, these medicines have been extensively used in the South of France and in foreign countries.

The formulæ are few, and correspond to the preparations of iron generally used in France. They are: 1. *Pills* of carbonate of iron and manganese, or iodide; 2. *Lozenges* of lactate of iron and manganese; 3. *Syrups* of lactate or of iodide of iron and manganese; 4. *Ferro-manganic chocolate*; 5. *Effervescing solution* of iron and manganese.

It has been said that manganese not only preserves water, but purifies that which has undergone a change (Martin Lauzer). Ferro-manganic waters (of which there are many in France and other parts of the continent) can be preserved and carried to a distance;—which cannot generally be done with simple ferruginous waters.

M. Petrequin commences by giving the powder of iron and manganese, with some vinous drink; he then administers two pills daily, one before breakfast and one before dinner, replacing them soon by lozenges. The syrups and chocolate complete the treatment. He gives the medicines at meal-time. The syrup he gives before breakfast, in doses of a tea-spoonful; and he finds it useful to administer directly after it some infusion of centaury, or of camomile flowers and orange.

Large doses are unnecessary and useless; for they are liable to produce irritation of the stomach and exhaustion of the nervous system; and the reparation of the blood is slow and progressive, and cannot, even were it desirable, be effected rapidly. Besides, the iron and manganese are not absorbed in any greater quantity, if large doses are given.

Preparations of Manganese and Iron.—M. Burin-Dubuisson, of Lyons, who prepared most of the ferro-manganic combina-

tions used by M. Petrequin, has published an interesting *brochure*, in which he gives the necessary details relating to the subject. The following formulæ are extracted from it.

Powder for Effervescing Solution of Manganese and Iron.—Take of coarsely powdered bicarbonate of soda, 20 parts; tartaric acid, 25 parts; powdered sugar, 53 parts; finely powdered sulphate of iron, $1\frac{1}{2}$ parts; finely powdered sulphate of manganese, $\frac{3}{4}$ parts: mix carefully and keep in well stopped bottles. A tea-spoonful is mixed with each glass of wine and water drunk during meal-time.

Pills of Carbonate of Iron and Manganese.—Take of pure crystallized sulphate of iron, 75 parts; pure crystallized sulphate of manganese, 25 parts; crystallized carbonate of soda, 120 parts; honey, 60 parts; water, a sufficient quantity. Pills of 20 centigrammes (3 grains) are made; they keep easily, without becoming oxidized, in well-closed vessels. From two to four are given daily.

Ferro-manganic Chocolate.—One part of carbonate of iron and manganese is first mixed with four of sugar, and divided into large lozenges; of these, 100 parts (grammes) are mixed with 500 of chocolate paste, in the preparation of which 100 parts of sugar have been left out. This will make 800 lozenges, each of which contains about three centigrammes (nearly half a grain) of carbonate of iron and manganese. The chocolate decomposes the hydrated carbonate of manganese and iron of the saccharate into hydrated sesquioxide of iron and manganese; there is no metallic taste.

Syrup of Lactate of Iron and Manganese.—Take of lactate of iron and manganese, 4 parts; powdered sugar, 16 parts; rub together, and add of distilled water, 200 parts; dissolve rapidly, and pour into a matrass over a water bath, containing 384 parts of broken sugar: filter the solution. This syrup contains about 15 parts of lactate of iron and 5 of lactate of manganese in 3,000 parts. One or two spoonfuls are taken daily.

Lozenges of Lactate of Iron and Manganese are made by adding 20 parts of the lactate to 400 of fine sugar, with a sufficient quantity of water. The mass will make 840 lozenges; of which six or eight are taken daily.

Syrup of Iodide of Iron and Manganese.—M. Burin Dubuisson forms a solution of iodide of iron and manganese, in the proportion of one part by weight to two of water: the proportion of the salts is about three of iodide of iron to one of the iodide of manganese. Six parts of these are mixed with 294 of simple syrup; of this, M. Petrequin gives one or two tea-spoonfuls daily.

Pills of Iodide of Iron and Manganese.—Take of the

official solution prepared by M. Burin Dubuisson, 15 parts (grammes); honey, 5 parts; some absorbent powder, $9\frac{1}{2}$ parts. Divide into 100 pills. The honey and the solution are first mixed, and evaporated at first rapidly, then more slowly, to ten parts. Then add the powder, and divide the mass into four parts, which must be rolled in powder of iron reduced by hydrogen; each of these must then be divided on an iron plate into 25 pills, and again rolled in the iron powder. Finally, they are covered with a layer of tolu, according to M. Blanchard's process.

All these preparations must be made very carefully. M. Burin Dubuisson has ascertained that the commercial salts of manganese frequently contain copper, and even arsenic; he hence insists on the necessity of calcining the sulphate of manganese, twice, or more frequently, at a dark red heat, and of carefully testing the solution.—[*London Journ. of Med.*

On the internal administration of Chloroform in Delirium Tremens. By GEORGE E. FENWICK, M. D., Lecturer on Materia Medica, St. Lawrence School of Medicine, Physician to the Montreal Dispensary.

J. S., aged about 40, labouring under an attack of Delirium Tremens, brought on by suddenly stopping all stimulants after having led an irregular life for months, came under my care on the 16th September, 1852. *Present symptoms.*—Great anxiety expressed in the countenance, fear of impending danger frequent sighing, general tremor, delirium when left alone, which was of the quiet muttering kind, as if holding conversation with some imaginary person, pulse 110, weak, tongue covered with a whitish fur, bowels had been opened slightly that morning, appetite capricious, had not slept since the Saturday previous. It was 9 o'clock at night when I first saw my patient; I ordered a full opiate, and as he had formerly been in the habit of chewing opium, I prescribed it in six grain doses, to be repeated every second hour until he slept; two doses only were administered, the effect of which was to deprive him of consciousness, but he did not sleep, he raved and walked about his room the whole night.

The following day, Friday, he was worse, tremor increased, constant muttering, though when spoken to he conversed rationally on whatever subject was stated. He stated there were too little devils playing the drum on his head, and that they kept up such a noise he could not sleep. The eye exhibited greater wildness and less fear than last night, the pulse was the same 110, but fuller and more bounding, bowels not open.

I prescribed two blue pills rolled in Croton oil, to be taken immediately and at night the following draught:—

R. Spt. Æth. Sulph. ʒii.

Chloroform, ʒi. M.

This draught to be repeated every second hour until sleep was induced. Owing to a mistake the patient did not take the pills till 8 o'clock, P. M., and half an hour afterwards the draught was administered; the pills operated rather briskly, both up and down, an hour after they were taken, and the draught was not repeated.

Saturday, 18th. Found my patient somewhat better, although he had had but one draught of the chloroform, still, through the night he had dozed once or twice for a few minutes at a time. The symptoms not being urgent, I ordered porter to be taken during the day and nourishment. Visited my patient at 9 o'clock, P. M., determining to administer chloroform myself and watch its effects.

I gave three teaspoonsful of a mixture composed of chloroform and spirits of sulphuric ether, in the same proportions as above; in a few minutes he complained of drowsiness; he closed his eyes and became perfectly tranquil, the respirations became deeper and slower, the pulse fell from 96 to 62; to keep up the action I held the bottle to his nose for a few minutes; I watched him for half an hour, during which time he appeared to be in a natural sleep. This state lasted for about three hours. At leaving, I directed the draught to be repeated in two hours if necessary, but my instructions were not attended to.

The day following, Sunday, he appeared much refreshed, less tremor, pulse 72, had eaten a hearty breakfast. I did not repeat the chloroform through the day; that afternoon he slept for an hour and a-half; at night I again visited my patient, and again administered the chloroform and ether as before; a single dose threw him into a profound sleep, from which he did not awake till six o'clock the following morning. From this time he gradually recovered, slept naturally and well without the use of chloroform; the only unpleasant symptom noticed was headache, which came on for two or three mornings afterwards, and lasted for some hours; this appeared to be relieved by porter.

The second case which fell under my observation was one of quite a different character:—

Mr. S., labouring under an attack of delirium tremens, brought on by a debauch, came under my care 2nd October, 1852. When I saw him there was considerable anxiety, great tremor, the eye wild and staring, features bloated and swollen, pulse 120, full and bounding, tongue furred, bowels constipated

had not slept for two nights ; I administered 2 drops of Croton oil immediately, and prescribed the following mixture :—

R. Tr. Opii 3 ii.

Ant. Pot. Tart. gr. iv.

Aquæ ʒ iv. M.

Dose, a tablespoonfull to be taken every three hours. The Croton oil operated five or six times, and after the first dose the antimony was borne by the stomach.

This treatment was continued up to the evening of the 4th when, as there was no improvement nor tendency to sleep, I determined to employ chloroform, and accordingly prescribed the following :—

R. Spt. Æth. Sulph.

Chloroform, aa ʒ ½ M.

Dose, a desert spoonful to be taken every two hours until sleep is induced. After the second dose my patient fell into a quiet slumber which lasted six hours. The following day, the 5th, he was much better, countenance improved, eye less starting, much less tremor, pulse 88, bowels had been moved that morning. I ordered the chloroform to be repeated at night. After the first dose he slept, and did not awaken till the following morning, when he felt much refreshed, and quite himself ; he got up, dressed and took a short walk ; being fatigued, on his return he laid down again and fell into a sound sleep which lasted three hours. I ordered the chloroform to be repeated if necessary at night ; however he slept all night without it ; from this date he recovered rapidly.

Chloroform has frequently been employed in the form of inhalation, with advantage, by Dr. Todd and others. In the American Journal of Medical Science for January, 1852, Dr. Pratt of Baltimore, published two cases of delirium tremens, in which the internal administration of Chloroform was attended with most marked success. Since preparing this paper I have read another case of delirium tremens published by Mr. Butcher in the Dublin Medical Press, in which the internal use of chloroform was attended with like success. Mr. Butcher draws attention to the lowering of the pulse when the perfect effect of the medicine was produced. This was most marked in the first of the above cases, the pulse fell from 96 to 62, it never rose above 76 while the patient was under treatment. In the second case, I had not an opportunity of observing the immediate effect of the medicine on the heart's action, but the pulse fell from 112, which was the number of pulsations previous to the exhibition of the chloroform to 88, which was its standard the following morning.

The foregoing cases, though by no means severe or alarming

in their character, yet serve as further evidence of the successful employment of chloroform internally in the above class of diseases. In the first case opium had a decidedly injurious effect; all the symptoms were aggravated; at first I felt at a loss how to act: having read Dr. Pratt's cases, I determined to adopt the same line of treatment. Another point I would draw attention to: in the one case, the disease was brought on by want of an accustomed stimulus—in the other, the attack followed excess of the stimulus—in both the symptoms were peculiar, and in both the result of the treatment satisfactory.

[*Canada Med. Journal.*

[The use of quinine in Typhoid fever has many advocates in the United States, but there are also many who regard it as not only inefficient but positively injurious. Hence the suggestion has been made, that when it has proved beneficial the cases were not genuine typhoid fever. We therefore lay before our readers the following article, as the cases referred to occurred in a country in which malarial fevers are as rare as those termed typhoid are common.]

Remarks on the Treatment of Typhoid Fever by Disulphate of Quina. By J. W. HAYWARD, Esq., Liverpool.

In a practice, private and parochial, of which I have had charge for some time, at the south end of this town, I made particular observations on eighty successive cases of fever of the typhoid type; and I found the first symptom, in twenty of them, was diarrhœa; in twelve, diarrhœa and vomiting; in seven, vomiting alone; the rest began with pain in the head. All had pain in the head afterwards; sixty-six describing it as "lightness," fourteen as "heaviness." All had tenderness of abdomen—nine to a great extent. Seventy-one complained of soreness of the flesh over the whole body—some to such an extent that their impression was that they had rheumatism. Seventeen had considerable inflammation of the sub-maxillary glands. Seventy-three had delirium, twenty-one of which were very severe. In all, the tongue became very dry, brown, hard, and cracked; the first crack was generally a deep, longitudinal one down the centre of the tongue, (even whilst it was clammy and velvety in appearance, and of a milk-and-water colour,) extending from the base to nearly the apex; then many transverse and oblique ones. In twenty-four, the skin became rough and brown, with petechiæ observable. In all, the thirst was intense; and the other symptoms of fever were not

less evident; therefore suffice it to say they were well-marked cases of fever of the typhoid character.

In twenty-seven the treatment was commenced in the "first stage;" in fifty-three in the "second stage." Three were fatal. All the rest recovered more or less quickly.

The principal treatment in all except one was the use of disulphate of quina,—so much recommended by Dr. Dundas, of this town,—of course modified according to the predominating symptoms. Thus, if I found the pulse quick, weak and thready; the tongue cracked, brown, and dry, (rough or smooth;) great thirst and delirium; no appetite; tenderness of abdomen; soreness of flesh, &c., I put the patient upon disulphate of quina, in solution, at once; four or five grains every two hours. If great restlessness, and no sleep, I added three or four minims of tincture of opium to each dose. If general sinking of vital powers, some wine or brandy, with beef-tea. If the solution of quina were vomited, it was given in an equal quantity of wine, or wine-and-water. If the patient continued sinking, I increased the quantity of quina, but never had occasion to extend beyond seven grains per dose. When ringing in the ears occurred, the quantity was decreased, but still kept up till there was a good appetite. If the delirium was intense, the pain in the head described as heavy, (which were always strong subjects, whose bowels were confined,) with strong pulse, a dose of chloride of mercury, and sometimes a blister to the nape of the neck. If tenderness of abdomen was great, a few leeches or sinapism. If vomiting, a sinapism over the stomach. If diarrhœa, a little calomel and opium, or diacetate of lead and opium; though this symptom sometimes required nothing more than the tincture of opium given with the quinine.

In seventy-nine cases, marked improvement was observable in the course of twelve hours: the pulse was the first to improve, then the delirium to give way, then the pain in the head, the thirst, the soreness of flesh, tenderness of abdomen, dryness of tongue, and the appetite to improve. A pulse of 120, small and thready, would become 90, softer and fuller; and in the majority of the cases the pain in the head and delirium ceased entirely in the same period, and a rapid improvement followed; with the small exception of two cases of children ten and eleven months old, neither of whom had the medicine regularly, and both were complicated with bronchitis. The eightieth case was that of a woman, in which quina was not used. These three died, evidently from typhus. If the typhoid symptoms were allowed to become well marked, and the patient to sink much, his recovery was slow, and he required wine or brandy. I feel confident, from personal observation, that were the disul-

phate of quina to be used promptly, and to cinchonism, the mortality even of typhus itself would be very small.

P. S.—The practice alluded to is that of Dr. Whittle, who wishes me to make this communication to you.—*Lond. Lancet.*

Method of fixing Loose Bodies in the Knee-Joint. By Dr. E. BARTLETT.

All surgical writers agree upon the necessity and difficulty of properly securing loose bodies in the knee-joint, for the purpose of removing them. The following method is proposed, as one on which we can rely: Having placed the loose body at the upper part of the synovial cavity, to the right or left of the tendons of the rectus and vasti muscles, let an assistant firmly grasp the lower end of the femur with both hands, having one thumb on the right and the other on the left side of the foreign body, with their extremities in contact below it: thus confining it in a triangular space, two sides of which are formed by the thumbs, the other and upper one being bounded by the capsular ligament. This arrangement, with due pressure, secures it effectually, and without difficulty, gives the operator the use of both his hands, allows the incision to be made in the direction of the limb, and prevents the escape of synovia, and the admission of air or blood to the joint.

[*Boston Med. and Sur. Journal.*

Miscellany.

Our Journal.—In issuing the first number of another volume of the Southern Medical and Surgical Journal, we take occasion to return our thanks to the numerous contributors who have made it the medium of their communications to the profession, and who have thus added to its usefulness as well as reputation. Thirty-two writers, residing in nine different States, viz., Alabama, Florida, Georgia, Louisiana, Massachusetts, Mississippi, South Carolina, Tennessee and Virginia, have furnished us fifty-two original articles for the last volume, which will, we think, compare favorably with the original department of any of our contemporaries. The great majority of these papers are essentially practical, very few of them being theoretical disquisitions. This we regard as an important feature in this publication, and have always borne it in mind in making up the Eclectic department. Among our selections, we count sixty-six original *American* articles, derived from our respected exchanges, thus making no less than one

hundred and eighteen contributions to medical knowledge by our own countrymen, in the volume for 1852 ! We are not of those who will transfer to their columns every idle lucubration or hasty suggestion found in foreign prints, while they pass without notice the writings, however valuable, of their fellow-citizens. This indifference, manifested by some of our contemporaries, to the dissemination of American contributions, has been long felt and complained of by the profession—and while we know that we have not republished as many of them as we would have wished, we have the satisfaction to reflect that we have done as much as our limits would permit. There are Abstracts, Retrospects, and Periscopes published in Europe, republished in the United States, and bought extensively by our professional brethren, and which yet scarcely contain an allusion to anything done or written in this country. Why so ? Can it be that nothing is done or written by the American medical profession worthy of a place in such compilations ?

Dr. Norwood, and others, have now for two years been publishing from time to time, in this Journal, the discovery and usefulness of an agent capable of controlling with almost mathematical certainty the action of the heart in disease. There are upwards of thirty medical periodicals published in the United States, with all of which, save one, we exchange. Of these, we doubt whether more than five or six have ever noticed the discovery directly or indirectly. How different would the case have been if the newly ascertained property of *Veratrum Viride* had been first announced in England, in France, or even in the depths of Germany ! We might cite similar instances of disregard of valuable contributions published in the Journals of almost every section of our country. They are therefore not peculiar to this nor to any other of our native periodicals.

The rapidly increasing patronage extended to this Journal, gives us the gratifying assurance that our course meets the approbation of the profession, and no efforts will be spared to retain this.

We take pleasure in calling the attention of our readers to the valuable articles in this number, from the pen of Drs. Jackson and Norwood—both upon subjects of great interest, and by writers extensively known. We regard it quite an honor to be favored with the contributions of the great discoverers of the anæsthetic property of Sulphuric Ether, and of the heart-subduing influence of *Veratrum Viride*.

Case of Abstinence for three months, no Food, during that period, being taken into the Stomach. By J. L. PIERCE, M. D., of Fallsington, Bucks county, Pennsylvania.—On the 13th of January, 1836, my friend, Dr. May, was about leaving Philadelphia for the East, and requested me to take charge of a number of cases of considerable interest, which he then had under his care. Among these was a lady, who had for many months been severely affected with disease of the stomach. As the exact character of the disease may admit of dispute, and as it is not this to which I wish to draw attention, but a peculiarity in the mode of treating it, I have headed the article in the manner it stands.

I called, with Dr. May, to see this patient on the 13th of January, 1836. She was about 26 years of age, sallow complexion, emaciated, pulse 120, feeble, but regular. A spot over the left epigastric region, of an inch and a half in diameter, was very tender on pressure, and considerable tenderness existed for some distance around it. Her appetite, though not craving, was sufficiently good to enable her to eat almost any food, though she had been restricted to a very simple diet. Her bowels were open every two or three days, passages natural. She was in the habit of vomiting several times a day, and her food was generally rejected within a few minutes after it was taken. She had been sick about three months, and had been confined to the bed a large portion of this time.

I was informed that her case had been considered as carcinoma of the stomach. Her treatment had consisted of such articles as were calculated to allay the irritability of the stomach and the vomiting, and to quiet the severe pains of the epigastric region. A constant sore had been kept up over the affected spot by means of blisters, which were dressed with anodyne ointments, and occasionally leeches were applied to the spot.

On taking charge of the case, I watched it for some days very closely. I found that neither food nor medicine appeared to remain upon the stomach, but that as often as either was administered, vomiting ensued; and with the food ejected, there was from a tea-spoonful to a table-spoonful of matter, such as is usually discharged from an abscess. Should nothing be taken into the stomach for some hours, emesis would take place of this kind of substance by itself. During the first week, I pursued the same mode of treatment as had been made use of formerly, but I soon became satisfied that it was altogether useless, and that the patient must sink unless some more effectual plan were adopted. The simplest medicine taken into the stomach was rejected as soon as the severer kind, and even gum Arabic water or barley water was as irritating as oysters or roast beef could have been.

Towards the close of January, I asked her if she was willing to submit for one month to an entire abstinence of all kinds of food by the mouth, and be nourished solely by enemata. The idea was a novel one to myself, but my view was that the stomach must rest, and let the character of the disease be what it might, it would thereby

stand a much better chance of healing. That there was a suppurating sore in the stomach appeared evident, and rest was absolutely essential. She was now exceedingly emaciated, and so feeble that she was entirely confined to the recumbent posture.

On the 1st day of February we entered upon our new mode of treatment. I directed lamb or mutton broth of good quality to be kept constantly on hand, and a half a pint to be used as an enema every three hours. During the first week, I allowed her to take not exceeding a tea-spoonful at a time of gum Arabic water or pure water, several times a day, with the request that she would lessen the frequency of it towards the latter part of the week, so as to be able to refrain from it entirely on the commencement of the second week. She very readily acceded to my wishes in every respect, and I have not the least doubt of her acting with perfect honesty towards me. I kept up a running sore over the epigastric region, which I dressed with simple cerate or basilicon ointment, sprinkled with morphia.

During the first week my patient vomited from three to six times daily, discharging pus, and a considerable quantity of matter resembling tubercles. Her sensation of hunger was at times great, but she bore it with remarkable fortitude. At the expiration of the week she was evidently more comfortable, and became increasingly so during each succeeding week of the month, at the close of which her condition was as follows: Vomiting occurs on an average about twice a day; substance discharged the same as before, tinged with a little blood; tongue clean; pulse fuller than formerly, and less frequent; countenance more healthy. I now asked her if she were willing to continue the same treatment for another month. She did not hesitate in giving a prompt response; and, during this month, not a drop or morsel of any thing passed her lips.

April 1. Condition in every way improved. The vomiting occurs once in two or three days; quantity less; appearance of discharge white, with but very few of those particles resembling tubercles; bowels regular; pulse 90; very little pain; strength improving.

I now requested her to continue the treatment one month longer. She consented to do so, and it was astonishing how rapidly the improvement progressed. By the middle of the month I consider the cure to have been nearly completed, but thought it best not to depart from the course we had been pursuing, lest irritation of the stomach should renew the vomiting, and other unpleasant symptoms. About the 24th of the month, I allowed her a tea-spoonful of gum Arabic water three or four times a day, with directions to increase it in frequency each successive day, so that the stomach might be gradually prepared for its usual nourishment. By the close of the month I felt satisfied that the disease of the stomach had ceased, that the sore had healed, and that my patient, by a forbearance and perseverance seldom equalled, had been rescued from an untimely grave. She was now able to sit up a considerable portion of the day, a part of the time attended to some needle work or knitting; could walk some distance without assistance; complexion good; pulse 80; bowels regular, and much fleshier than when I commenced my attendance upon her.

I continued calling on the patient occasionally until the 18th of May, when I took my leave of her, with many blessings showered upon my head.

In consequence of her removal to Southwark, I lost sight of this patient for nearly two years, when I accidentally met her in the street. Her appearance had so much improved that I scarcely recognized her, and she assured me that she was in the enjoyment of uninterrupted health.—[*Am. Journ. Med. Science.*]

Binocular Microscope. (From the Transactions of the Phys. Med. Society of New Orleans.)—At a meeting of the Physico-Medical Society, on Saturday evening, 2d October, Prof. J. L. Riddell called the attention of the Society to an instrument of his own invention and manufacture, which promises to be of incalculable advantage in microscopic researches, especially in the prosecution of microscopic anatomy and physiology.

He remarked, that he last year contrived, and had lately constructed and used, a combination of glass prisms, to render both eyes serviceable in microscopic observation. The plan is essentially as follows:

Behind the objective, and as near thereto as practicable, the light is equally divided, and bent at right angles and made to travel in opposite directions, by means of two rectangular prisms, which are in contact by their edges, that are somewhat ground away. The reflected rays are received at a proper distance for binocular vision upon two other rectangular prisms, and again bent at right angles, being thus either completely inverted, for an inverted microscope, or restored to their original direction. These outer prisms may be cemented to the inner, by means of Canada balsam; or left free to admit of adjustment to suit different observers. Prisms of other form, with due arrangement, may be substituted.

This method proves, according to Prof. Riddell's testimony, equally applicable to every grade of good lenses, from Spencer's best sixteenth to a common three inch magnifier, with or without oculars or erecting eye-pieces, and with great enhancement of penetrating and defining power. It gives the observer perfectly correct views in length, breadth, and depth, whatever power he may employ; objects are seen holding their true relative positions, and wearing their real shapes. In looking at solid bodies, however, depressions sometimes appear as elevations, and vice versa, forming a curious illusion; for instance, a metal spherule may appear like a glass ball silvered on the under side, and the margin of a wafer may seem to ascend from the wafer into the air.

With this instrument the microscopic dissecting knife can be exactly guided. The watch-maker and artist can work under the binocular eyeglass with certainty and satisfaction. In looking at microscopic animal tissues, the single eye may perhaps behold a confused amorphous, or nebulous mass, which the pair of eyes instantly shape into delicate superimposed membranes, with intervening spaces,

the thickness of which can be correctly estimated. Blood corpuscles, usually seen as flat disks, loom out as oblate spheroids. Prof. R. asserted, in short, that the whole microscopic world could thus be exhibited in a new light, acquiring a ten-fold greater interest, displaying in every phase a perfection of beauty and symmetry indescribable.

[*New Orleans Med. and Surg. Journal.*]

Experimental investigations on the antidotal and revivifying properties of Nitrous Oxide.—Under this caption the Boston Medical and Surgical Journal contains the interesting researches of Dr. George J. Ziegler, who deduces from them the following conclusions :

Firstly, That nitrous oxide or protoxide of nitrogen is a powerful and direct arterial, nervous and cerebral stimulant.

Secondly, That it exerts a direct chemical influence on the blood, by supplying the essential elements for the arterialization of that fluid, and to a certain extent by inducing that process, thus producing in it similar changes to those effected by the atmospheric air, as proved by the effect on, and character of the reëstablished respiration.

Thirdly, That it is in these various modes antidotal to the effects of certain narcotizing agents.

Fourthly, That where vital excitability is not completely destroyed, this remedy has the power of sustaining and increasing it rapidly, and sufficiently to preserve life in numerous instances in which it would otherwise be destroyed.

Fifthly, That it will reëstablish life action even after all the usual evidences of its existence have failed, such as innervation, respiration and circulation ; provided, firstly, that the muscular contractility, or *vis insita* of the heart and other tissues is not lost ; secondly, that the blood has not coagulated or deteriorated to such an extent as to be insusceptible of arterialization and revivification ; thirdly, that there is no organic lesion of any vital part sufficient of itself to prevent recovery ; and fourthly, that innervation is still susceptible of réexcitation.

In conclusion, I will state that in my last paper on the therapeutic applications of this agent in the form of surcharged liquid more especially, I inadvertently omitted to mention a peculiarity in its physiological action, which, however, might be anticipated from its influence over the contiguous renal apparatus, viz., its stimulant effect on the generative organs, thus operating as an aphrodisiac. This effect, like its diuretic, is not, however, constant or universal ; yet, nevertheless, its application may prove useful in atonic states of this apparatus. With respect to its favorable therapeutic influences and applications, therein detailed, I have no reason to change my views, further experience and reflection only confirming still more strongly all former observations and impressions.

Preservation of Camphor.—The difficulty of preserving camphor in a state of powder is well known, and is especially experienced by those practicing in the country, who carry their own medicines with

them. Dr. Dubois, of New Utrecht, informs us that it can be kept without change in this condition, by rubbing together in the state of powder two parts of camphor, one part of rhubarb, and one part of ginger, the articles thus combined with the camphor not in the least interfering with its therapeutic effects.—[*New York Med. Times.*]

Prize Essays of the American Medical Association.—At a meeting of the Association, held at Richmond, Va., May, 1852, the undersigned were appointed a committee, to receive voluntary communications on medical subjects, and to award two prizes, of \$100 each, to the authors of the best two essays.

Each communication must be accompanied by a sealed packet, containing the name of the author, which will be opened only in the case of the successful competitors. Unsuccessful communications will be returned on application, after the 1st of June, 1853.

Communications must be addressed (post paid) to the Chairman of the Committee, Dr. Joseph M. Smith, 56 Bleeker-street, New York, on or before the 27th of March, 1853.

JOSEPH M. SMITH, M. D.

JOHN A. SWETT, M. D.

W. PARKER, M. D.

GURDON BUCK, M. D.

ALFRED C. POST, M. D.

New York, Sept. 14th, 1852.

Epidemics of South Carolina, Florida, Georgia, and Alabama.—The undersigned, Chairman of the Committee of the American Medical Association on the epidemics of South Carolina, Florida, Georgia, and Alabama, *vice* Dr. W. M. Boling, resigned, respectfully solicits from the physicians of those States accounts of epidemics of all diseases that have been observed by them during the year 1852.

In the description of each epidemic, attention is earnestly directed to the following points:

1st. The origin and progress, together with its phases, if they present any peculiarities.

2d. The cause, either certain or probable.

3d. Its contagious character, or otherwise, with proofs thereof.

4th. The prognosis.

5th. The treatment.

6th. Collateral circumstances, such as meteorological and other phenomena, having a direct or indirect bearing upon it.

The physicians of Georgia who may be disposed to furnish the committee with accounts of epidemics, will be pleased to address their communications to Dr. S. N. Harris, of Savannah, one of the committee. From those of South Carolina, Florida and Alabama, the undersigned will gladly receive contributions.

Charleston, Nov. 1st, 1852.

D. J. CAIN, *Chairman.*

Correction.—On page 726, in our last No., in the caption of Dr. WELLBORN's article, read Newnan, Coweta County, Ga., instead of "Newton County, Ga."

SOUTHERN MEDICAL AND SURGICAL JOURNAL.

Vol. 9.]

NEW SERIES.—FEBRUARY, 1853.

[No. 2.]

PART FIRST.

Original Communications.

ARTICLE IV.

On Spinal Irritation. By P. C. WINN, M. D., of Eufaula, Ala.

The obscurity under which the physiological endowments and actions of the *spinal system* have been veiled, has naturally resulted in much error as to its pathological conditions, symptoms and effects; for, unless the healthy functions of an organ are understood, it is impossible to reach a correct conclusion respecting its pathological state.

The spinal system constitutes the great nervous highway of the animal organism, and yet but little, we believe, was written upon, and less *known*, of spinal disease, until within the last half century: and although the researches and investigations of Sir Charles Bell, Dr. Marshall Hall, Magendie, and others, have thrown great light upon the anatomical structure and distribution of this system of nerves and upon its pathological conditions, causing it to be more thoroughly studied and consequently better understood than formerly; yet much, indeed, remains to be learned—a vast field is here open, inviting the earnest inquirer after truth to “seek that he may find.”

There exists such a reciprocity of action, especially in disease, between the spinal cord, its nerves, and the parts to which they are distributed, that the practitioner often finds himself lost, or led widely into error, in forming a diagnosis, when the spinal marrow and its nerves are the seat of disease.

The influence of this system of nerves is so universal and so

diversified, and that principle called sympathy, which is nothing more than the extension of a disease, or of the symptoms of disease, is here so extensive, and so little understood, that the physician encounters great difficulty in making a satisfactory diagnosis, and especially is there great difficulty in assigning to disease its true location when there exists no other evidence of inflammatory action except that of pain; for we frequently meet with disease of some of the viscera, and of other textures of the body, while the pain or disease is referred to the back, the spinal marrow, the head, while the converse of this is also true.

But we design speaking of *Spinal Irritation* as a special disease—a disease *sui generis*. It is not easy to define clearly what the disease consists in—whether it be an inflammation, increased excitability, an atony of the nerve, etc. And perhaps it will be as difficult to determine, with positive certainty, whether the disease is located in the substance of the spinal cord, the nerve leading from it, or in the investing membrane.

The term *Spinal Irritation* is one of such extensive application, and, too often, so vaguely used, that we must evidently encounter some difficulty in restricting it to its proper limits, and in applying it to that condition of the spinal cord which we intend to designate as *Spinal Irritation*. And in adopting the opinion which we do, we are conscious that it will conflict with the doctrine of some whose opinions are entitled to the highest regard. We hope, however, that this article will not be thought to savor of vanity, for the writer makes no pretensions to originality.

We are of opinion that the term strictly applies to an inflammatory condition of the spinal cord or the nerve given off from it, just at its exit from the vertebral canal, which is nothing more nor less than a myelitis or neuritis, and which may exist in any of its stages, from simple congestion up to acute inflammation or ramollissement. We do not propose considering it in its acute form, where the symptoms are clearly and distinctly developed, but in the latent form, where the symptoms are obscured and masked.

We are of opinion that the disease most generally consists of simple congestion; or, when of long standing, of chronic in-

flammation of the cord, the nerves leading from it, or of their immediate investing membrane. We come to this conclusion—namely, that it is a congestion or inflammation of the cord or its nerves—from a consideration of the physiological functions of these organs, together with the symptoms developed in the disease and the remedies most efficient in arresting and curing it. Now we are all aware that muscular action is dependent upon the spinal cord and its nerves, and also that the sense of feeling depends upon those in connection with the brain—in other words, that to produce the sense of feeling in the extremities, it requires, for instance, that the nerves, spinal cord and brain be intact, which state of things must also exist to insure voluntary motion. In the disease under consideration these functions are usually altered, increased, diminished, or perverted. For instance, if pressure be made over an irritated or irritable portion of the spinal cord, it may produce spasm of one or more muscles, cough, difficulty of breathing, nausea, pain, frequently very acute, in some remote part of the body, but always in a part supplied with nerves coming from that portion of the cord manifesting disease. And in this opinion we must beg leave to differ with some writers, among whom is Dr. Wood, of this country. He remarks, in treating of this disease, vol. ii., p. 738, that “the most rational view of the affection seems to be, that it is seated essentially in the ligaments of the vertebræ, and is generally of a rheumatic or gouty character.” But it appears to us that this view of the subject would not give a satisfactory reason for many of the anomalous symptoms attending the disease—on the contrary, we have little doubt but that many of the pains thought to be gouty and rheumatic, and treated as such, are nothing more than symptoms of spinal disease.

Among the many articles written upon, or having relation to, Spinal Irritation, we notice one by Prof. Ford, (vol. i., p. 166, Southern Med. and Surg. Journal, 1837,) on Remittent fever, which throws great light upon the subject now under consideration, and which bears the marks of earnest, ardent toil after truth, and which, too, has already accomplished much in making intelligent and modifying the treatment of remittent and intermittent fever—the great disease of the South.

In the article referred to, we find the opinions which we candidly adopt. In it may be found ample proof that the pain felt under pressure is not referable to the skin, the bone itself, or the ligaments, as some contend, but to the cord, its nerves and their membrane. From the remarks of J. B. Todd, on this subject, we infer that he, though less explicit than Prof. Ford, is of the same opinion. He says, "When not only pressure from without, but even the slightest motion, aggravates the pain, we are then forced to admit that the origin of the evil is in the vicinity of the spinal cord." And we conclude from these manifestations under pressure and percussion, such as pain, cough, dyspnœa, &c., that the disease is wholly dependent upon some morbid condition of the cord itself, or of the nerves given off by it, and not in the meninges or neurilemma. For, we cannot conceive how those remote symptoms could be developed were the membranes alone the seat of disease, though there might be pain and tenderness at some point in the vertebræ; nevertheless, we would not deny but that the membranes may be diseased at the same time with the cord or the nerves, and, in fact, we think it highly probable, from the vascularity of these membranes, that the disease may be primarily developed in them; yet, certainly, the thousand remote and anomalous symptoms cannot show themselves until the nervous tissue is itself involved. We must, however, confess that pathological anatomy has done but little to sustain the doctrine here assumed; and yet there need be no surprise at this, when we take into consideration the anatomical structure of the vascular system of this part—that is, the great number of veins and venous radicles, their free and numerous anastomoses with the superficial and muscular veins, as well as the absence of valves in them.

The author last quoted infers from the above facts, that a congestion sufficient to produce the symptoms may easily occur, and that congestion be easily dissipated. He contends that "the very sections which are made to reach the supposed seat of the disease are sufficient to dissipate the venous congestion." This much we have stated to show that anatomy affords some support to the doctrine of *Spinal Irritation*.

To give *in extenso* all the symptoms of this disease, in a

dissertation of this character, would lead us too far; for, in performing such a task we should have to enumerate the entire catalogue of diseases to which humanity is liable, as there is scarcely a disease which may not be simulated or masked by the one *sub judice*. The most prominent and, we might perhaps say, pathognomonic symptom, is that of pain or tenderness under pressure, percussion, or the application of a sponge filled with hot water. This pain is excited by motion, lifting or carrying heavy burdens, such as throw much exertion upon the spinal column. Frequently the patient is not conscious of any irritation or disease of the spinal marrow until detected by the physician: he may complain of some uneasiness, aching or vague sense of pain along the spine, but often the symptoms show themselves in some remote organ or structure. The symptoms may be those of pain, formication, numbness, spasm, paralysis, dyspnœa, dysphagia, palpitation of the heart, pyrosis, colic, many symptoms referred to the uterus, &c., nearly or quite all of which may be increased by pressure, or percussion over the affected region of the spine. The diseased point of the spinal marrow and the complaining organ will be found exactly to correspond—that is, the part to which the disease is referred being supplied with nerves directly from the affected point of the spine. Usually the tongue is clean, but may sometimes be coated with a whitish fur; appetite variable, usually however there is slight anorexia; pulse natural, or a little accelerated and weak, seldom or never increased in volume or frequency, unless there be inflammatory action elsewhere; irritable temper and melancholy are frequent attendants upon this disease.

The diagnosis is difficult, and requires no small amount of cautious and close investigation; for there are many diseases closely connected with, if not actually depending upon spinal irritation for their existence; and hence spinal disease is often mistaken for inflammatory or other affections of various organs. Hysteria, and all the host of diseases termed nervous, are usually primarily or secondarily connected with spinal disorder; nor should the practitioner neglect, in any such case, a close and thorough examination of the spine, or fail to apply the remedies suitable to the treatment of spinal disease; for it

may save him the regret of mistaking this disease (as often happens) for dyspepsia, colic, hepatitis, disease of the heart, kidneys, uterus, &c. And no doubt many an unfortunate patient has been made to undergo the tortures of a treatment for, first one and then another of these diseases, until he has *painfully* passed the ordeal of treatment for each, in succession, when, perhaps, a few cups or a blister to the spine would have at once relieved the sufferer.

Our young friend, Dr. W. C. Brandon, of Floyd county, Ga., recently gave us the history of a case in point, which happened in the person of a lady who had been the subject of disease for ten years, during which time she had been under the care of several *doctors*, who, in their turn, subjected her to treatment for a whole catalogue of diseases, with which they alternately supposed her to be affected, ptyalizing her two or three times, and passing her through an ordeal of treatment for "dyspepsia," liver "complaint," gravel, &c. When called in, he found her confined to the parturient bed, suffering with ovaritis, and complaining of pain in the region of the liver, kidneys, of dyspeptic symptoms, and of a "hurting" between the shoulders; but discovering (save in the ovary) no sign of inflammation, except pain, he examined the spine, and on finding the four upper dorsal and lower cervical vertebræ very tender, and some slight sensitiveness in the lumbar region, he had the upper portion blistered and sinapisms applied to the lower, which relieved those anomalous pains and the sense of "hurting" and "soreness" in the back and neck which had continued so long to annoy her, and (the ovaritis being in the meantime subdued) she, using occasionally vegetable tonics, soon found herself in a greatly improved state of health.

But, with care, there need be but little danger of mistaking the disease, where there is supposed rheumatism, dyspepsia, hepatitis, nephritis, gastro-enteritis, &c., and no symptom present, except pain, of the existence of a phlegmasia, the physician may at once be suspicious that he has a case of Spinal Irritation. Where spinal disease exists for a length of time, these and other affections may supervene from the irritation kept up in the part directly supplied by the diseased nerve, or nerve leading from the diseased portion of the spinal cord, or,

more remotely, from the ganglionic nerves becoming involved on account of their intimate connection with the spinal system, and, consequently, producing a functional, if not structural, disease.

We might enlarge upon the diagnostic symptoms between this and other diseases for which it might be mistaken: but in most instances—such as hepatitis, pleuritis, dyspepsia, nephritis, &c.—there are always evidences of the disease sufficient to prevent the mistakes to which we have alluded. The difficulty with young practitioners is too often a want of discrimination. If their patient complain of pain in a particular organ, the first impression on their mind is the existence there of inflammation, and having probably erred in the outset, a treatment is instituted accordingly. In all these cases of doubtful character the spine should be closely and thoroughly examined; for in cases where the spine is not primarily affected, it may become so secondarily, on account of that reciprocity of action (before mentioned) which exists between it and the parts which it supplies with nerves.

The prognosis of Spinal Irritation, *per se*, is not unfavorable. Where it is of long standing, we think it may probably bring on hydrorachitis, ramollissement, or possibly induration, which may produce death.

In giving expression to this opinion of the probable results of this disease, we are not aware of having any written facts to sustain the supposition. We think it probable that a distant organ, kept irritated by a diseased nerve, or a nerve leading from a diseased portion of the cord, may eventually produce such a morbid action in the part as to result in death.

Remarks upon the treatment of this disease need occupy but a small space, as it is simple and usually efficient. There are, perhaps, few or no diseases that give evidence of the therapeutic action of remedies earlier than the one under consideration. If the case is one of recent origin, and attended by any inflammatory excitement, perhaps the detraction of blood by cups to the affected part of the spine will be the most suitable course. But if of chronic character, and the tenderness not excessive, simple vesication will accomplish the desired end. In case a protracted counter-irritation is desirable, pustulation with

ungent. tart. antim. et potass. is advisable. Setons would accomplish the same result, though inadvisable if milder means will answer the end. Where the irritation is slight, or occurs in other diseases, as in intermittent and remittent fever, the application of sinapisms will frequently relieve the patient in an exceedingly short time. It may often be necessary to use some constitutional remedies in connection with the local treatment, as, for instance, when connected with dyspeptic symptoms, it may be necessary to use some mild cathartic and antacid. If general debility exist, tonics, vegetable or mineral, as the practitioner may think best, should be administered—the ferruginous preparations should never be omitted, if the patient is anemic or chlorotic.

We might extend remarks upon the treatment of this disease, and especially upon the treatment of those diseases connected with or dependent upon it—such as hysteria, chorea, perhaps some cases of dysmenorrhœa, &c., but we have already overleaped the bounds assigned to ourselves in this dissertation.

ARTICLE V.

Remarks on the Use of Empirical Remedies ; with some suggestions as to the best means of abating the evil. By JOHN S. WILSON, M. D., of Air-Mount, Alabama.

Dr. Robert Campbell, in an able essay,* recently published, has very appropriately compared the relationship of Empiricism to Medical Science, to the antagonism existing between virtue and vice ; for every one will be struck with the appositeness of the comparison, when he contemplates legitimate Medicine, with her noble aims, her “divinity of purpose,” and her self-sacrificing devotion to the cause of suffering humanity—and then turns his attention to Charlatanry, with all its base tricks “for coining guilty gain,” with its numberless artifices to prostitute Science to its unhallowed service, and its ceaseless efforts to impair the confidence of the community in those who are

* An Essay on Empirical Remedies ; read before the Medical Society of the State of Georgia, April, 1852. By Robert Campbell, M. D., Chairman.

the legitimate conservators of the public health, and the true expounders and guardians of medical science. To say, then, that empiricism and medical science are as much at variance as virtue and vice, or light and darkness—to say that physicians, by virtue of their position and education, are in duty bound to expose the tricks and fallacies of charlatanry and to warn a credulous and uninformed community against its impositions, is but to utter a truism. Yet, while this is admitted by our profession, it is a lamentable truth that we have been recreant to the responsible trust committed to us: the most of us “are prone to regard quackery, in its wide spread and rapidly increasing operation, with a bearing of complaisant regret and supine resignation, as a necessary and inevitable event.”* Some among us “testimonialize for quacks”—some are engaged in the sale of their nostrums—while many prescribe them “without ascertaining their composition.” And, alas! there are *some* who have sacrificed honor, a pure conscience, professional respectability, *every thing* that adorns the man of virtue and of science, to lucre, and enlisted in the “*legion of quackery!*” I do not intend to review the influences which combine to sustain the “nostrum trade:” this has been ably done in the essay referred to; and I do hope that its circulation will be co-extensive with its merits, and that it will receive the attention demanded by the importance of the subject discussed.

As the principal design of this article is to make some suggestions, as to the best means of counteracting the monstrous impositions of quackery, I have alluded to the humiliating derelictions, and the supineness of physicians, not only because I consider these to be the most potent auxiliaries of empiricism, but also because I think if the evil under consideration is ever successfully combatted, it must be done by our profession. It is true, that the influence of our noble profession has been much diminished by the protection afforded to quackery by legislative enactments, together with the low standard of education and morals among us; but I believe if this latter difficulty can be removed we can accomplish much, *in spite* of hostile legislation; and I even cherish the sanguine hope that we will finally

* Dr. Campbell's Essay.

be able to *reform* our legislators, and induce them to abolish the Patent Medicine Law, together with several State laws which are equally inimical to the public welfare, and the dignity and usefulness of the profession.

As a proper appreciation of the causes of an evil is a desirable, if not an essential, pre-requisite in devising a judicious plan for its removal, no apology is needed for briefly alluding to the causes, which, in my estimation, have conspired to degrade Medicine, by transforming some of her legitimate descendants into quack-procurers and nostrum venders. The most influential of those causes is, doubtless, the imperfect system of medical education among us, whereby many receive the honors of our colleges who are morally and intellectually incapacitated for a proper discharge of the responsible duties devolving upon them as physicians and men of science; and this defective system of education, I think, owes its existence to two principal causes—viz: 1st, the multiplicity of medical colleges, and the consequent rivalry; and 2d, the disregard of the interests and respectability of the medical profession, manifested by our State legislatures, in the passage of laws which place every ignorant and unprincipled medicaster and half-educated licentiate on a footing of perfect equality with those who have been crowned with the highest collegiate honors, after years of laborious and expensive study. For in this utilitarian age, it can hardly be expected that our medical institutions will be very stringent in requiring high moral and intellectual attainments, when they know that these advantages will bring to their alumni no legal protection or immunities; while the majority of students, influenced by similar considerations, will flock to those colleges where they can “pass” with the greatest facility and expedition. These are serious obstacles to medical “reform;” but still I am fully persuaded that they can be obviated, by the combined action of the profession, through the medium of County and State organizations, and, above all, by co-operating with our National Association in its efforts to consummate the noble work for which it was instituted.

I shall, therefore, leave these remote influences in the able hands to which they have been committed, and proceed to

make some suggestions as to the more direct means of elevating the moral tone of the profession, with regard to quack nostrums; and of thus depriving quackery of the encouragement given it by physicians. To promote this desirable object, until a more radical reform can be effected, I would suggest, 1st. That the Faculties of our Medical Colleges make it a *special* duty to impress upon their classes their moral and professional obligations as physicians, and that they be warned, seriously and *earnestly* against compromising the dignity of the profession and the interests of the community, by giving quack certificates—by prescribing secret and unknown compounds, or by engaging in their sale. No one, who has read the history of medicine, or noted the tenacity with which students cling to the errors, even, of their instructors, will, for a moment, doubt the potency of their influence, when this is fortified by truth, by honor, and by professional interest and respectability. So fully am I convinced of the powers of our professors in the premises, that I can attribute the extent of the evil under consideration, only to a dereliction of duty, on their part, originating either in indifference or want of a correct appreciation of their own influence: I would therefore invoke their attention to these humble suggestions, and entreat them to avail themselves of their commanding position, by warning their pupils continually against any alliance with empiricism.

I would suggest, 2nd. That all within the pale of the profession, who, in any manner, encourage the use of patent or secret remedies, be "*read out*," and that they be declared unworthy of "the rights, privileges and honors of physicians"—such as, 1st, the privilege of a seat in any medical society or association; 2d, the honor of consultation; and 3rd, the right (conventional) of gratuitous medical attention in sickness.

I will conclude these brief suggestions, by submitting the "PLAN" of the committee before referred to; and I would recommend its adoption by the profession generally, as the best means of acting on the people—*after the profession* has been expurgated from empiricism.

PLAN. "1st. The establishment of an efficient standing committee in this body, whose duty it shall be, with the assistance of the general society, to collect and publish, as far as

practicable, *through the popular channels of intelligence*, all the instances of the pernicious effects, resulting from the employment of empirical remedies, which may come under the observation of the society—for the advisement of the people.”

In the above extract, I have taken the liberty of italicising the words “*through the popular channels*,” &c., because I think that this mode of attack has not been properly appreciated by the profession: much has been written in exposition of the injurious effects of empiricism, and almost every medical journal contains something of this kind; but these disclosures never reach the *people*, and so far as their influence on them is concerned, they had as well be published in Hebrew, as in a medical journal. The *popular channels of intelligence*,* must be used then, to disclose the monstrous evils of quackery and the antidote thus made to accompany the bane.

The 2d recommendation of the committee is, in general terms, as follows:—That the committee report the facts thus collected to the American Medical Association; soliciting their recommendation of such a plan to the several State Medical Societies and Associations of the Union—“*for the purpose of accumulating evidence sufficient for the arraignment of this injurious system, as a national grievance.*” And also for this committee to solicit the interference of the National Association, in memorializing the National Congress for the abolition of the patent medicine law, &c.—the evidence of the joint report of the several State committees being adduced as the reasonable apology for such application.

ADDENDUM.—Since writing the above, my attention has been arrested by the following requirement of the St. Louis University. I transcribe it from the October No. of this Journal, for the purpose of giving it my hearty endorsement, and to recommend its adoption by all our colleges, not only *pro forma*, but *bona fide*.

“The Doctorate at the St. Louis University. 5th. And that

* Besides the papers of the day, I would suggest that each State Society avail itself of that happy device of Quackery—‘a *Medical Almanac*,’ and that the facts collected, together with some of the principles of Medical Science, be published in this, for gratuitous distribution.

he publicly assent to the following promise, prior to the conferring of the degree—viz :

You, A. B., do solemnly promise that you will, to the utmost of your ability, exert your influence for promoting the welfare and respectability of the profession; that you will demean yourself honorably in the practice thereof; that you will not put forth any nostrum, or secret method of cure, nor engage in any other species of quackery; and that you will not publish any matter or thing laudatory of yourself, or derogatory to the profession; and in the conferring of this degree, it is done with the express understanding that the Faculty reserve to themselves the right and privilege to revoke said degree whenever the promise here made shall be violated."

ARTICLE VI.

Bite of a Copperhead—"Trigonocephalus Contortix"—treated with Whiskey. By N. HARRIS MORAGNE, M. D., of Abbeville, South Carolina.

On the 21st of June last, I was called to see a negro man belonging to Capt. P——, of Abbeville district. Found him partially delirious; skin hot and dry; pulse very much excited, ranging from 100 to 120; left leg and ankle swollen to a great degree. Upon making enquiry into the history of this case, I learned that the patient had been bitten about twelve hours previously by a "trigonocephalus," or, as it is frequently styled in this part of the country, copperhead or highland mockeson. This very poisonous reptile was concealed beneath the step of a *meat-house*, and inflicted a wound upon the inside of the foot, near the ankle-joint. I immediately applied a ligature above the seat of affection—prescribed poultices over the wound; and olive oil, ammonia, &c., internally.

22d. The patient is *in statu quo*—no abatement of the swelling, delirious: ordered whiskey, *ad libitum*.

23d. No decided improvement—still anxious, restless, and uneasy; skin hot and dry. Continued the whiskey, combined with capsicum: it was administered until the patient was fully under its influence, without regard to quantity. Left opium to be given if necessary.

24th. Had passed the "crisis." A profuse perspiration was out over his entire system; the tumefaction was subsiding; the delirium had ceased; he spoke rationally, and speedily convalesced.

Gibson says—"Of the numerous American serpents, two species only are known to be poisonous—the crotalus or rattlesnake and copperhead." If he includes under the common name of copperhead, both the highland and the water-mockerson, then we concur with him in the assertion. The two latter are of the same family, but not of the same species, which is abundantly manifest by their mode of living.

The same writer says: "These reptiles are more lively, and their venom more active, during very warm weather. Upon the approach of the cold season, they become languid, and then strike reluctantly, and frequently without any ill consequence."

The interesting *case* which I witnessed whilst a student in the University of New York, furnishes a striking proof of the speedy operation of the poison, even in the dead of winter.

Dr. W., of that city, was bitten on the hand, by a rattlesnake, sent to him by a friend from the State of Alabama. The hand soon began to swell, and in a few hours the whole arm was very much tumefied, presenting a mottled appearance, even to the shoulder and axilla. He had the best medical advice the city afforded, yet, after intense suffering for two or three days, he died.

I have no doubt, that if the *alcoholic treatment* had been instituted in this case, with the application of the ligature above the seat of affection, (which last was timely suggested by some of the Southern students present,) Dr. W. would have recovered.

ARTICLE VII.

Cases of Lithotomy. By L. A. DUGAS, M. D., &c.

However unimportant the report of individual cases in medical or surgical practice may sometimes appear, unless they offer new facts for consideration, they are not without value in a statistical point of view and in the collation of facts from which practical inferences are to be deduced. Were practition-

ers less backward in laying their experience before the profession, whether successful or otherwise, a vast amount of useful information would be accumulated, to which systematic writers could often refer with advantage. The three cases about to be placed upon record, are all of the kind operated upon by the writer during the past year, and will serve to show that surgical operations performed under the most favorable auspices will sometimes terminate fatally, whereas others may prove successful under the most unfavorable combination of circumstances. There are therefore in surgery as well as in medicine, elements of controlling influence yet to be studied—yet to be appreciated.

CASE I.—Mr. John M. W., of Heard county, Ga., about 24 years of age, had been suffering a number of years with stone in the bladder, when I visited him on the 10th day of March, 1852. I found him confined to his bed, as he had been for some months, and suffering excruciating paroxysms of pain about every hour when attempting to urinate. He appeared to be a man of robust constitution, although considerably debilitated and emaciated in consequence of his local affection. The general condition of his system seemed to be quite favorable to the operation, so that, the calculus having been readily detected, we determined to remove it the next morning.

The usual preparations having been made, I proceeded on the 11th, at 9, A. M., to the administration of chloroform, under the direction of Dr. J. Harriss. The patient was very soon brought under its full influence and the calculus removed by the bi-lateral section and the double lithotome, according to Dupuytren's method. The whole time consumed was unusually short, and the patient returned to consciousness in a few minutes, without having suffered any pain. He expressed himself much gratified, was cleansed and put to bed, feeling as comfortable as any one I ever saw under similar circumstances, and conversing with his friends. He continued so until after dinner, when, having other engagements, I left him in charge of his judicious neighbors, Doctors Redwine and Hunt.

I subsequently received the painful intelligence that he died 28 hours after the operation. Dr. Redwine, who furnished me the facts, states that a short time after my departure, the patient

became sick at the stomach, then restless, taking but short naps, and frequently calling for water; that he complained during the night of pain in the stomach and bowels; that the wound having become closed with coagula, bloody urine was passed per urethram, but resumed its course through the wound upon this being washed with tepid water and a syringe; that the loss of blood was very slight; but that he went on gradually sinking, without any evident cause, until 1 o'clock on the 12th, when he died.

Some might be disposed to attribute the fatal result to the Chloroform; yet, when we reflect that the patient required very little of it to induce anesthesia—that he regained his consciousness and clearness of intellect in a few minutes, and that he remained so several hours before showing any unpleasant symptoms, such an inference would seem scarcely justifiable.

Two calculi were removed, consisting of oxalate of lime. The larger of the two weighed 32 grains, was of irregular shape, being $\frac{3}{4}$ of an inch long, $\frac{1}{2}$ an inch wide, and $\frac{1}{3}$ of an inch thick. The smaller one weighed 10 grains, was cylindrical, and measured $\frac{5}{8}$ inch in length and $\frac{3}{8}$ of an inch in thickness.

CASE II. The subject of this case was Mr. Zac. S., of Lincoln county, Ga., aged 21 years, and upon whom I performed the Bi-lateral operation of Lithotomy on the 23d of June last, in the presence of Drs. McCord, Wilkes, Bently and Dunn. I am indebted to Dr. McCord for the following account of the patient's history.

"The first symptoms of stone were manifested in his early infancy, but did not assume a positive character until he was two years of age, when he was annoyed with straining, difficult and painful micturition, the flow of urine being sometimes suddenly arrested. He continued in this state, being sometimes better and sometimes worse, until 14 years of age, when there appeared to be an almost complete subsidence of suffering, unless he rode on horse-back. This kind of exercise would be followed by painful urination and the discharge of mucus. This deceptive abatement of the disease continued but a short time, after which all the previous symptoms returned. The least exercise would aggravate them, and he would pass large

quantities of blood and mucus with his urine. I first saw him in January last. He had then frequent, urgent and irresistible desires to pass urine, attended with straining, burning, and shooting pains along the urethra. The mucus would sometimes remain mixed with the urine, and sometimes separate from it and form a deposit in the bottom of the urinal of a viscid tenacious matter, which would adhere to the vessel when inverted. The flow of urine appeared sometimes to be impeded by it. There was great tenderness and pain over the region of the bladder, in the perineum and extending to the sacrum."

I visited Mr. S. on the 22d of June, and found him in bed, to which he had been uninterruptedly confined for upwards of six months. Emaciated in the extreme, without appetite, but great thirst, he experienced about every fifteen minutes (both night and day) the most excruciating paroxysms of painful efforts to urinate. Worn down by long suffering, loss of sleep, impaired digestion, &c., he seemed to be on the verge of the grave. The sound revealed the presence of a calculus of some size, but it was very doubtful that he could, in his reduced condition, bear the operation required for its removal. There was, however, no time to lose, and, after frankly stating the unpromising prospect to the parties concerned, it was determined to operate the next morning. Dupuytren's bi-lateral section was made, under the influence of chloroform, and an ammoniaco-magnesian calculus removed, which weighed two ounces and two scruples, was globular, its greatest diameter being $1\frac{1}{2}$ inches, and its least $1\frac{1}{4}$ inches. During the operation he fainted and appeared to be lifeless, but gradually recovered and was put to bed. A few hours after the operation, it was observed that the oozing of blood from the wound was greater than usual, and that in his debilitated condition it might be fatal, unless checked. The usual means were adopted to lessen this hemorrhage, but it continued until midnight, rendering the free use of brandy and broth necessary to sustain life. The loss of blood was not such as to have been serious to a patient less exhausted, but in this instance it was exceedingly alarming. It was probably occasioned by the great vascularity of the bladder and prostate consequent upon such long standing irritation.

I left the patient, twenty-four hours after the operation, in charge of Drs. McCord and Bently. In three days he was fully convalescent, and gradually got entirely well.

CASE III. Stephen M., of Jasper county, 5 years of age, had suffered from stone in the bladder three years, when he was brought to this city by his mother in October last. His general health was but slightly impaired. Being put under the full influence of chloroform on the 21st, the bi-lateral section was performed as in the above cases, and a calculus of uric acid removed. It was a flattened oval $\frac{3}{4}$ of an inch long, $\frac{1}{2}$ inch wide, and $\frac{1}{4}$ inch thick, weighing 35 grains. The child rapidly recovered his health without any unpleasant symptoms.

REMARKS.—The first case terminated fatally, although the condition of the patient was as favorable as we usually find it in calculous persons, and no appreciable accident or complication occurred to account for the result. Death took place too soon to have been induced by infiltration of urine or any inflammatory process. There was no amount of hemorrhage sufficient to induce sinking. He recovered very soon from the influence of the chloroform upon the brain, and although the pain in the stomach (epigastrium) annoyed him some time, nothing is more common than to find this effect of chloroform without serious consequences. This death must be put down to the account of what is called, for want of a better term, nervous exhaustion.

The second case resulted in restoration to health under circumstances of the most unpromising character. Whether the syncope was occasioned by the chloroform or not, is difficult to determine. He had inhaled very little of it. It is probable that had he breathed any more of it, the syncope would have been fatal. The oozing of blood still added to the danger; yet he recovered, under the judicious management of his physicians and the good nursing of kind parents.

The third case was successful, as they generally are in children when the operation is not too long deferred.

PART II.

Eclectic Department.

Clinical Illustrations of Sub-acute Ovaritis. By E. J. TILT, M. D., Senior Physician to the Farringdon General Dispensary and Lying in Charity, and to the Paddington Free Dispensary for women and children.

The light of modern physiology thrown on many facts derived from the highest sources, enabled me, some years since, to apply to these facts the rules of reasoning adopted in general pathology, and I then endeavoured to give a wider and safer basis to our knowledge of the diseases to which the ovaria are liable, and by so doing, to lend my humble efforts to oppose a tendency to consider the womb as principally affected in all diseases of women. Others have subsequently added to our information on these points; but I am not aware that my views have sustained any serious attack. In the papers published in *The Lancet*, as well as in a work on "Diseases of Menstruation and on Ovarian Inflammation," I avoided one chance of error by principally making use of facts registered by others, without any preconceived views of their importance. I now propose to publish some of the cases I have met with, leaving others to decide how far they support the views I have put forth.

CASE 1. Emma W——, twenty-two years of age, of middling stature, and with red hair and grey eyes, was admitted to the Paddington Free Dispensary, July 14, 1851. She menstruated at twelve, and has ever since been regular every month, even during pregnancy and the ten months she suckled her child. For several months previous to, and since, weaning the child, she has suffered much from pain in both ovarian regions, which pain was always increased by menstruation, by walking, by pressure, by ascending the stairs, or by any sudden jar. Lately, the left ovarian region has become the most painful, and the left breast has been likewise sore and swollen. For the last few weeks the legs swell at night; there is slight leucorrhœa, little fever, and she complains of feeling "heavy for sleep," and would sleep on all day if she could.

On making a digital examination, there was no sign of uterine disease, but pressure directed towards the left ovary was intensely painful. I ordered the following compound camphor mixture:—Solution of potash and tincture of cardamons, four drachms each; tincture of hyoscyamus, six drachms; camphor mixture, six ounces: a tablespoonful to be taken three times a day, and a small quantity of the following powder to be taken

in a little milk at night:—Sulphur, two ounces; biborate of soda, one ounce; while three or four drachms of the following ointment were to be applied, not rubbed, over the lower portion of the abdomen:—Strong mercurial ointment, one ounce; extract of belladonna, two drachms. I then directed a thin linseed poultice to be applied over the anointed surface, and over that a piece of oiled silk, with the understanding that this application was to be removed and re-applied as soon as possible in the morning, at two or three in the afternoon, and before disposing the patient for her night's rest. (I thus enter into minutiae, as their observance alone leads to success.)

July 17th. The patient is better: the pains are only violent at times; there is no leucorrhœa, and the bowels are comfortably moved.

21st. Ovarian pains all gone; the mercurial ointment is therefore discontinued.

28th. I learnt that on the 22nd, after an attack of diarrhœa, menstruation returned ten days before it was due, but unaccompanied by ovarian pains. I prescribed the following pills, to be taken at night:—Sulphate of quinine, one scruple; extract of opium, five grains; extract of liquorice, a sufficient quantity to make ten pills. But upon leaving me, and before this treatment could be begun, menstruation again appeared, and there was a throbbing and swelling of both breasts, and pain referred to the pubis. On making an examination, I found the neck of the womb hot and swollen; I ordered injections with a solution of acetate of lead, and I returned to the application of the compound mercurial ointment. I saw the patient after the subsequent menstrual period, which was normal as to time, quantity, and pain; the womb was ascertained to be healthy, and the patient was quite recovered.

This case was first one of sub-acute ovaritis, lasting for months, until the increased uterine activity, swelling of the womb, and irregular and prolonged menstruation, necessitated the employment of local measures to remove uterine congestion. The mercurial applications, however, should not have been discontinued on the subsidence of the ovarian pains; for as the womb was in a healthy state on the 14th inst., if they had been continued, the slight attack of uterine disease would have probably been avoided.

If I had not positively ascertained, on the 14th, that the womb was in a healthy state, I should, on finding it slightly diseased, on the 28th, have concluded that the previous pains were to be attributed to the beginning of undiscovered uterine affection, and not to ovaritis, which I believe to have been the primary affection, determining the uterine inflammation in the

same way that the physiological congestion of the ovary determined the physiological congestion of the womb in menstruation.

CASE 2. Sarah H——, thirty-two years of age, with black hair, grey eyes, and a Roman cast of countenance, was admitted to the Paddington Free Dispensary on the 2nd of June, 1851. She first menstruated at fifteen, and has always been regular; but at each period she had suffered from pain in the right leg and thigh. She married at twenty-one. Conception never took place. The last menstrual period came on eight days before it was due, and was attended by a great increase of the pain habitually felt in the leg and thigh. The catamenial flow lasted its usual time, but the pains did not abate on its disappearance, and in addition the patient suffered from constant pain in the right ovarian region, augmented by exertion, by walking, or by pressure. The water was freely passed; the bowels, which were much relaxed during the last epoch, were now confined; there was no leucorrhœa; and a digital examination indicated that the womb was healthy; slight fever; and pressure directed towards the right ovarian region increased the patient's pain. I ordered an antimonial mixture, and the compound mercurial ointment, directing it to be used as in the preceding case; likewise the compound sulphur powder, as in the previous case.

June 4th. Better in every way: the fever is reduced, the pain less intense, and pressure on the ovarian region determines less pain. The ointment was ordered to be continued, and the compound camphor mixture as prescribed in the first case. A small quantity of carbonate of soda was ordered to be taken in a little cold water after meals, and two compound colocynth pills every night.

9th. The patient came limping into the room with tears in her eyes. The pain in the right thigh had left her, but that in the right ovarian region had returned with more intensity than before. There was difficulty in passing water. On making a digital examination, I found that the womb was lower down than on the 2nd June, and that its neck was swollen and painful in its posterior half; no leucorrhœa. The patient was ordered to continue the former treatment, and, in addition, to have, twice a day, as an enema, a cupful of clear starch, in each of which enemata were to be exhibited forty drops of laudanum.

12th. Four enemata had been administered to the patient, and the pains had disappeared. I subsequently ascertained that at the next period she menstruated after her usual manner.

The painful uterine swelling detected on the 9th of June, was certainly secondary to that of the ovary, since, on the 2nd, the womb was found healthy.

CASE 3. Sarah W——, a lymphatic girl, aged twenty-one, with light hair and blue eyes, tall and slender; has been living in London, as a servant, for the last four years. Menstruation appeared at seventeen, after suffering for a month from headache and great drowsiness. From the third month of its first appearance menstruation came on regularly. It assumed the monthly type, lasting four or five days, and being in small quantity.

Eight days before the last epoch, she was seized with intense pain in that portion of the left hypogastric region which corresponds to the ovary. Notwithstanding the pain, she did her accustomed work, and menstruation came on at the regular time, and as abundantly as usual. Unfortunately she was at that time obliged to go up and down stairs much more than usual; and far from being relieved by the catamenia, there remained an intense pain, which she compared to labour pains. She became hot, thirsty, and feverish at night, and she was admitted as a patient at the Paddington Free Dispensary on January 9th. I found the abdomen sore, particularly in the left ovarian region, where was also a sensation of fulness felt by the patient herself, and a slight swelling, which could not be detected on the right side. Walking or moving on the left leg, and pressure on the painful spot, increased the pain. There was no leucorrhœa, nor pain above the pubis.

I ordered eight leeches to the seat of pain, and poultices to be afterwards applied over the leech-bites; but as the tongue was foul, and there was a tendency to sickness and constipation, I also ordered an emetic, and compound colocynth pills.

The leech-bites bled freely; the vomiting seemed to relieve the intense pain, which was, on the contrary, exasperated by the purgative action of the pills; and when I saw her on the 13th, she was better in every respect, and the intense pain was circumscribed in the left ovarian region. There was no pain above the pubis, no leucorrhœa, no external irritation.

This intense pain produced by menstruation, so well limited to an organ we know to be highly congested at that time, unconnected with uterine disturbance, with peritonitis, what is it? Those who attribute to the womb all acute disease of the genital apparatus, will say that the pain in the left ovary was the result of the uterine congestion determined by menstruation; and as, in the absence of marked uterine symptoms, I did not feel myself justified in making a speculum or even a digital examination, I could not object to the position, if, in similar cases (Nos. 1, 4, 6, 9, & 10.) an examination had not been made, without any uterine disease being detected. Considering, therefore, the case to be one of sub-acute ovaritis, I followed

up the previous treatment by the use of mercurial ointment, as in the former cases.

16th. The patient was comparatively free from pain.

27th. The ointment, pills, and mixture, were continued until menstruation came on at the usual time, and in the usual manner. After it was over, the pain in the left side had completely disappeared; but on considerable exertion, or pressure on the right ovarian region, the patient experienced the same kind of pain, only less intense. She was directed to continue the use of the ointment and poultice at night only; and on the 10th of February she was discharged cured.

CASE 4. Mary C——, aged twenty-two, looked like a Flemish girl, very stout, with a ruddy complexion, auburn hair and blue eyes. She was admitted at the Paddington Free Dispensary, May 15, 1851. Menstruation had been easily established at sixteen, but had always been accompanied by a great deal of lumbo-abdominal neuralgia, appearing every month, or sometimes missing for two, six, or nine months without any considerable inconvenience. She had been very lately married. On interrogating the patient, I heard that when menstruating, three weeks ago, the flow, after lasting two days, had stopped for three, and then returned for one day. Its subsidence was followed by a violent pain in both ovarian regions whenever she moved about. It hurt her to pass water. There is considerable pain in the right breast, slight leucorrhœa, and considerable fever. Any sudden movement of the lower limbs brings on the pains, so does ascending the stairs, and pressure on the ovarian regions, particularly on the right, which I found swollen and tense; a state of things the patient expressed by saying "she was all on one side." A digital examination convinced me that the womb was normal; the finger directed towards the ovaries greatly increased the pain, particularly on the right side, where a solid lump could be distinctly felt, while on the left side nothing similar could be distinguished, although pain was determined by pressure with the finger. The bowels were quite regular. I prescribed an antimonial mixture, and ten grains of Dover's powder at night, the compound mercurial ointment to be spread on the ovarian regions, and the application of linseed-meal poultices.

22nd. The fever had abated, the pains in the back had disappeared, and those in the ovarian region were less intense. The same treatment was continued.

Menstruation returned at the third instead of at the fourth week, and had lasted its usual time, when, from standing for three hours, it returned as a flooding, and continued for several days.

June 4th. She was much better ; pressure on the right side still, however, brings on pain and a sensation of numbness in the right limbs. In July the patient had menstruated regularly, and was free from pain.

If this was not a case of sub-acute ovaritis, developed under the influence of matrimony in an excitable girl of a sanguine habit, what is the disease to be called ? The very abundant uterine discharge may have been considered critical in this instance, as it was followed by decided improvement.

CASE 5. Elizabeth W—, twenty years of age, a brunette, dark hair, hazel eyes, of slender make and middling stature, is a needle-woman, single, and in good circumstances. After slight pains in the back, she menstruated at twelve ; was regular from the first ; the catamenia occurred every four weeks for three or four days, very abundantly. They had stopped for a year without any known cause, and they had returned without physic, being regular for the last three months, but much less in quantity, and paler in appearance than usual. She was admitted to the Farringdon Dispensary, January 8, 1850, complaining of violent pains in both ovarian regions, which pains had originated during the last insufficient menstrual flow, and had remained after its cessation. She also complained of a good deal of pain at the pit of the stomach, the intensity of which was said to be proportional to that experienced in the ovarian regions, increasing and diminishing as the latter increased and diminished ; but the pain at the præcordial region was relieved by pressure ; while this measure increased that in the ovarian regions. There was great lowness of spirits, involuntary tears, invincible drowsiness, and a sensation of stupidity on waking. No uterine symptoms, no leucorrhœa, no fever or thirst.

I considered the case one of sub-acute ovaritis, and I ordered five leeches to each ovarian region, to be followed by the constant application of thin linseed poultices, and the compound camphorated mixture to be taken.

22nd. The patient had been much relieved by the treatment ; the pains, instead of being continued, had become intermittent, occurring two or three times a day ; she still continued drowsy. I ordered the mercurial ointment to be made use of as in the preceding case, an opium plaster at the pit of the stomach, and myrrh and aloes pills to be taken a day or two previous to her epoch.

27th. After taking the pills, which procured three motions, the patient menstruated more freely than the last time, and with more than usual forcing pains. Two more pills taken two days after the first, produced the same effect, and since menstruation is over, there is much less pain.

Feb. 3rd. I gave her sulphate-of-iron pills, and a few weeks after, as menstruation had become normal, and the patient's health had so much improved, she was discharged from the dispensary. In this, as in several of the preceding cases, the patient complained much of feeling unusually heavy, dull and stupid, with a great tendency to sleep; a state of the nervous system which frequently expresses the physiological or morbid action of the ovario-uterine apparatus on the brain. As it is convenient to express this in one word, I call it pseudo-narcotism; and it is right to remark that I have noticed this state in connexion with sub-acute ovaritis, in women who did not habitually experience this pseudo-narcotism at the menstrual periods.

CASE 6. Jane A——, twenty years of age, of a sanguine complexion, reddish-brown hair, and blue eyes, tall and stout, is a washerwoman. After two years of great suffering, she menstruated at eighteen, the flow appearing regularly from the first every three weeks, lasting three or four days; was of a trifling amount, and never missed until marriage, which took place at nineteen. Two months ago she miscarried, lost a great deal of blood, and since then has not menstruated. Admitted Jan. 13th, 1850, at the Paddington Free Dispensary. The patient complained of great forcing pain in both ovarian regions. She said the pain was increased by walking, by pressure, and by going up and down stairs. She also complained of severe *forcing* pain at the nipples, which was increased by the ovarian pains, as well as by anything hot she might take. There was no appearance of disease in the breasts, no leucorrhœal discharge, nor, on a digital examination, any evidence of uterine disease. I ordered the treatment prescribed in the former cases. The patient got gradually better, and at the next period she menstruated regularly.

CASE 7. Emily B——, admitted to the Farringdon General Dispensary, October 27th, 1851. She was twenty-three years of age, of a sanguine temperament, with reddish hair and grey eyes, and unmarried. Menstruation first appeared at seventeen and continued regularly every month; but for the last few months the excretion was very pale and scanty, and the last catamenial flow only lasted one day, and was attended by a good deal of pseudo-narcotism, leaving, without any known cause, a severe and constant pain in the right ovarian region, but during the day only; for when quiet in bed, the pain left her. The exertion of going up stairs increases the pain, likewise slight pressure on the right side, while great pressure on the left ovarian region gives no pain. Slight leucorrhœa; no fever; but the mouth looked as if painted with yellow dust;

the tongue was furred, and the patient said she had been bilious for the last few days; the bowels were regular. I ordered an emetic at night, the compound camphorated mixture before mentioned, and carbonate of soda after meals; and the compound mercurial ointment to be applied in the usual way to the side affected.

Nov. 1st. All pain and uneasiness had left the patient.

15th. She returned on account of a cough, and I learnt that she had just menstruated, and that for many years the discharge had not been so abundant, of so good a colour, and attended with so little suffering.

In this amenorrhœal form of sub-acute ovaritis there was no chlorosis.

CASE 8. Anne B——, aged nineteen, with very dark hair, and eyes dark and lustrous, swarthy skin, and sanguine complexion, of average stature and size, and menstruated for the first time, without prodromata, at seventeen. Three months after its first appearance, the discharge re-appeared painfully, but with regularity, every fortnight, lasting three or four days, and was very abundant. Recently, after four months of amenorrhœa, menstruation returned of its own accord, much more abundantly, and with more pain, during which time, being exposed to wet, she caught cold, and was seized with violent pain in the right ovarian region, accompanied by considerable thirst, heat and fever. Another gentleman had attended her at her own house, and ordered twelve leeches to the seat of pain, poultices, and some medicine; and after being confined to her bed for a fortnight, menstruation returned at the usual fortnightly period, with more clots and greater pain, and lasted seven days, instead of three or four.

On the 20th of February, 1850, she was admitted to the Paddington Free Dispensary. The pain in the right ovarian region, although much abated, was still considerable, and was exasperated by stooping, walking, or by pressure. No leucorrhœal discharge. I ordered the usual compound camphor mixture, the ointment, poultices, and two grains of sulphate of quinine, with two of extract of hyoscyamus, in a pill, every night; but before the treatment was begun, the patient again menstruated, and the function lasted with great pain for three days; and on its disappearance, a light leucorrhœal discharge ensued. The treatment was then put in force, the local pains gradually disappeared, and on the 4th of April I heard that menstruation had re-appeared at monthly periods.

Aug. 11. She applied again, for an attack of jaundice; and I learnt that she had not again suffered from pains similar to those which brought her before to the dispensary.

Dec. 1851. Anne B—— again called, and stated that, during a severe cold, menstruation came on at the usual time, lasted eleven days, ceased by degrees; and that upon its cessation, the ovarian pains, bearable before, became intolerable, causing her to walk doubled, particularly when going up stairs. She could not wear her stays, pressure determining a darting pain.

It is interesting to observe how, in this patient, the menstrual function, which by the use of sulphate of quinine, I had brought to the four-weekly type, soon relapsed from the normal to the three-weekly, and then again from the three-weekly to the fortnightly, which it now adopts; and how, in close correspondence with these irregularities of physiological action, there is also a tendency to relapse into the morbid condition, which I think deserves to be called sub acute inflammation.

CASE 9. Dr. Vinen, of Bayswater, requested me to see a patient of his, in September, 1849. Mrs. L—— was then twenty-eight years of age, with a pale complexion, middling stature, dark hair, and hazel eyes. She had first menstruated at twenty, but was always irregular both as to time and quantity, it being sometimes scanty, at others very profuse. Since marriage the function had become more regular, but was still variable in amount.

In the previous January she complained of acute pain in the right ovarian region. Two months after, consulting Dr. Vinen, he discovered a distinct swelling in that region, and, some weeks after, the same appeared on the left side, accompanied by great tenderness at all times, but particularly at the menstrual epoch. The catamenia then became more scanty, darker, and more painful than usual. There was dorsal pain and slight leucorrhœa. Twelve leeches had been applied, with but little benefit, but blisters had been more efficacious. When I saw the patient she was exhausted by continued suffering, and was at times affected with hysteria. Menstruation had not appeared for the last two months. Digital examination was painful both to the vagina and to the womb, which was somewhat swollen: there was acute pain on pressing in the direction of the ovaries, the right one being still swollen; both breasts were very painful. I ordered the treatment previously described.

On the 1st of August the patient was better; examination was no longer painful; pressure in the ovarian regions was less so; the womb was neither swollen nor painful, neither did it present any lesion when examined through the speculum. In addition to the previous treatment, I ordered cold-water injections to be made twice a day, per rectum, and twice a day a vaginal injection of two drachms of tincture of hyoscyamus in half a pint of tepid water.

The patient rapidly improved, lost all pains, and became stronger. She has had no relapse, but, although married several years, has never been pregnant.

CASE 10. Charlotte K.— called upon me October 18, 1849. She was about twenty-six years of age; her constitution being lymphatic, but her hair and eyes dark. In childhood she had several abscesses in one of her legs and groin. Menstruation appeared at fourteen, but at sixteen was suspended, from her catching cold: and when it did return it was three-weekly instead of monthly as before. It was either profuse or scanty, and preceded for a week by great pain in the ovarian regions. Pressure, walking, or stooping, aggravated this pain. This state lasting for several years, had brought dyspepsia, palpitation, hysterical symptoms, and there was often leucorrhœa.

Mr. Pughe, of Aberdovey, in North Wales, considering the case to be one of chronic ovaritis, sent her to me. Digital examination was so painful, that I contented myself with having ascertained that the vagina and neck of the womb were swollen, hot, and inflamed. Pressure on the ovarian regions was also very painful. Leeches had been applied to them a fortnight previously, and with great benefit; I therefore ordered twelve more to be applied, prescribing the usual treatment, with the addition of aloes pills, and cold-water injections per rectum.

Oct. 28th. I was able to make a speculum examination, and ascertained that there was no ulceration of the womb, which I was led to expect from the persistence of many symptoms which usually indicate it. When the finger in the vagina was directed towards either of the ovaries, a sickening pain was determined; and when the left hand pressed moderately on the ovarian region, so as to press the mass of intervening tissues between both hands, the pain became intolerable.

This patient was for several months under my care. After each menstrual epoch, six leeches were applied to each ovarian region; when the leech-bites had healed, a blister was applied to the same part; and when these were healed, the same surface was anointed with mercurial ointment until the time when menstruation made its appearance.

Feb. 13th, 1850. She was without pain or discharge, and menstruation had assumed its normal type, completely losing the prolonged pains by which it had been accompanied. In fact, she returned to Wales quite well.

As the preceding cases are fair samples of many others which I have met with, it may be well to see to what deductions they lead.

1st. With regard to the predisposing causes of sub-acute

ovaritis, irregularity of the menstrual process is one of the most frequently observed; for eight times out of ten it occurred in connexion with menstruation. The earlier part of womanhood also would seem most liable to this disease, as seven out of ten patients were under twenty-three, and the eldest was only thirty-two. Those too are most liable to it who are endowed with a sanguine constitution, dark eyes, and red, auburn, or black hair, constitutional peculiarities generally supposed to be allied to an ardent temperament. In this, my experience is supported by that of Dr. Pistocchi, of Bologna, who has lately published some interesting cases of ovaritis, and says that all the patients were women gifted with strong passions. Six out of the ten patients were single.

2nd. The determining causes were over-exertion during menstruation, the sudden impression of cold, marriage. In five cases, however, none could be discovered.

3rd. The symptoms: pain in one or in both ovarian regions. The pain being fixed, but sometimes subject to irregular exacerbations, being increased by pressure, by going up and down stairs, by a false step, or by anything that could jar the corresponding limb. It is well to notice that pressure on the ovarian regions did not generally determine pain in the course of the lumbo-abdominal nerves. In two cases the pain was accompanied by an amount of abdominal swelling discernible to the eye, obscurely felt on pressure on the abdomen, better appreciated by a vaginal examination, and which would have been made certain if a rectal examination had been deemed requisite. In five cases there was considerable pain and swelling of the breast corresponding to the side affected, and of both when both sides were diseased. This symptom was most marked in Case 6, which did not occur at a menstrual epoch. Dr. Pistocchi has noticed it in two cases; but I think Dr. Lightfoot has gone too far in considering it as pathognomonic of ovarian inflammation. Thus, the mammary glands, the uterus, and the ovaries, form a chain of organs as strongly linked together in the morbid as in the physiological state. In Case 1 there was numbness and pain in the corresponding limb, a symptom noticed by Dr. Simpson. In four cases there was fever, but of no great intensity.

4th. The terminations or morbid conditions induced by sub-acute ovaritis were, a painful congestion of the womb in four out of ten instances; in three, remittent menstruation; a deficiency of the menstrual excretion in three more; and in two, bilious plethora. Dr. Rigby has dwelt on the sickening nature of the pain determined by ovaritis; and Dr. Woolley, of Brompton, tells me he has often seen cases similar to those

above described by me, and frequently noticed sickness as one of the symptoms. Dr. Laycock alluded to it some time back as a symptom frequent in this, as in all ovarian states, both physiological and morbid.

5th. *Treatment.*—The same local measures previously described were always adopted, with the addition of leeches and the internal exhibition of antimonials when there was fever. In the cases coinciding with marked biliary derangement, I prefaced all treatment by an emetic, the temporary increase of pain thus mechanically determined being amply compensated by the relief speedily afforded to the patient. Sulphate of quinine was very useful in bringing back menstruation to its normal type; and I cannot too strongly recommend its exhibition alone, or combined with steel or opium, according to the case.

6th. *Duración of the disease:* Generally about twenty-one days; but in one case, of probable long standing, it was four months, and in another there was a relapse.

With this summary of my cases I might conclude, if they did not afford me an opportunity of offering some remarks on the diagnosis of sub-acute ovarian affections—remarks, the length of which will perhaps be indulgently received, on account of the recognised difficulty of arriving at a correct diagnosis.

Cases similar to the preceding have doubtless been of frequent occurrence, but they have been differently interpreted.

I. Formerly when they were met with, and sometimes even now, particularly when they do not occur at the monthly periods, they were confounded with diseases of the womb, and called inflammation of the bowels—a name which will doubtless be considered erroneous, as far as the localization of the disease is concerned, but which, being correct in the indications of its nature, fortunately often leads to proper treatment.

II. When cases similar to those I have reported took place at, and in connexion with, the menstrual periods, they were, and are even now, confounded with many other morbid states, under the name of dysmenorrhœa. They are considered to be merely an increase of that pain by which menstruation is usually attended, and generally left without treatment. This I believe to be often detrimental to the patient's after health.

III. Some would be inclined to explain my cases by incipient uterine disease, and might be impelled by theory to resort to measures, excellent in uterine, but unnecessary, if not dangerous, in ovarian disease. Being in doubt as to four out of the ten cases, a digital examination convinced me that there was no uterine disease; and in the history of the other cases there was nothing to make me suspect its existence, nor to warrant a vaginal examination.

Admitting that mine were neither cases of inflammation of the bowels, of dysmenorrhœa, nor of uterine disease, I must remark that they can only be explained by supposing them to depend on a nervous affection of the ovary itself, or of the lumbo-abdominal nerves, which supply *alike* the womb and the ovaries and their protecting cavity, unless I am right in considering them to exemplify a subdued type of ovarian inflammation. It would be impossible for me to show that they did not depend on ovaralgia or lumbo-abdominal neuralgia, unless I be permitted to clear the ground by a few remarks on these affections.

Ovaralgia has been admitted by systematic writers, vaguely described by German pathologists, and lately brought prominently forth under the name of ovarian irritation, by Dr. Fleetwood Churchill. But while admitting that the ovaries, like the uterus, may express their own disorder by pain transmitted to the same system of nerves, we must also bear in mind that those nerves may take upon themselves a morbid action, quite independent of diseased ovaries or womb—that lumbo-abdominal neuralgia may exist.

Certain forms of *lumbo-abdominal neuralgia* were well described by Chaussier; but it is only since the modern investigation of the nervous system, that it has been permitted satisfactorily to explain, by lumbo-abdominal neuralgia, certain morbid states formerly ascribed to the abdominal viscera. Without pretending to say that ovaralgia does not exist, I must own that I have never as yet been able to detect it, and am inclined to think that cases described as such are to be referred to lumbo-abdominal neuralgia. I make this assertion with some hesitation, because by so doing I find my opinion opposed to that of an obstetric authority of so great a value, that by differing from it I incur the risk of being wrong; but, if wrong, my dissent will furnish Dr. F. Churchill the opportunity of more forcibly vindicating his own opinions.

On perusing Dr. F. Churchill's interesting communication on Ovarian Irritation, in the impression of the *Dublin Medical Review* for July, 1851, and comparing it with what Drs. Beau, Valleix, and some other French authors, have written on lumbo-abdominal neuralgia, it will, I think, be evident that they have all described the same disease. Neither would it be difficult to explain the mistake: for it is well known that it is in the nature of the affections of nerves to be attended by pain more concentrated in certain points, whence at times pain radiates, and pressure to which increases pain. The lumbo-abdominal neuralgia is often indicated by one or more of the following *foci* of pain: 1, the lumbar; 2, the iliac; 3, the hypogastric; 4, the inguinal; 5, the uterine.

I think that Dr. F. Churchill, being particularly struck by the inguinal or ovarian point of pain, has described, under the name of ovarian irritation, a complaint which has been justly referred to a morbid sensibility of the lumbo-abdominal nerves by Drs. Valleix, Oxenfield, Beau, and others. He has followed in this the example of Gooch, who described as irritable uterus those cases of lumbo-abdominal neuralgia in which the neck of the womb is the principal centre of pain; an example already set by neuro-pathologists, who have described as spinal irritation an ill-defined group of symptoms.

I refer the reader to Dr. F. Churchill's paper, and to the French authorities I have quoted, in proof of the great similarity, if not identity, of the morbid state described as ovarian irritation or lumbo-abdominal neuralgia. But, under all circumstances, I object to the term *ovarian irritation*, because it has already been employed to express the physiological action of the ovaries, and imports another vague and indeterminate term into ovarian pathology, already sufficiently obscure. If it be only pain, let it be called ovaralgia, or lumbo-abdominal neuralgia.

Supposing it to be conceded, until further researches, that ovaralgia is but another name for lumbo-abdominal neuralgia, then it only remains to me to establish the diagnosis between it and sub-acute ovaritis, which is often rendered difficult by the similarity of the seat of pain in both complaints. Those of a nervous temperament are most liable to lumbo-abdominal neuralgia, not brunettes of a sanguine constitution, as in most of the cases given in the previous papers. Pain exists in all, but while in sub-acute ovaritis it is more fixed, continues with the same intensity without regular exacerbation, and is exasperated by every kind of pressure, in lumbo-abdominal neuralgia it is quite the contrary; for although there may be at all times a dull, aching sensation, it is frequently not so, and the pain recurs by repeated attacks, and is relieved by wide or even by continued pressure with the united tips of the fingers. Dr. F. Churchill rightly says, that what he terms ovarian irritation is characterized by a kind of nervous tenderness which shrinks from the weight of the finger as much as from severe pressure, and not by the *positive* pain, as in my cases.

There is no swelling, no heat, no pain of the ovaries, when these organs are subjected to a rectal examination, as correctly stated by Dr. F. Churchill, whereas there is heat, swelling and pain, in sub-acute ovaritis.

The pain is unaccompanied by any sympathetic pain of the breasts, or fever, in lumbo-abdominal neuralgia; not so in sub-acute ovaritis. Lumbo-abdominal neuralgia is so frequent an

accompaniment of uterine disease, that Dr. Beau and others expect to find it when the former exists, and Dr. Bennet looks upon its ovarian forms as almost pathognomonic of uterine disease, while sub-acute ovaritis is not so frequently induced by uterine disease. Lastly, with regard to the treatment. Repeated blisters and opium are of most use in lumbo-abdominal neuralgia, but such remedies, valuable in the later stages of the disease, require to be employed after leeches, emollients, &c., in sub acute ovaritis.

If my argumentation is not at fault, then my ten cases were examples of sub-acute ovaritis; and, moreover, the complaint can be diagnosed with a degree of precision quite sufficient for all practical purposes, without having recourse to any internal examination. It was so in six out of the ten cases, while digital examination was necessary to establish the diagnosis in four, but a rectal examination in none.

Taught by former experience, wherein the coincidence of sterility or uterine disease rendered imperative a more accurate examination, I concluded from the symptoms narrated that the ovaries were sub acutely inflamed, although my finger did not feel them swollen, although I did not see the patient wince under the moderate pressure of the finger mediatly applied to these organs; but I maintain that whenever the case is complicated or great pressure is required, it is necessary to make a rectal examination. From not having done so in the following case, I did not understand the whole of its bearings, and some readers may think that I was completely wrong.

CASE II. Sarah N——, twenty-three years of age, is tall, of slender make, with light brown hair and grey eyes; she is a washerwoman in good circumstances. After painful prodromata, lasting six months, menstruation first appeared at thirteen, and at once assumed the monthly type, lasting five days, and being very abundant, bright and clotty. She was married at seventeen, and has had two children, and during both lactations, prolonged to the thirteenth and twentieth months, menstruation occurred every fourteen days with unusual abundance. Three months previous to her applying to me for relief, she consulted a physician, who judiciously told her to wean the child. On doing so, menstruation returned to the monthly type, and became less abundant; but while still debilitated by the effects of habitually profuse menstruation, and about the time when it should have come on, she got wet through, caught a violent cold, attended by fever, and instead of the catamenia appearing, the patient was seized with agonizing pains in both ovarian regions, described by her as bearing down and pinching pains, and augmented by standing long or by pressure.

There was no leucorrhœa and no pain about the pubis when, January 13th, 1850, she applied for relief at the Paddington Free Dispensary. I ordered five leeches to each ovarian region, and the usual treatment; but not hearing anything more of the patient, I sought out her abode, and learnt that she had derived benefit from the treatment, but that having been obliged to exert herself more than usual, on account of her mother's illness, she had miscarried of a three-months fœtus three weeks after coming to the dispensary.

Dr. Rigby admits that ovaritis is one of the most frequent causes of abortion, and possibly my diagnosis was correct, and a relapse may have brought on the miscarriage. This view seems to be confirmed by the great tendency to exaggerated ovarian action ever since the patient first menstruated, while the sudden invasion of acute pain in the ovarian region, in consequence of menstruation being interrupted by a feverish cold, would certainly again cause me to give a similar diagnosis; but I abandon the case, as it admits of more than one interpretation, and merely bring it forward to show that I acquired but a very imperfect knowledge of it, because I neglected a vaginal and rectal examination. If the patient had been thus examined, early pregnancy would have been detected, and she would have been warned against the excess of fatigue which caused the death of the embryo, and the prolonged illness of the patient. This case is well worth the attention of those medical men who object to a rectal examination on the ground of its being an unclean practice; as if a medical man can be stopped by such a consideration when health is at stake!

We cannot conclude without observing that sub-acute inflammation of the ovaria does more than cause temporary ill-health; it may determine or be accompanied by ovarian peritonitis, and even set up inflammation in the surrounding portion of the peritonæum. Many obstetric writers, as well as Burns, Sir C. M. Clarke, and Dr. R. Lee, have noticed the great frequency of lesions of the ovaria, and of false membranes in their vicinity, even in unmarried women—lesions not to be accounted for by any previous severe uterine disease, from which it could be ascertained that the patients had suffered.

With regard to peritonitis, I will merely cite the practice of one London hospital, St. George's, where the post-mortem appearances are noted with a praiseworthy care. In 1850 the medical practice afforded four instances of idiopathic peritonitis; three occurred in young women of twenty, twenty-one, and twenty-four years of age; with two the menstrual function was deranged, and the patients anæmic; the third, though

married eighteen months, had had no children, and, after passing pus in her stools, she recovered.

Now, it seems to me that the cases I have previously related are explanatory of those of peritonitis. Again, there is a form of peritonitis to which women are more subject than men, in which bridles of lymph are so attached as to bind the intestine, and produce fatal incarceration. Why should such bridles be more frequent in the vicinity of the ovaria? I will answer this question, as I found it in one of the last impressions of the *London Medical Gazette*. An eminent pathologist, Dr. Renaud, of Manchester, relates the death of a girl, twenty-one years of age, from two bridles of lymph, which produced an impenetrable stricture of the ileum, and adds the following reflections:

“My own experience in pathological anatomy, and a perusal of the cases recorded by others, justifies the conclusion that peritonitis in females arises in many instances out of a chronic form of congestive irritation, to which the generative apparatus within the abdomen is liable. That this long-continued irritation, as manifested directly by pain and throbbing in the region of the ovaries and uterus, and indirectly by lumbar irritation, dysmenorrhœa, crural pains, &c., does frequently advance to local inflammatory action, is sufficiently evident from the bands of lymph that are so frequently seen matting the ovaries, broad ligaments, and oviducts together. If, therefore, folds of bowel or portions of omentum are in contiguous relationship with parts influenced by these morbid actions, it is not contrary to rational pathology to infer that they will partake in a limited degree of the same actions; and a bond of union being thus morbidly constituted, it only requires time and the peristaltic action of the bowels to elongate the lymph into a band, which, under accidental circumstances, may prove an incarcerating medium.”

Rokitansky is likewise of opinion that internal constrictions of the intestines, are much more frequent in females than in males; a result which indeed might have been anticipated, on account of the great changes in function, structure, and position, to which the abdominal viscera are subject, by menstruation, pregnancy, and from uterine and ovarian tumours; and this esteemed author also reports that—

“In two instances with which I am familiar, the pressure of the prolapsed ovarium, loaded with purulent fluid, produced in each case a fatal form of *ileus*. In one of these the tumour filled the rectum; neither bougie nor injection could be conveyed beyond it, and such was its apparent solidity, that I did not for a moment contemplate puncturing. But the deception was fatal to the patient. The second case, very similar in all re-

spects to the first, occurred in the practice of a surgeon in the country, who sent me its history, and the morbid parts for examination."

The pith of the present communication can be summed up in a few words:

I. It seems urgent on us carefully to bear in mind the frequency of inflammatory products in or about the ovaries, the frequency of intense suffering in the ovarian regions at the menstrual periods, and the great probability of both facts standing one to the other in the relation of cause to effect.

II. It seems incumbent on us to bear in mind the greater liability of young women to idiopathic peritonitis, and incarceration from bridles of inflammatory lymph, at the very age when I have shown that even the sub-acute inflammation of the ovaries is most frequent, and therefore the imperative necessity of watching over the first stages of a complaint, which, being too often left to nature, is as frequently productive of serious mischief.

III. Lastly, that sub-acute ovaritis can be distinguished from uterine affections as well as from lumbo-abdominal neuralgia, but that at all events no harm can ensue from the treatment recommended.—[*London Lancet*.

Letters upon Syphilis. Addressed to the Editor of L'Union Medicale, by P. RICORD. Translated from the French, by D. D. SLADE, M. D.

[Continued from Page 682, vol. 8.]

FIFTH LETTER.

My dear Friend,—I promised to commence to-day the great questions to which the study of blennorrhagia gives rise. I shall endeavor to do honor to this serious engagement; serious in fact, for, as I hope to be qualified to show, the point that I undertake to discuss at this moment may be considered as the key stone to the syphilographic edifice.

All that I have thus far said upon blennorrhagia, relates to simple blennorrhagia, which may be considered or not as the product of a special virus, but a virus completely foreign to that which syphilis, properly called, produces. However, this blennorrhagia, according to a great number of authors, can produce consecutive accidents perfectly identical to those which chancre produces. It is incontestable that a great number of patients, affected with constitutional syphilis, do not accuse for antecedents anything but blennorrhagia. These patients are sometimes right. I do not deny the fact; but after having

verified it, I do not confine myself to leaving it in a crude state, and to crying out with emphasis, it is a fact, and then oppose it with intolerance.

The entire question can be reduced to these terms: when a blennorrhagia has been the point of departure of a constitutional syphilis, has there not been something else than that which we have before studied in blennorrhagia properly so called? Experimentation has proved, and pathological anatomy has come to its aid, that the urethra, and the deep and concealed points of the other genital mucous surfaces, can be the seat of chancre, the necessary source of syphilitic accidents. It is for not having recognized the concealed chancre, that the doctrine of Balfour, of Tode, of Bell, and that great scaffold built upon the experiments of Hernandez, have very nearly given way.

With the doctrine of the existence of urethral or concealed chancre, the virulent blennorrhagia cannot be doubted; it is identical with chancre, it is the chancre itself.

This idea is not new in science, and I am astonished that the detractors from priority have brought nothing against me in this respect. However, it is a long time since the ulcerations of the urethra were recognized. Mayerne, in the seventeenth century, attributed at that period the urethral blennorrhagia to pus produced by ulcers within the urethra, and gave to it the name of *pyrroia*. Many others still, whom I do not wish to recall, have verified the presence of ulcerations in the urethra; but do you not consider it strange to see Swediaur, who sustains the identity of blennorrhagia and of chancre, say precisely that which cannot be denied, viz, that blennorrhagia is virulent when ulcerations exist in the urethra!

If in three autopsies of persons hung, who were affected with blennorrhagia, Hunter did not prove the presence of ulcerations in the urethra; if in an autopsy of which M. Phillippe Boyer has given an account; if in some others still nothing has been found, it is because they had to do with simple blennorrhagia. I have shown to the Academy of Medicine two specimens of pathological anatomy, the designs of which and the accompanying observations may be found in the *clinique iconographique* of the Venereal Hospital, and upon which MM. Cullerier and Lagneau have made a report. These specimens presented some chancres of the urethra at different depths, which previous to death had been recognized by inoculation.

Thus, inoculation first, and pathological anatomy afterwards, have proved, in an incontestable manner, the existence of chancres of the urethra. To tell the truth, no one denies it, even those who wish to ascribe to simple blennorrhagia the conse-

quences of syphilis. The chancre concealed in the urethra is not, then, an hypothesis, but a fact proved as certainly as any other medical fact. And yet, singular phenomenon! those even who have best studied the chancre of the urethra—who, like M. Baumès, have been able to recognize it *at the depth of an inch* in the canal—when it comes to establish the logical deductions of its existence, love better to launch into the field of hypothesis, than to admit that which observation and good sense point out to them. Observe, in fact, M. Baumès and others, establish, with a rare sagacity, the differences which exist between chancre and blennorrhagia, in tracing with clearness the differential characteristics, and arriving, at the end of his comparison, to conclude upon the identity of these two accidents.

It is always, dear friend, the same contest between the logic of facts and the preconceived ideas of which I have noticed the results even in the great mind of Hunter. Very recently I have again perceived these singular manifestations, in a pamphlet, otherwise interesting, of M. Lafont Gouzy fils.

But here some serious objections present themselves. “The existence of chancre in the urethra cannot explain all the cases of constitutional syphilis, which appear to have blennorrhagia as a point of departure.” “The number of urethral chancres is too small relatively to that of the constitutional veroles with blennorrhagia as antecedent. In fine, there are some cases of blennorrhagia in which it has been impossible to verify the urethral chancre, and which have been followed by constitutional accidents.”

Here I am going to astonish greatly my antagonists by making the concession that all this is true. But you will see, dear friend, that this concession is but apparent; for I hasten to add, that which ceases to be true are the explanations which have been given of these facts.

It is very certain that relatively to the immense number of blennorrhagias which exist, the symptomatic blennorrhagia of concealed chancre in the urethra constitutes the exception. In fact, they say to me, with an appearance of reason, but how is it, then, that the number of cases of syphilis coming on after the pretended chancre of the urethra, should be almost in proportion with the veroles coming on after the external chancre? Here, my dear friend, I ask all your attention, not because I wish to be subtle or captious, but because the form of reasoning which I am forced to employ to answer this objection, itself very subtle and captious, has need of being followed in all its conditions.

Yes, the chancre concealed in the urethra is rare.

No, the number of veroles, the consequence of chancre concealed in the urethra, does not appear rare.

You are about to cry out, sophistry; but hear me.

That chancre in the urethra is rare, is incontestable; my experiments, those of my honorable colleague and friend, M. Puche, and those of many other observers, have proved it without reply. Do you wish that I establish a proportion? I much desire to do so. Let us admit 1 in 1,000, which is, I am convinced, far greater than the reality. Let there be, then, on one hand, 1 chancre of the urethra, in 1000 cases of blennorrhagia. Do you recollect on the other hand, how frequent and extended is blennorrhagia? Do you recollect that Lisfranc, with perhaps a little exaggeration, said that out of 1,000 adults, he counted 800 who had had, who had then, or would have blennorrhagia? However this may be, my dear friend, out of 1,000 cases of blennorrhagia, there are 999 of which you never hear mention, which will have had no unhappy consequences, against a solitary one, which will have determined the constitutional infection.

It is a small number, without doubt, but make your calculations upon the hundreds of thousands, upon entire populations, upon the population of Paris, for example, which numbers three to four hundred thousand adult men; compute the number of blennorrhagias contracted in this great city; only calculate for the concealed chancre but the small number of 1 out of 1,000, and you will still arrive at a sufficiently large number of blennorrhagias which would consecutively determine the verole.

Well, what happens in practice? That you do not see in the hospitals nor at the consultations of physicians, but those patients in whom the syphilitic infection has been preceded by a blennorrhagia with a concealed chancre. A physician of a hospital devoted to these diseases, could meet, in the course of his practice, with ten, twenty, thirty examples; but what is that in comparison to the number of simple cases without any unhappy consequences? But those patients who have no other antecedent than the blennorrhagia for their constitutional infection, strike the mind of observers; the remembrance of them remains deeply engraved; their number, relatively small, increases in their imagination, and they do not fail to present this as a formidable objection to the non-identity of blennorrhagia and syphilis.

You see to what this objection is reduced; I hope that I have destroyed it. I am accused of founding an hypothesis with the concealed chancre, of establishing a system. However, I have proved the fact of its existence by pathological anatomy.

I have deduced it also from my experiments with inoculation. Is it not true that blennorrhagia in the immense majority of cases is exempt from every consequence of syphilis? To what, then, can we attribute the infection when it comes on after blennorrhagia? I myself attribute it to concealed chancre! and my adversaries—to what do they attribute it? To a pretended identity, which the observation of every day, and great abundance of facts, incessantly contradict. And it is I whom they accuse of being systematic, I who elevate a doctrine upon the basis of observation, of experimentation and of pathological anatomy. What, then, are my adversaries, who, for the sole support of their doctrine, invoke but a rude fact, the interpretation of which does not repose upon any of the elements necessary at the present day for the demands of science!

Believe, then, dear friend, that it is my adversaries who launch themselves into the way of hypothesis, whilst I, on the contrary, strive to bring them back into the path of reality. You see now that it is easy to conciliate these two terms of my proposition.

Yes, the chancre concealed in the urethra is rare; but the number of veroles, the consequence of chancre concealed in the urethra, does not appear small. It does not appear small, because we see again only those patients who have been suffering from this concealed chancre; but if a strict proportion could be established between the cases of blennorrhagia not followed by syphilitic accidents, and those which have given place to them, we should see that the last are proportionally very rare, and that this appearance of frequency is entirely illusory.

But in other respects, in all the cases in which the constitutional verole has been referred to blennorrhagia, have all possible precautions been taken in order not to be led into error? I do not believe it, when I see that some are contented with a diagnosis offered by the patient, and with his own history. We could truly say that the physician has in some way declined his jurisdiction. You will see some striking examples of this confidence of the physician in the story of his patient, in the works of MM. Martins, Cazenave, and in the thesis, in other respects so well written, of M. Legendre.

How many causes of error there are in the stories of patients! Blennorrhagia is ordinarily a very painful and annoying accident, and one which leaves behind some smarting recollections to those who have had it. When you interrogate patients upon their previous history, it is always of their blennorrhagia that they first speak; they do not suspect the importance that the chancre can have, which, while it infects, is ordinarily

indolent, suppurates but little, has little tendency to extend, and often cicatrizes of its own accord; it is rare that they make mention of this accident, and if by a pressing inquiry you cause them to bring the circumstance to mind, they will tell you that it was a superficial chancre, a simple excoriation. I am allowed to call to mind, that it is only since my works, that the manner of considering blennorrhagia as regards the accidents of constitutional syphilis, has been a little more strict. In following the course which I have marked out, we are forcibly brought to confess that the great number of urethral blennorrhagias which do not furnish inoculable pus, were not followed by constitutional accidents.

Among other statistics advanced, I shall cite the most recent, those made last year by M. Lafont Gouzy, who, out of 380 cases of urethritis inoculated, found but two cases in which the inoculation gave any results. One of the two presented, four months later, symptoms of constitutional syphilis.

In this work of M. Lafont Gouzy, he has mentioned two cases in which the inoculation gave no result, and which were, however, followed by syphilitic accidents. We shall have occasion later to explain these exceptional cases.

M. Baumès cites five examples of individuals affected by *simple* blennorrhagias, in which the constitutional infection is nevertheless seen to appear at a later period. From these facts our honorable colleague draws an argument in conclusion, that the blennorrhagia non-symptomatic of chancre, can, like the chancre, produce the syphilitic infection.*

But, first, are all the veroles which have been attributed to blennorrhagia really the consequence of it? If we did not take care of the manner in which statistics were made, we should find, as M. Cazenave and others have, that blennorrhagia is the most frequent antecedent of the constitutional verole, because it is really rare to find individuals who have not had one or more attacks of blennorrhagia. But, when knowing the value of the chancre as a necessary antecedent, we seek what its frequency is, even among the authors where its valuation leaves so much to desire, we find, in the statistics of M. Cazenave, for example, that out of 72 observations, blennorrhagia existed, alone or with buboes, but 18 times, while chancre occurs 38 times. From which M. Cazenave concludes, very logically, as you see, that blennorrhagia is the most frequent antecedent of syphilis. The same results from the summing up of the observations of M. Legendre, and the same logical conclusion follows.

* One of the five patients of M. Baumès had a chancre previously; it is, then to this chancre that the verole of this patient must be referred.

It remains established for science, and in my opinion, that from the statistics of my antagonists even, the chancre visible and avowed by the patient, is still the most frequent antecedent of syphilis. My wards of the Hospital du Midi enclose at this moment 61 cases of well-marked constitutional syphilis; all, without exception, have had chancre as precedent.

Now, in cases where we cannot go back to the pre-existence of a chancre, neither by the recollections of the patient nor by interrogation, what reason is there to deny absolutely the pre-existence of an urethral chancre? You see, then, what we should think of the opinion of M. Cazenave, expressed in these terms, "Far from blennorrhagia never giving place to secondary symptoms, it would appear, on the contrary, to determine them more frequently than the chancre."

You know, dear friend, for it is in your own Journal, that this opinion of M. Cazenave has been warmly approved. M. Vidal (de Cassis) has expressed his sentiments for M. Cazenave in the following manner, which he says is not an academic authority, but which has the advantage of being an authority quite special.

"We know what the position of M. Cazenave is, the vast theatre upon which he makes his observations, his taste for statistics, for all the means, in fact, which, according to my adversaries, conduct to certainty. Well, M. Cazenave *has succeeded in establishing* that the symptom of which the virulence is rarely affirmed before experimentation, is exactly the symptom most virulent, the most infectious, according to observation."

It is true that to prevent M. Cazenave from being too much in a hurry to felicitate himself upon this warm approbation, M. Vidal hastens to add, on the following page:

"However, I do not dare to go as far as M. Cazenave, who, according to my ideas, puts too many syphilitic eruptions to the account of blennorrhagia. Blennorrhagia, in my opinion, is an affection much more contagious than infectious."

That is just my idea, Monsieur Vidal, as you are well aware; only permit me to express my astonishment that it is yours, you who believe that M. Cazenave *has succeeded in establishing* the contrary. I do not wish to insist longer upon this flagrant contradiction, which is, after all, perhaps, but a criticism of conciliation.

As to the cases of blennorrhagia of which the inoculated muco-pus has not given any results, and which have been followed by a general infection, the observations which have been reported of them leave much to be desired, and are, I ask pardon of my learned brother of Lyons, to be received with exceptions. The astonishing credulity, the truly blind confidence of

some physicians, although rendering their works very respectable, are far from carrying conviction into all minds. In these particular cases I do not wish to spare the symptomatology of constitutional accidents, which is incomplete, relatively to some important points, upon which I shall desire to return; I wish, also, that in these cases, constitutional syphilis should really be the subject of inquiry.

I admit that the appearance of these syphilitic accidents agrees, as regards the period, with the time in which blennorrhagia is developed; but are we very sure from this fact alone that the patients have had nothing but blennorrhagia—that syphilis could not have penetrated by another way? My brother physician at Lyons has somewhere said that I denied the possibility of a constitutional syphilitic infection from a simple blennorrhagia, because I had never seen an example of it. It is, on the contrary, because I have seen many patients in whom physicians, who do not think as I do, have recognized but a simple blennorrhagia, where I have found another door for the entrance of syphilis, that my convictions have become more and more profound. When those who maintain that a simple blennorrhagia should give place to the verole, have told you that the patient presented no ulcerations, either upon the genital organs or upon the fingers, they think they have nothing more to exact. They forget the instances without number that the surface of the body presents secret, concealed doors, which close as soon as they are opened, so that the patients are ignorant, or it is for their interest to conceal their knowledge. How many students have come to me from the other hospitals of Paris, in whom nothing but a blennorrhagia has been proved, and in whom I have found chancres in unusual places. While upon this subject, here is a story, analogous to many in my practice.

A lady came to consult me for a disease of the rectum, the symptoms of which, she complained, were those of a fissure. Upon examination I found absolutely nothing about the anus. But the finger introduced into the intestine, discovered, at the height of the superior sphincter, a fissure situated upon the anterior portion and reposing upon a callous surface. I proposed an operation; the patient refused, and I ordered her enemata of rhatania. This treatment had scarcely lasted fifteen days, when in another visit I perceived an exanthematic eruption, having all the characteristics of a confluent syphilitic roseola. Upon farther examination I recognized the swelling of the posterior cervical ganglions. The patient suffered from nocturnal cephalalgia, and already scabs commenced to develop themselves upon the scalp. To me there could be no

farther doubt upon the nature of the accidents. I then examined the genital organs; but I could only perceive a slight uterine catarrh. Interrogated upon the conditions in which this lady could have been placed as regards the contagion of syphilis, she confessed that her husband was diseased, that he had ulcerations on the penis, and that in the fear of communicating them to her, he had had relations with her *a preposterâ venere*. Thus the nature of the fissure was unveiled to me.

In this case is it not true, that without the painful accidents brought on by the fissure, this ulceration would have passed unperceived? It would have then happened that we should have had for the sole antecedent of syphilis, a simple uterine catarrh. But there exists still other causes of error which I wish to point out to you. This will be the subject of my next letter.

Yours, &c.

RICORD.

[To be continued.]

On Contraction of the Knee-joint. By JOHN WATSON, M. D., of New York.

DEAR SIR,—The object of the present communication is to direct your attention to the extension pulleys as a means for overcoming contraction and ankylosis of the knee, and to suggest the propriety of the same apparatus in the treatment of permanent contraction and rigidity at the hip-joint, as well, perhaps, as for breaking up the temporary callus in badly united fractures.

Among the mechanical contrivances now in use for this purpose, as applied to the knee, are—first, the screw clamp, which is secured to the limb in the neighborhood of the affected joint, in such a way as to straighten it, by what has been aptly called the crushing process. Secondly, splints and rollers, also applied in the neighborhood of the joint, with the view of straightening it gradually: and again, in cases of long standing and perfect consolidation of the joint, the excision of a prismoid section of bone, either from the opposing portions of the tibia and femur, or from the shaft of the latter just above the joint. It remains to be proved how far the extension and counter-extension apparatus, adjusted to the ankle and perineum, may be advantageously introduced as a substitute for less certain or more hazardous mode of treatment.

The application of the clamp, though recommended and occasionally employed in Europe, has not met with much favor here. The force being employed directly over the joint by this apparatus, to be effectual in many cases of long standing, must be such as to inflict severe injury on the soft parts; and the

surgeon, not having the entire means of regulating the degree of pressure requisite, may carry this to such an extent as to crush the cancellated structure of the condyles, or injure the nerves and blood vessels in the vicinity of the joint. Splints and rollers, to be of any service, must be of long continuance; and when at last they appear to be in some degree successful, the limb is drawn down by making a second crook in the upper part of the shaft of the tibia, rather than by any yielding at the joint itself. The consequence of this is, that the limb loses considerably in length by making this double curve; and even when straightened, the leg is thrown at some distance behind the proper axis of the femur. The excision of a prismoid section of bone from the front of the joint, is an operation applicable only to the severest cases of confirmed bony consolidation; and though occasionally attended with admirable results, is one of great severity and much hazard, and, as already proved by experience, liable to fatal consequences.

I have taken no pains to inquire upon what authority the extending and counter-extending straps and pulleys may have heretofore been suggested or recommended in such cases, nor am I aware that they have for this purpose been ever before employed. But in the following case, this mode of drawing down the contracted limb was entirely successful; and I am disposed to think that it should hereafter always be borne in mind, as one of the expedients to which the surgeon should resort, before attempting more painful or dangerous proceedings.

Peter Fred, a German seaman, aged 23, was admitted into the New York Hospital, on the 16th of June, 1852, with his left knee flexed nearly at a right angle, and so rigid as not to yield in the least to any force I could apply to it by my own unaided muscular strength. The contracted joint was free from pain, and of the same temperature as the rest of the limb. The patella was firmly glued to the side of the inner condyle by ossific union. The rigidity and contraction had existed sixteen months, and had followed a sub-acute attack of rheumatism, or synovitis, the result of exposure to wet weather in California. The patient, in other respects a healthy man, had been under treatment at an hospital in San Francisco for several months, and subjected to repeated blistering, cupping, and other measures, local and general, without advantage.

On the 19th of June, with the patient on his back and under the full anæsthetic influence of sulphuric ether, with a counter-extending strap to make traction from the perineum, and with the extending band secured to the leg just above the ankle, I had the force of the pulleys applied to the limb, as in luxations of the hip-joint, keeping my hands around the joint, to judge by

feeling of the amount of yielding, and to thus regulate the force of extension necessary for straightening the limb.

As the extending cords became tense, I could feel, and even hear, the adventitious adhesions gradually giving way, until, at length, the patella sprang loose from its attachment to the condyle, and the diseased limb was drawn out as straight as the other. On loosening the apparatus, I now found I could flex and extend the joint as freely as if it had never been the seat of diseased action.

After the operation the limb was adjusted in the straight apparatus, as usually employed for fractures of the thigh; a full anodyne was administered, and an evaporating lotion applied to the knee, to guard against the occurrence of excessive inflammation. For the first twenty-four hours after the straightening of the limb, the patient suffered considerable pain in the joint; but the ensuing inflammation was slight; and on the 2d of July it had so far subsided as to admit of passive motion. The patient soon afterwards began to move about the ward, with a stiff splint secured behind the knee, to prevent any renewed tendency to contraction. On the 29th of July, he was walking about the yard with the aid of a staff; the joint, however, still tender. He remained under observation, using daily embrocations to the joint, and wearing his splint behind the popliteal space, until the 6th of September, at which date he was discharged, with his limb in a position which enabled him to walk with a slight halt, but with some remaining rigidity and tenderness, though with no further apparent tendency to contraction.

A point of much importance in all cases similar to the foregoing, is to determine the time at which the sudden straightening may be attempted with most advantage. While the disease is still acute, and the contraction merely muscular, the limb may be managed by ordinary splints for favoring the straight or, at most, a very slightly flexed position; and, in the sub-acute or chronic state, so long as inflammation persists, any effort at sudden straightening is likely to be followed with a renewal of acute inflammation, and the recurrence of the contraction to as great an extent as ever. The rule, then, should be to wait until the inflammation has wholly subsided, a fact readily determined by the subsidence of pain, but more conclusively by finding the integuments in the neighborhood of the affected joint of no greater temperature than the surface of the skin in other parts of the limb.

An instance somewhat analagous to the foregoing, in which both knees were affected, but in which the rigidity was still confined to the fibrous and other softer tissues around the joints,

occurred to me a few years since, and forcibly illustrates the truth of the remark just made, in reference to the proper time for resorting to forcible extension. The patient was a young man of 22 or 23 years of age. The contraction, in this case also, was the result of a sub-acute attack of rheumatism. The limbs were so drawn up, that the heels were almost made to strike against the nates. The rheumatism in the right knee had entirely disappeared, but in the left there were still some slight remains of the inflammatory process. The contraction and rigidity of the joints had existed six or eight weeks. Finding the right knee not absolutely ankylosed, I at once resolved upon straightening it suddenly; and this I effected in an hour or so, after expending the whole of my muscular force upon it. The limb, once brought down, was secured in the straight position for a week or ten days, after which the disposition to contraction disappeared, and the joint was restored to its healthy functions. Having thus succeeded with the right, I resorted to the same procedure for the left knee; and after still more protracted efforts, I also succeeded in bringing this limb into the straight position. But the operation was an exceedingly painful one. The inflammation returned with greater violence than at first; the patient afterwards entered the medical wards of the hospital, where, notwithstanding every effort to keep the joint straight, it ultimately contracted so as to be of little use to him in walking, and afterwards continued so.

It is remarkable how slight a degree of inflammation in and about the joints may now and then give rise to the most obstinate tendency to contraction. Many instances, if necessary, I could enumerate in proof of this remark. One of the severest of these was that of a young man, who, after an attack of rheumatism, lost the use of almost every joint in his body, large and small, even to those of his fingers and toes, so that he lay for years bed-ridden, and drawn up into a sort of ball. Another instance was that of an elderly lady, who, in consequence of rheumatism, had both elbows permanently flexed and ankylosed. I have at present under observation a gentleman suffering from ankylosis of his right elbow, resulting from the same cause; and a young lady of 17 years of age, in whose case, without any real deformity of the spine, most of the dorsal vertebræ have become consolidated; the patient, in the meanwhile, free from pain, and not conscious of ever having at any time suffered from active inflammation in any portion of the spinal column. Blows and punctures often lead to similar contractions; and where these occur in early life, depriving the limb of its freedom of motion, they lead to arrested development. One instance of this sort, in which the right

arm has ceased to grow in keeping with the left, I have recently witnessed in a young man, who, many years since, fell upon his elbow, and has ever since been troubled with a rigid and contracted joint, now firmly consolidated by bony deposits.

The well-known case of ankylosis of the hip, upon which the late Dr. J. K. Rodgers operated for producing an artificial joint, came on without any sensible injury of the part, and without any evidences of inflammation, while the patient was confined to bed in the New York Hospital, and under treatment for a fracture of the opposite limb. Now, in this case, even as soon as the ankylosis was first detected, mere manipulations with the hand were unable to overcome it. But had the extension pulleys been applied, there is reason to believe that the limb might have been restored to usefulness. The operation of exsection of bone, which was resorted to as the only efficient means of treatment then devised, might thus have been wholly obviated. The patient, though in the utmost jeopardy for many days after the operation, ultimately recovered, gaining little, however, by the operation; for the artificial joint soon afterwards consolidated. But in a more recent case, in which the same operation was employed, the result was disastrous. It is not probable, however, that in this latter case, where the ankylosis was of many years' duration, the extension pulleys would have proved available. The use of these for ankylosis at the hip, would, *a priori*, appear to be safe only while callus is yet nascent, and before the consolidation has become so firm as to jeopardize the bony structure of the acetabulum in attempting to overcome it.

The pulleys may also be substituted, with advantage, for the more common mode of breaking up the recent callus in badly united fractures. It has been shown by Dupuytren, that the formation of the permanent callus after fractures, is completed only after an interval of many months; and yet after an interval of three weeks, I have known the temporary callus in a badly united fracture of the thigh, so very firm as to resist the best directed efforts to break it up by mere lateral force. I remember to have once assisted my friend and former colleague, Dr. Hoffman, in overcoming a similar deformity after fracture of the leg. In this instance, after etherizing the patient, the rigidity was overcome by the union of direct pressure over the seat of accident with extension at the heel. The pulleys, however, would probably have been an easier and more expeditious mode of management.

Surgery, as well as medicine, has its fashions; and while we are ready to apply the extension pulleys to new uses, we are learning to dispense with them in some of the very accidents

for the cure of which they were originally introduced. Our celebrated countryman, Nathan Smith, about the commencement of the present century, as I learn from one of his early pupils, Dr. Batchelder, of this city, as well as from the memoirs of him prepared by his son, Dr. N. R. Smith—was in the habit of pointing out to his students a method of reducing luxations of the femur on the dorsum of the ilium by merely flexing, adducting or abducting, and rotating, the thigh upon the pelvis. Others, both in this country and Europe, have since the time of Nathan Smith, been accidentally successful in a few similar cases. In Germany, since 1823, the subject has been more carefully studied and systematized by Colombat, Wattmann, Kluge and Rust; and in our own State, within the past year, by Dr. W. W. Reid, of Rochester, whose process, as described in the last volume of the Transactions of the New York State Medical Society, is nearly identical with that of the latter writer. I am happy to say that the attention which Dr. Reid has anew attracted to this subject, is likely to make the flexing, adducting, abducting and rotating process, the general mode of treatment in all recent luxations of the head of the femur backwards; and that, at the New York Hospital, two, if not three cases have already been reduced, without the aid of pulleys, by this method.—[*New York Medical Times*.

On Bright's Disease, and its Connection with Puerperal Convulsions. By Dr. LITZMANN, (*Deutsche Klinik*.)

Prof. Litzmann, of Kiel, has published an elaborate memoir on renal disorder in pregnant and puerperal females. He commences by describing twelve cases in which albuminuria and œdema were present; some of the women had convulsions. Three of the patients were pregnant with twins; and nine (including the twin cases) were primiparæ.

In several of the cases, especially where convulsions were present, or were threatened, or where there were symptoms of toxæmia, as amaurosis, etc., carbonate of ammonia was discovered in the blood and in the air expired from the lungs, and in the blood of the children who were born dead. This was in accordance with the view of Dr. Frerichs, that urea, when accumulated in the blood, becomes converted into carbonate of ammonia, and then produces toxæmic symptoms.

Including the twelve cases described, Dr. Litzmann has examined the urine of 131 persons; 79 during pregnancy, 80 during labor, and 80 after parturition. In these he has found albumen present in 37, and absent in 95. The examinations

began, with few exceptions, in the last three months of pregnancy; in almost all cases they were repeated several times, and in several daily; yet it is possible that small quantities of albumen may have escaped notice. In order to avoid the admixture of foreign matters from the vagina, the catheter was used always with parturient women and those who had been delivered, and frequently in pregnant females. In the latter, as well as in those who had been confined, the urine passed in the morning was employed. Heat, and acetic, or nitric acid, were the tests used.

Of the 95 females, whose urine contained no albumen, 53 primiparæ and 42 multiparæ. Of the 37, who had albuminuria, 26 were primiparæ and 11 multiparæ; among the latter was one who had albuminuria in her first pregnancy, and two who were pregnant with twins.

Among the 37 females, the urine of 16 was discovered to be albuminous during pregnancy. In ten of these the albuminuria continued during labor, and for some days afterwards; in four, where it was less intense, it disappeared before confinement; and in two cases, circumstances prevented the examination from being continued.

In four women who had been confined, and whose urine contained albumen, no examination had been made during pregnancy. There can be no doubt, however, from the quantity of albumen found, and the other symptoms, that it had been present.

In four females, in whose urine no albumen had been found during pregnancy, albuminuria appeared during labor—in two evidently for the first time.

In ten persons, in whose urine no albumen had been present during pregnancy, there was considerable albuminuria after delivery. In eight of these, the urine, examined during labor, was found to contain no albumen.

In three females who had been confined, and in whose urine albumen was found, no previous examination had been made.

There is a form of albuminuria unconnected with renal disease, but arising from catarrhal irritation or blennorrhœa of the bladder. Dr. Litzmann has observed it twice during pregnancy, several times during labor, but most frequently after delivery. It generally appears on the second or third day, and goes on increasing till the sixth or seventh. The quantity of albumen is not great, and is connected with the presence of purulent mucus. At first, the urine after delivery is high-colored, of acid reaction, and contains a large quantity of urates and uric acid, with epithelium and a number of pus or mucous globules. It afterwards becomes lighter and yellow in color,

slightly turbid, contains less urates, and is less acid; on long standing, it deposits a sediment of pus corpuscles with more or less epithelium. Fibrinous casts of the uriniferous tubes are never present. In only two cases was the quantity of pus in the urine sufficient to be detected by the naked eye: these were more chronic than most of the others, recovery not being effected till the eighth week.

In both these cases, which Dr. Litzmann relates, there had been, during labor, long continued pressure on the urethra and neck of the bladder; and this may have acted as the proximate cause of the disease. Yet cases very often occur, in which the same causes do not produce such effects. In most of the women affected with vesical catarrh after delivery, the disease could not be attributed to the above cause, the labors having been regular, and even easy. Dr. Litzmann is inclined to believe that slight vesical catarrh, which may not cause albumen to be present in the urine, but which may furnish a sufficient quantity of pus or mucous corpuscles to be detected with the microscope, is of not unfrequent occurrence after delivery. During and before labor the disorder is much more rare. When it occurred during labor, this had almost always been tedious, without, however, in general producing retention of urine. One of the pregnant females who were affected had rigors, and was at the same time seized with rheumatic swelling of the hands. In another, the vesical catarrh was brought on in the fourth month of pregnancy, by retroversion of the uterus. The local symptoms were generally very slight; on being questioned, the patients would acknowledge a little uneasiness or sense of heat in the bladder; but the state of the urine was generally the only diagnostic symptom. Of the 37 cases of albuminuria which Dr. Litzmann observed, he believes that half were referrible to catarrh of the bladder: this he ascertained in nine of the cases, by microscopic examination.

Simple albuminuria, and Bright's disease, (albuminuria accompanied by fibrinous exudation into the uriniferous tubes), pass gradually into each other in pregnant females. In most of the cases in which albuminuria had reached a high degree, fibrinous casts were found towards the end of pregnancy, or during labor and the early part of the subsequent period. Of the 37 cases of albuminuria, Dr. Litzmann found 13 to be connected with Bright's disease: in 7, fibrinous casts were found: in one, the urine could only be examined once, and, though not then found, they were probably present: and in five, there could be little doubt from the quantity of albumen and the concomitant symptoms—eclampsia in three cases—that they were present, although the urine was not examined microscopically.

There can be no doubt that, as has already been pointed out by Rayer, the albuminuria of pregnant females arises from a mechanical obstruction to the renal circulation. From the experiments of Frerichs, it appears that the retardation of the venous circulation on the kidneys easily produces exudation of albumen and fibrin, and finally, even of blood. On the other hand, increased arterial impulse, as from ligature of the aorta below the renal arteries, only rarely gives rise to slight albuminuria; and it is when one kidney is extirpated at the same time with the ligature of the aorta, that any notable quantity of albumen appears in the urine. In favor of the mechanical view of the explanation of albuminuria in pregnant women, may be adduced its predominance in primiparæ—a fact recognized by all observers. The tight and unyielding abdominal wall must naturally cause the uterus to press more strongly on the organs lying behind and above it. It is moreover probable that when albuminuria has occurred in multiparæ, it has been but a repetition of the same affection which they had as primiparæ.

In many of the cases, there were also other causes which tended to increase the pressure. Five of Dr. Litzmann's patients, who had albuminuria, were pregnant with twins; in others, there was a large quantity of liquor amnii, or a large child, or both; in one case, there were periodical contractions of the muscles, especially of the recti, pressing the uterus against the spinal column; in four cases, the pelvis was narrow. Three of the patients had chronic pulmonary catarrh. It cannot, however, be denied, that cases are often met with, in which all these circumstances are present without producing albuminuria; and, on the other hand, that albuminuria sometimes occurs, even in a high degree, without more than ordinary pressure being apparently exerted. An additional ground for assuming the dependence of albuminuria in pregnant women on mechanical impediment to the circulation, is found in the rapidity with which it disappears as soon as, by emptying the uterus, the free circulation of the blood has been re-established.

Besides the mechanical cause, we must take into account the state of the blood in pregnant women (increase of water and fibrin, decrease of albumen, diminution of the red, and increase of the colorless particles). Most of the pregnant women with albuminuria, observed by Dr. Litzmann, had a more or less chlorotic appearance; while others appeared fresh and healthy. The state of the blood did not appear sufficient to cause albuminuria, without an impediment to the circulation of the kidneys. In common with Lever, Devilliers and Regnault, etc.,

Dr. Litzmann has not been able to recognize cold or the abuse of spirituous liquors, etc., as the cause of albuminuria in pregnant females—[*London Jour. Med.*

Intussusception of the Bowels. By DANIEL BARBER, M. D., of New Richmond, Ohio.

GENTLEMEN—Believing that the following plan of treating intussusception of the bowels is not familiar to the minds of many of the profession, I submit to you a case in which it was successfully applied.

The subject was a young man aged twenty years. He had two attacks of colic within ten days. Constipation followed immediately upon the last. He was treated for four days with purgatives—warm water injections—bleeding, &c., without any effect. At the end of this time (Nov. 13) I was called in consultation with Dr. Bennett, of Withamsville, the attending physician. I found the case as follows: Pulse 120, abdomen tympanitic, and tender to the touch, extremely severe paroxysmal pain of the bowels, frequent vomiting of highly offensive matter, obstinate constipation.

To subdue the tendency to peritoneal inflammation, we repeated the bleeding and administered the sulphate of morphine, in half grain doses every two hours until he was brought fully under its influence. When I returned on the evening of the 14th, the pulse had fallen to 96—the tenderness and pain of the bowels were materially diminished—the vomiting less frequent and distressing—constipation continued. Frequent and large quantities of warm water has been continued to be injected. At my suggestion the following plan of treatment was now adopted.

We procured a small quantity of brewer's yeast, from which was prepared in the usual way a quantity sufficient for our purposes. At about midnight, we gave a tumbler half full, and ordered the same quantity to be repeated once or twice every hour.

On the afternoon of the 15th, when it was obvious from the quantity taken and retained, that the intestines above the obstruction, were distended with carbonic acid gas, the colon was likewise inflated with atmosphere by means of a pair of fire bellows.

By these means combined, the intestinal canal throughout its whole course was inflated, and the obstruction reduced.

At nine o'clock in the evening, a copious evacuation of the bowels ensued, followed by several others during the night. At the same time the explosions of gas were so violent as to be

heard at some distance from the house—it was literally keeping up a regular fire. The patient was at once relieved, and speedily recovered his former health. Besides the distending force of the gas, it is very probable that it exercises a beneficial influence by its sedative and antiseptic properties.

I believe this practice originated with the French, but to what extent it has been applied I know not. Dr. Johnston and Rogers, of this place, have given yeast in two or three cases of this disease during the course of their practice here, with success. In one case relief was afforded on the fourteenth day of the attack, after every other means had been tried and failed.

From the above facts I should feel disposed to give this plan a trial in every case, where the ordinary means fail. Should relief not be obtained in a reasonable length of time, and the case be protracted, and as it were hopeless, a moderate exhibition of the yeast, by its antiseptic properties and by gently exciting the peristaltic action, would afford perhaps the best prospects of success.—[*Western Lancet*.

On Syrup of Assafetida. By RICHARD PELTZ.—(From an Inaugural Essay.)

Assafetida has long held a high rank as an antispasmodic and expectorant, but has not been used as extensively as its virtues seem to demand, both on account of its very unpleasant odor and acrid bitter taste. It consists chiefly of resin, gum and volatile oil.

The Pharmacopœia of the United States recognises as official two fluid preparations of assafetida—the tincture and the mixture. These, although very good preparations, are both objectionable on account of their unpleasant taste. This, I think, could be remedied in part by making a syrup: with which purpose I have undertaken a number of experiments, to ascertain if a preparation could not be made which would contain in a more agreeable form all the medicinal properties of assafetida soluble in water.

Having tried a number of methods I concluded that the following formula would make the most perfect preparation; Take of assafetida, an ounce; boiling water, a pint; sugar, two pounds. Triturate the assafetida in a mortar with a portion of the boiling water until a uniform paste is formed, then gradually add the remainder of the water, strain and add the sugar, applying a gentle heat to dissolve it.

Much heat should be avoided in forming the syrup, as the volatile oil, which is present in considerable proportion in good assafetida, (nearly 6 per cent.,) a portion of which is dissolved or held in suspension by the water, would be dissipated.

There is an advantage in using boiling water, as it takes up more of the gum-resin, which is permanently dissolved or held in suspension by the sugar.

This syrup, when first made, is nearly white, but upon exposure to light gradually assumes a pinkish tinge. It is of the same strength as Mixture of Assafetida of the Pharmacopœia, and owing to its being more pleasant to the taste, might with advantage be given as a substitute for that preparation, especially in cases of children, as the sugar conceals much of the unpleasant acrid taste of the assafetida.

This syrup is much more permanent than the mixture, having kept some for several months without any apparent change, except in color, owing to the action of the light on the resin; while the mixture kept in the same situation became very unpleasant in a short time. It also has an advantage over the tincture in being entirely free from alcohol, which sometimes is objectionable.—[*Am. Journ. of Pharmacy*.

On Glycerin Ointment. By JOHN H. ECKY, M.D., Philadelphia.

DEAR SIR—I send you a formula for an ointment which I have found very useful for chapped hands, lips, excoriations of the skin, &c., &c. I have called it Glycerin Ointment.

R Spermaceti,	℥ss.
White wax,	℥j.
Oil of almonds,	℥ij. (f.)
Glycerin,	℥j (f.)

Melt the wax and spermaceti with the oil of almonds at a moderate heat; put these into a wedgewood mortar, add the glycerin, and rub until well mixed and cold. I have used this ointment in my own family, and distributed much of it among my friends, and can testify to its value.

I am not aware that an ointment of which glycerin forms a part has been proposed, and yet few physicians are ignorant of its property of protecting and soothing inflamed surfaces of the skin and mucous membrane. To such, however, as are unacquainted with its valuable properties, I would refer them to the article on glycerin, in the last edition of Wood & Bache's Dispensatory.—[*Ibid*.

Solution of Gum Shellac in Alcohol.

Since Professor Dugas's notice of gum shellac in alcoholic solution as a valuable external application to arthritic joints. I suggested it to a patient who has long been a sufferer with chronic rheumatism, and learned that at least four years ago

he applied it on the recommendation of a physician, and with great relief for the moment. Finding it to fail after a while, he tried what he termed "*a better coating*" for the joints, which was a fresh egg beaten up with salt and spirits turpentine. This he found more impermeable than the former, but like it was temporary in the relief it afforded.—[*New Orleans Monthly Medical Register*.]

Sulphuric Acid in Diarrhœa. By JOHN L. VANDERVOORT, M.D.

Diarrhœa, as met with in young children, especially during the heat of summer, not unfrequently proves a troublesome and intractable disease, resisting the antacid and astringent treatment so commonly resorted to. During the past summer it was unusually prevalent throughout the city, the stools being frequent, and of a mucous or watery character, accompanied by little pain, except when the intestines were distended with flatus. Failing with the remedies usually employed, I resorted to the use of sulphuric acid, as suggested by several London and provincial physicians. It was first given in an obstinate case at Yorkville; the child was teething, and was naturally robust and healthy. For several weeks he had had more or less looseness of the bowels, with occasionally slight vomiting, and discharges of mucus, tinged with blood. Strict regimen and minute doses of blue mass and opium failing to exercise more than temporary effect upon the disease, and observing that his gums had become spongy and disposed to bleed upon the slightest touch, I changed his treatment, and gave him four drops of the acid in a wineglassfull of sweetened water several times a day. Seeing him again in a couple of days, I found him much improved; his discharges were less frequent; there had been no more vomiting or bleeding from the gums. The same treatment was continued for a week, when the child's health was quite restored. In several other cases of similar character, the acid was given, and with like happy results; and within a few days an infant, which had been troubled with excessive looseness of the bowels for nearly a week, was cured by a few doses.

One very great advantage which this remedy has over those in general use, is its agreeable taste, resembling in this respect lemonade; hence it is well adapted to children whose aversion to medicine cannot readily be overcome.

In a late number of the *Provincial Medical and Surgical Journal*, is a paper on this subject, by Mr. Sheppard, in which he alludes to upwards of fifty cases of diarrhœa, many of them very severe, in which he had used the sulphuric acid; in only

one instance did it fail, and in that case the chalk and astringent treatment was also unsuccessful. His experience led him to the following conclusions:—

1st. It is more *efficacious* than alkalies, opiates, and astringents, in a proportion greatly exceeding ten to one.

2nd. It is *more rapid* in its action (especially in children,) in a proportion greatly exceeding twenty to one.

3rd. It seems to act in a more rational and (if I may so express myself) scientific manner, by increasing the *tone* of the mucous membrane of the alimentary canal, rather than by simply astringing its pores.

4th. The worse the case, the more rapid and marvelous seems to be the cure; a most striking feature, as compared with the treatment by chalk and opium.—[*N. Y. Medical Times.*

Psoriasis Palmaris cured by Iodide of Arsenic. By J. F. SANFORD, M. D., Professor of Surgery in the Iowa State University.

Mr. J. Reed, of this city, merchant, aged 38 years, of sanguine temperament and vigorous constitution, applied to me, some time during the past July, on account of a disagreeable and troublesome affection of the hands and feet, with which he had suffered for the previous five years. Upon examination we found the palmar surface of both hands, wrists, and the heels of both feet covered with thick dense epidermal scales, very dry and hard, with deep fissures or cracks extending in the direction of the natural furrows of the palm and fingers. The hands and feet were extremely stiff and painful, and any attempt to use them suddenly, was attended with an increase of pain and bleeding from the cracked portion of the diseased surfaces. There was also a very troublesome pricking or tingling sensation in the parts and a burning or itching that much annoyed the patient. The eruption in question made its appearance by one or more inflamed and painful spots, of a dull red color, which extended in various directions, frequently coalescing. After a short time these patches became covered with scales, which increased in number and thickness until the epidermis seemed thick and hard as leather, and cracked as above mentioned. Occasionally when these inflamed spots occurred in exposed situations, they would suppurate and thus augment the suffering and discomfort of the patient. The discharge from these little abscesses was thin and sanious, and after its escape the spots seemed disposed to heal without those further changes, described as occurring when suppuration did not take place.

Inquiries were made relative to the influence of season upon

the development of the disease, but it did not appear that it underwent any great change at any particular time. The eruption had been constantly present, to some extent, for five years, and although partially disappearing occasionally, these periods of partial exemption had no connection, as far as the patient was aware, with the season or with his habits of life.

He had applied to numerous physicians, from whom he had doubtless taken the usual remedies, and had also used various popular medicines, but without relief.

The *cause* of the disease, in this case, was obscure. There was no constitutional indisposition, nor any thing in the patient's occupation or habits, that could act as a local irritant in its production. His general health was remarkably good; he had not suffered a functional disturbance for months, and presented, when I saw him, otherwise than in the respect mentioned, a perfect physiological condition.

I commenced the treatment by ordering him to drink freely of cream tartar, until the bowels were moved, and to apply at night a bread and milk poultice to the diseased parts. The poultices, which he had frequently applied, always relieved the heat and pricking of the parts, and softened the thickened epidermis, so that he could scrape away considerable portions after its removal. During the day he kept a cloth to the feet, wet in a strong solution of the Bicarb. of Soda, and frequently washed the hands in the same.

I was led to the employment of Iodide of Arsenic, by accident. I had intended, after a brief preparatory treatment, to place the patient on a more energetic course, and had thought of the *Liquor Hydriodatis Arsenici et Hydrargyri*, as the medicine to be used, as recommended by Dr. Graves, of Dublin. Finding myself unable to procure this at our drug stores, and having much confidence in the combination of Iodine and Arsenic in the treatment of various cutaneous diseases, I resorted to the Iodide of Arsenic.

Eight grains of this medicine were dissolved in four ounces of distilled water. Of this solution, twenty drops were given three times a day, in a few drams of sweetened water. At the same time, fifteen grains of the same medicine was thoroughly incorporated with one ounce of simple cerate, a small portion of which was well applied to the diseased surfaces every morning after the removal of the poultice. The parts were thus in a soft state, admitting, without pain, the removal of the thickened cuticle, after which they were in good condition to receive the ointment.

This treatment was followed by the most decided and happy results. In two weeks the patient was very much improved,

and in six weeks from the time it was commenced, no vestige of the disease remained. Two months have elapsed since the cure appeared complete, and there is no return of the disease. This being the first thorough disappearance of the eruption in five years, together with its exemption from the modifying influences of the season encourage me to believe that the cure is permanent.

The prompt and decided effects of the Iodide of Arsenic in a case calculated to test its curative powers, justifies the inference that it is a remedy of peculiar efficacy in the treatment of the various forms of Psoriasis. No disagreeable local or constitutional effects followed its use.

I am at this time employing the same remedy in a protracted case of Pityriasis, with the prospect of a similar happy result.
[*Western Medico-Chirurg. Journal.*]

On the Treatment of Chilblains. By M. TROUSSEAU.

M. Trousseau washes all parts affected with chilblains, three times a-day, with the following lotion:—Borax, 50 parts; water, 500. Four table-spoonfuls are added to a quart of water. He also prescribes, both for the prevention and removal of chilblains, the following lotion, to be used night and morning:—Sal ammoniac, 20 parts; water, 40; proof spirit, 10. When ulceration has occurred, he prescribes one of the following formulæ:—Tannin, 10 parts; water, 500. Or, ext. rhatany, 10 parts; quince mucilage, q. s. This is mixed up as a soft electuary, with which the parts are smeared; and the application is also an excellent one for the cracked lips which occur in the winter.—[*L'Union Med. Med. Chir. Rev.*]

Miscellany.

Aromatic Schiedam Schnapps.—The sale of Holland gin, neatly put up in medicinal bottles and labelled as above, accompanied with the highest encomiums of its wonderful efficacy in innumerable diseases, is one of the most cunning and nefarious devices we know of for leading the unwary into the vice of intemperance. We have lately noticed it repeatedly upon mantel pieces, along with "other vials of physic," and apprehend that it will become one of the most popular nostrums of the day. It is said that one of its greatest recommendations is that "it is not bad to take."

Dr. Drake's work on the Diseases of the Interior Valley of North America.—We are happy to learn that Dr. Drake had prepared a considerable portion of the second volume of this great work, and that it is the desire and intention of his children that the portion so prepared should not be lost to the Profession. It is to be revised by a competent person and printed as soon as possible.

Ligatures of large Arteries, by Prof. ROUX.—M. Roux, the Nestor of French Surgery, occasionally furnishes the statistics of his extensive experience. In a paper communicated to the Chirurgical Society we find that he has ligated the popliteal artery once, the femoral 46 times, the brachial 20 times, the carotid 6 times, the axillary 4 times, the sub-clavian 3 times, and the external iliac twice—making 82 operations.

Ink for the Million.—To the Editor of the American Journal of Pharmacy: The following formula for making a very *superior ink* is not generally known. The facility of its preparation, and its almost incredible cheapness, (about two cents a gallon,) render it worthy a place in your Journal.

R. 12 oz. avoird. Ext. Logwood,
 $\frac{1}{2}$ oz. “ Bichromate Potash.
 5 gallons water;

Dissolve the ingredients separately in water and mix them together, and in a short time the ink will be fit for use.

An analysis of the above would be very desirable.

As an instance of the very great coloring property of hæmatoxylon, I have found that 1-100th of a grain dissolved in 4,000,000 times that quantity of water, will be tinged a fine pink color by the addition of a little aqua ammonia.

Yours truly,

Philadelphia, Sept. 13, 1852.

W. H. PILE.

Electricity.—Professor Faraday has instituted a series of experiments, with a view to determine the quantity of electricity connected with the atoms or particles of matter. He says, it is wonderful to observe how small a quantity of a compound body is capable of being decomposed by a certain quantity of electricity. “One grain of water will require for decomposition, an electric current equal to a very powerful flash of lightning.” The chemical action of a grain of water upon four grains of zinc, can evolve electricity equal in quantity to that of a powerful thunderstorm; and he states, that from his experiments it would appear, that 800,000 such charges of the Leyden battery would be necessary to supply electricity sufficient to decompose a single grain of water. The Leyden battery of which he speaks consists of fifteen jars, containing 3510 square inches, or

about twenty-four and a half square feet of coated glass, charged by thirty turns of a plate electrical machine, the plate being fifty inches in diameter, and of immense power, giving ten or twelve sparks an inch long for each revolution. The author of a paper on this subject in the *Philosophical Magazine*, remarks, that "the estimate that 800,000 discharges of the battery of fifteen jars, equal to a powerful flash of lightning, would be necessary to resolve a single grain of water into its elements, is certainly astounding, when it is recollected that, according to Professor Faraday, the quantity of electricity that decomposes a body, is the equivalent quantity of electricity that had previously held the elements of that body in combination; for he, with Davy and others, conceives that electricity and chemical affinity are identical powers. Hence in one grain, that is, one drop of water, there must be naturally existing, and constituting the affinity between its oxygen and hydrogen, no less a quantity of electricity, than 800,000 charges of a battery containing 3510 square inches of coated glass, or the equivalent of a very powerful flash of lightning. If this quantity of electricity were converted into one spark, it would be 4166 miles in length, taking Professor Faraday's mean estimate of one charge of his battery as the basis of calculation.—[*Annals of American Pharmacy*.

Muscular power of the Insane, by M. MOREL.—A general popular error prevails, that the insane are endowed with inordinate muscular power, and this explains, why so many persons are brought to the Mareville Asylum tied and corded. When M. Morel was first appointed to this, he found numerous patients bound up, reputed dangerous, and especially so, because of their vociferations. He set them at liberty without any ill effect, and attributes much of the violence that had previously occurred to the ill conduct of the attendants. He agrees with Jacobi, that as a general rule, the insane exhibit no inordinate muscular power, and some of the patients of almost colossal stature are easily managed by one person. Indeed, the insane when engaged in manual labor, soon tire and require frequent repose. If some of them by exception, work with a feverish activity, and display great strength, the majority are dejected and languid. The persons in whom he has met with the greatest developement of muscular power, belong to the following categories. 1. Persons of small stature, delicate complexions, and nervous temperament, and especially females who appear exhausted by their cries and agitations. Among such miserable looking beings, a power of resistance is developed under certain circumstances, which defies the united energies of several attendants. 2. Insane epileptics. 3. Monomaniacs who are not yet exhausted by the disease, or irrational treatment. When their passion is opposed, these persons sometimes manifest a resistance only to be overcome by several attendants.—[*British and Foreign Medico-Chirurgical Review*, from *Annales Medico-Psychologiques*.

BIBLIOGRAPHICAL.

Principles of Human Physiology, with their chief applications to Psychology, Pathology, Therapeutics, Hygiène and Forensic Medicines. By WILLIAM B. CARPENTER, M. D., F. R. S., F. G. S., &c. Fifth American, from the fourth London edition. Philadelphia: Blanchard & Lea. 1853.

The previous editions of this work have exhibited progressive improvement. Each has been better than its predecessor, more accurate and more elaborate. The rapid sale of it is but one of the evidences of the high estimate placed upon it by the profession.

In order to justify this favorable opinion, to keep pace with the advance of the science and fully to present his more matured views of it, the author has remodelled his former work, entirely re-writing many of the most important chapters and adding several others upon topics not embraced, or imperfectly discussed in former editions.

The changes are too numerous to admit of an extended notice in this place. At every point where the recent diligent labors of organic chemists and micrographers have furnished interesting and valuable facts, they have been appropriated, and no pains have been spared in so incorporating and arranging them, that the work may constitute one harmonious system. These alterations, although they have greatly enhanced the value of the book, have unavoidably added to its bulk, until it is no longer well suited to the purposes of the student. Its appropriate place is the library of the physician. The present work, and the "*Principles of General and Comparative Physiology*," by the same author, form incomparably, the best treatise on Physiological Science in the English language. M....r.

The Druggists General Receipt Book; comprising a copious Veterinary Formulary and table of Veterinary Materia Medica; numerous Recipes in Patent and Proprietary Medicines, Druggist's Nostrums, etc.; Perfumery and Cosmetics; trade, chemicals, &c., with an appendix of useful tables. By HENRY BEASLEY. Second American, from the last London edition, corrected and enlarged. Philadelphia: Lindsay & Blakiston, 1853, 12 mo., pp. 472.

This work is not intended for the Physician, and how far it will meet the wants of veterinarians and druggists, we are unable to say. It contains a vast number of receipts, which may be found useful on some occasions. The pages devoted to patent and proprietary medicines is of some interest to the physician, as it affords him a knowledge of the composition of many of the nostrums which are in frequent use in domestic practice. G.

Materia Medica or Pharmacology and Therapeutics. By WILLIAM TULLY, M. D. Vol. 1, No. 1., November, 1852. Springfield: George W. Wilson's Power Press. 1852. 8vo. pp. 64.

This work is to be published in monthly numbers, until it is completed. Its author has been long known to the profession, as a gentleman of considerable ability who has devoted much attention to the subject of which he treats. The work is to be "original, having none of the characteristics of a compilation." A proper estimate of the work cannot be formed from an examination of a single number; but if Dr. Tully's opinions upon other questions are as erroneous as those he advances on the subject of the absorption of medicinal substances, it is not probable that the profession generally will concur with a friend of Dr. Tully, who predicts that this work "will be the most original philosophical and practically useful book for the physician which has appeared since the writings of Sydenham."

It is supposed that about twenty numbers will complete the work. The price is twenty-five cents for each number. G.

The Physicians Pocket Dose and Symptom Book; containing the doses and uses of all the principal articles of the *Materia Medica*, and chief officinal preparations, etc., etc. By JOSEPH H. WYTHES, M. D., author of the "Microscopist," etc. Philadelphia: Lindsay & Blakiston. 1853. 1 vol. 12 mo., pp. 246.

The title of this little work very clearly indicates its character. The author supposes that "it might prove of service in the treatment of disease," and that it will prove acceptable to the "country practitioner." We are sick of the frequent disparaging references to country practitioners. Surely as high a degree of intellect and professional knowledge is required for country as for any other practice; and we are fully persuaded that our country practitioners generally, have as little use for "vade mecum" and "pocket remembrancers," as any other class of practitioners. If such a work as this is at all useful, it will be to those students of medicine who are "cramming" for an examination for which they do not feel exactly prepared. G.

Hand-Books of Natural Philosophy and Astronomy. By DYONISIUS LARDNER, D. C. L., &c. &c. Second course. Heat—Magnetism—Common Electricity—Voltaic Electricity. Illustrated by upwards of 200 engravings on wood. Philad.: Blanchard & Lea. 1853.

This is one of those admirable works, for the construction of which Dr. Lardner has such peculiar talent. It presents a condensed, yet lucid embodiment, of the present advanced state of knowledge, upon

subjects that must ever be interesting to the admirer of nature's wonderful manifestations of the greatness and wisdom of the Creator.

Braithwaite's Retrospect of Practical Medicine and Surgery. Part 26.—1853.

This number sustains the well-established reputation of the work ; but, like its predecessors and its rival, Ranking's Abstract, it is almost bare of American intelligence.

New Medical Journals.—We have received the first issue of "The Southern Journal of the Medical and Physical Sciences," edited by Drs. Jno. W. King, Wm. P. Jones, R. O. Curry and B. Wood, and published bi-monthly at Nashville, Tennessee. It is full of interesting matter and handsomely printed.

We have also received the Medical Recorder, published bi-monthly by the Memphis Medical College, edited by Professors Merrill and Quintard. It contains only 16 pages, but verifies the axiom, that "good things are put up in small packages.

We wish our confrères a full tide of success—and have added them to our list of exchanges.

In addition to the above works, we are indebted to the authors and publishers for the following pamphlets :—Professional Reminiscences of Foreign Travel, by W. Channing, M. D., of Boston. The Inaugural Address of Worthington Hooker, M. D., &c., &c., upon "the present mental attitude and tendencies of the Medical profession." History of the Medical Department of the University of Louisville : an introductory Lecture, delivered by Prof. L. P. Yandell. A popular Address, by Dr. W. F. Barr, before the Medical Society of East Tennessee. Proceedings of the National Pharmaceutical Convention, held at Philadelphia, in Oct., 1852. Congressional Report of Hon. E. Stanley, of North Carolina, and Hon. Alex. Evans, of Maryland, on the Ether discovery. Congressional Report of Hon. W. H. Bissell, of Illinois, Chairman, upon the Memorial of Dr. W. T. G. Morton asking remuneration for the discovery of the anæsthetic properties of Sulphuric Ether. Discovery of the late Dr. H. Wells, of the applicability of the Nitrous Oxyd Gas, Sulphuric Ether and other vapours, in surgical operations, &c. And a number of useful catalogues from the large publishers of medical and scientific works.

Erratum.—On p. 17 of last No., line 18 from top, read *ter-chloride*, instead of "too, chloride".

SOUTHERN MEDICAL AND SURGICAL JOURNAL.

Vol. 9.]

NEW SERIES.—MARCH, 1853.

[No. 3.

PART FIRST.

Original Communications.

ARTICLE VIII.

Remarks on an Operation for the Radical Cure of Hernia.

By W. H. ROBERT, M. D., of Orion, Pike Co., Alabama.

As the subject of Hernia is at present attracting a great deal of attention from the learned and skilful of the day, perhaps a relation of my experience in the treatment of this disease may not be amiss.

In 1846, a Dr. Woozencraft (probably the same now in California as Indian agent) passed through Georgia, offering to sell an Instrument, and the right to operate with it, for the cure of Scrotal Hernia. The instrument was a very small silver syringe, whose canula was about an inch long, and terminated in a trocar-shaped steel point. Two small orifices existed in the side of the canula near the steel point, from which the fluid contained in the syringe made its exit. This instrument would probably have held from 30 to 50 drops of fluid. Dr. Woozencraft's manner of operating, was—to purge the patient freely a day previous, and give a large anodyne in the morning to quiet the bowels; a good truss was then put around the body to be ready for application; the intestines were returned by taxis, and the instrument (previously filled with the oil of cloves, or tinct. cantharides) was passed in the canal just at its junction with the scrotum. To ascertain if any tissue was

intervening, and to be sure that the instrument was at least at or near the canal, the point was made to move about in various directions; one finger of the unoccupied hand was pressed upon the point of the instrument to ascertain if all was right; the fluid was then forced up to the canal, and the instrument withdrawn. The truss was immediately applied to prevent, by its pressure, the descent of the intestines. In about twenty-four hours there would commence a deposition of plastic lymph, which, Dr. Woozencraft argued, would cause an agglutination of the parietes, or an impaction of the inguinal canal.

As the price demanded by Dr. Woozencraft, for an instrument and the right of a county, was so exorbitant, I did not *bite* at that time, but waited a better opportunity. A few months after, I purchased an instrument from a man who had them for sale, probably as agent. My instrument differs some little from the one used by Dr. Woozencraft in having a lancet point.

Having taken notes at the bed-side of the first patient upon whom I operated, it is the only one I shall give in detail.

CASE 1. Burrell, a negro man, aged about 74 years, in fine health; he has been ruptured twelve or fifteen years. April 15, 1846, I operated on him by injecting at the external ring about 20 drops of the oil of cloves, in the presence of Drs. Ogilby, Jones, Wingfield, Howard, and Anderson, and Messrs. Wright and Man, students of medicine. He complained very little of the operation; after which, I applied one of Chase's trusses, to prevent the descent of the intestines.

16th, 10 o'clock, A. M. Patient had vomited some, which caused the descent of the intestines. I returned them very easily. Until now, he had not complained in the least of pain. 12 o'clock, M.—Complains very much of pain in the parts, which are swelled, tense and very painful to the touch. So far, the swelling is in the bottom of the scrotum. 7 o'clock, P. M.—Inflammation gradually progressing towards the external ring; he has not vomited since morning. I have allowed him small quantities of whiskey to-day, as he has been accustomed to the daily use of it. Pulse natural.

17th, 8 o'clock, A. M. Patient complains very much of the inflamed parts—they appear to be assuming the condition I

should think necessary to the radical cure of hernia, that is, the deposition of a large quantity of plastic lymph; he has not vomited since yesterday, and then only once; appetite good, and pulse natural. 6 o'clock, P. M.—Scrotum has been very much distended to-day, but is more flaccid at this hour, still very tender to the touch, especially about the ramus of the pubis. He has had an operation from his bowels. His pulse has been a little excited to-day, *but very little*.

18th, 8 o'clock, A. M. Burrell passed a good night: tumor not so painful as yesterday—not so large; that part of it which is near the external ring has a very firm feel. He had an evacuation from the bowels of firm consistence, and very painful. Pulse natural, and spirits good. Ordered, a dose of castoril.

19th, A. M. Tumefaction about the same as at last report, deposite so great as to occupy the whole of that side of the scrotum: says he has very little pain. There appears to be a perfect filling up of all, from the external ring to the bottom of the scrotum of the same side.

20th. Report differs in no way from yesterday. From this date the effusion was gradually absorbed, and at the end of a month, notwithstanding the use of a good truss, his hernia was as bad as ever.

CASE 2. In Glennville, Ala., July 24th, 1847, I operated on a middle-aged negro, ruptured on the right side. He was temporarily relieved, but the hernia returned in about a month. It is proper to say, however, that he was put to hard work (cutting saw-logs) as soon as he was relieved from the immediate effects of the operation.

CASE 3. In September following, I operated on a negro man, aged 40 years. This negro was ruptured on the right side, and was also laboring under ascites, with a fluid distention of the scrotum. When last heard from he remained well of the hernia.

CASE 4. At the same time as the above, I operated on Mr. C. J., a young gentleman aged 21 years, ruptured on the right side. This operation produced no effect whatever. In two weeks I operated on him again, without even temporary benefit.

CASES 5 and 6. In October following, I operated on two blacks, without any permanent effect.

All the negroes operated on were put to hard work as soon as they were sent home, so that, even if the operation promised anything, the chances of success were thus very much diminished, if not entirely destroyed.

So far as the danger of the operation is concerned, I have yet to learn that any existed. *I have even seen the intestines come down the day after the operation*, and returned without any bad consequences. I have seen the oil of cloves thrown into the canal, when a portion of the omentum was yet unreturned. This being returned, no more was thought of it.

Since the latter part of 1847, I have not operated at all; and if I were called upon again to make an attempt to relieve a case of hernia, I would rather trust to the application of a small seton at the external ring than to an operation of this kind. It is a well established fact, that when irritating injections fail to relieve *hydrocele*, a small seton through the sack is almost sure to cure it, by the deposition of a large quantity of coagulable lymph. The seton is much more likely to produce constitutional disturbances, and is consequently rather more dangerous; but I feel certain that it is much more likely to block up the external ring than any injection.

CASE 7. In May last, I operated on Mr. C. for strangulated hernia. A fold of the intestine, about six inches long, and a portion of the omentum had descended, and at the time of operating they were very badly strangulated. I returned the intestine, but being fearful that the omentum was too far gone, I determined to let it remain where it was—to relieve itself of suppuration; and, moreover, to see what effect a long-continued suppuration would have in curing the hernia permanently. Suppuration continued about a month—all the strangulated omentum sloughed away, and the external ring appeared to be blocked up.

I saw Mr. C. again in December last. There never had been any descent of any of the viscera since the operation. The intestines would pass down the canal to the external ring, and there seem to be arrested by its occlusion. He had never worn a truss. Further time is necessary to determine as to the permanency of the cure.

CASE 8. On the 21st of November last, I operated on a gen-

tleman fifty years of age, for the relief of strangulated hernia. He had been ruptured for twelve or fifteen years, and lately had been very much troubled with a descent of another portion of the viscera. He had generally been relieved by purgation. This course, however, had failed to relieve him of his present attack. I should think that from the moment of the descent the strangulation became perfect; for in less than an hour after, he had vomiting and hiccough, which continued, with very little intermission, until the completion of the operation.

I operated twenty-four hours after the accident. There was a very great depression of the vital energies. His skin was very cool, extremities so cold as to require artificial heat to them, constant shivering, pulse 130 to the minute and very weak. On cutting down, I found the old hernia to consist of a portion of the omentum, which had become so closely adherent to the scrotum as almost to constitute a portion of it. The recent hernia consisted of a fold of the small intestines about four inches long, and so tightly constricted as to make it very difficult and dangerous to pass up the probe pointed bistoury to divide it. As careful as I had to be, I cut my finger in protecting the intestines. After so long a time, I succeeded in passing the knife and removing the difficulty. Three hours after the operation, when reaction had become fully established, I found that a small artery, which had been divided in the first cut through the skin, was now bleeding freely. The whole scrotum was filled up to the external ring with coagula. I secured the ends of the artery with ligatures, and removed as much of the coagulum as possible; yet a great deal remained. I again united the edges with sutures and adhesive strips. Very little suppuration afterwards took place; the remaining coagula were probably absorbed. He recovered rapidly, and was up in ten days. Up to this day he has had no return of the hernia.

So far, Mr. Editor, as my experience will warrant the opinion, I think that many cases of strangulated hernia are rendered fatal by the severe and long-continued taxis which is frequently resorted to. Let us suppose a surgeon called to a case of strangulated hernia. The patient has been ruptured for a long series of years; this portion of the viscera (be it

omentum or intestine) has never been returned, we must reasonably presume adhesion to have taken place: another portion of the viscera comes down and becomes strangulated—can any one for a moment suppose that taxis will relieve such a case? An how can any one discriminate these portions—the one strangulated and the other not—without cutting down to them?

Again, I have seen cases rendered fatal by delay, from a false opinion entertained by some surgeons of the great danger attending these operations. I am satisfied that the patient suffers as much pain *before* as *during* an operation to relieve the strangulation. The operation of itself does not seem to me to be necessarily hazardous. The large proportion of successful cases are those who received early the benefits of an operation.

I have been insensibly drawn from common reducible hernia, to cases of strangulated hernia. In the illustration of my views, my cases of strangulated hernia presented points which I was desirous of bringing forward, and hence the blending of one kind with the other. The above article contains my own views upon this all-important disease. However much they may differ from those of others, I honestly entertain them, and therefore do not hesitate to submit them to the profession.

ARTICLE IX.

Extraordinary Case of Mortification, occasioned by Pregnancy. By SAMUEL L. HAMILTON, M. D., of Chattooga county, Georgia.

Mrs. S., aged twenty years, during the sixth month of her second pregnancy, was attacked June 5th, 1848, with remittent fever, which readily yielded to ordinary treatment. After this she did well for twelve or fifteen days, during which time nothing of importance occurred, but she was perhaps unusually hearty, which I attributed to her condition.

On the morning of the 20th of June I was again called to see her, and found her with extreme pain in the toes of her right limb, which was so cold half way to the knee as to yield to the

hand when applied the same sensation as that of a dead body. There was little or no excitement of the pulse; nor was there any evidence of disease in the general system. The pain in the foot increased from day to day, until the cries and shrieks of the unfortunate sufferer were almost too intolerable to be borne by her surrounding friends. It cannot be said that she was entirely easy until her delivery, except while under the influence of large doses of morphine, from the effects of which she would frequently awake in violent pain. Her sleep was evidently not profound, under as large doses of the above medicine as it is thought prudent to administer under any circumstances. It was attempted, but in vain, to restore the lost temperature of the extremity—every thing was used that would promise this result, without vesicating the skin, which was avoided, notwithstanding the repeated solicitation of friends for the application of a fly-blister. There finally appeared large yellow blisters upon the diseased surface, which, on being ruptured, discharged a serous fluid. It was evident, after this, that there was not sufficient vitality in the skin to warrant recovery. The skin and all the soft tissues now began to slough, until the ligaments of the metatarsal articulations were destroyed—at this point they were separated, which gave the patient some slight relief. The temperature of the limb was partially restored from above, down to within a short distance of the ankle-joint; the pain, however, continued unabated 'till labor came on, when it then entirely ceased. The lady was then delivered of a large child, after which there was not the slightest return of pain. The labor was in all respects natural—the stump of the extremity, which had been suppurating, now readily healed—the woman recovered, and has since given birth to two children, without the slightest return of the affection.

I was constrained to the opinion that this extraordinary case was the result of pressure of the gravid uterus upon the arteries and nerves passing through the pelvis—viz., the ext. iliac artery and the great sciatic nerve, whose extremities supply nutrition and innervation to the disorganized part. From the history of her previous pregnancy, it appeared that there had been running ulcers from about the sixth month until delivery; the cicatrices were still perceptible, and occupied the

regions covering the posterior and anterior tibial arteries. I think it a reasonable inference, that the pressure was exerted upon the nerves, as well as the arteries, from the intense and continuous pain which nothing could relieve until the removal of the cause, when the suffering simultaneously disappeared.

As to the treatment of the case, opium may have given some temporary relief. Position was tried without the least success. Supporters were also tried with the same result. We would feel inclined, however, were we called to treat another case, to try more thoroughly, both supporters and position. We might particularize as to local applications, &c., but they were such as common sense would dictate, and failed to afford any relief—we therefore omit their tedious detail.

ARTICLE X.

A Case of Retained Placenta. By P. W. HARPER, M. D., of Laneville, Hancock Co., Georgia.

On the 12th of last October, I was requested by my young friend, Dr. George W. Darden, of the Shoals of Ogeechee, to visit Miss C., of Warren county, thirty-one years of age. Before proceeding in detail with her case, it may not be amiss to say, that her feet are very much affected with the deformity familiarly called club-foot. She measures, from the sole of her foot to the knee-joint, $13\frac{1}{2}$ inches; from her knee-joint to her hip-bone, 11 inches; from her hip-joint to the crown of her head, $23\frac{1}{2}$ inches—making her exactly four feet high, of strong and healthy constitution, with no deformity about the *pelvis* that we could discover.

On my arrival, I ascertained she had been in labor about thirty hours. The membranes had been ruptured several hours before I saw her. On examination, I found the case natural, and of the fourth presentation. There was considerable rigidity of the parts, particularly the *os uteri*, which impeded the further descent of the *occiput* from the left *acetabulum*. We took sixteen ounces of blood from the arm, and then made another examination, and found the same contraction as before, without any progress of the child's head. The bowels having

been freely opened by a purgative, we gave her a dose of morphine, and left her to rest during the night. On examination the next morning, the parts were relaxed, and the *os uteri* so dilated as to admit the whole of the *occiput* into the left *acetabulum* with the anterior fontanel resting a little above the left *symphysis pubis*. The pains became very regular, and as we supposed would now expell the child; but in this we were disappointed. The head became so impacted in the *pelvis*, from its large size, that no efforts we could prudently use changed the relative position of the head: we could neither make it advance nor recede. In this situation she remained until the next day (14th), when, as the last resort, we had to lessen the size of the head, by perforating it at the posterior fontanel, and emptying it of its contents. Several hours yet elapsed before we could effect the delivery of the head, around the neck of which the *funis* was wrapped; this, placed immediately under the *symphysis pubis*, with the size of the child, weighing ten pounds, added greatly to the delay of the expulsion of the shoulders. After the delivery of the shoulders, she seemed much prostrated, and all pain ceased. For two or three hours, we gave the wine of ergot, in table-spoonful doses, every twenty minutes, without any effect whatever. We then had to use force by extension and counter-extension, which soon effected the delivery, at 12 o'clock, P. M., on the 14th, after having been in labour from the 11th, say four days. We then proceeded to extract the *placenta*, but here we met with insuperable difficulties by the contraction of the *uterus*, though not the hour-glass contraction. All efforts having failed, we had to desist. We gave her a dose of castor oil, and left her, with directions to take morphine after the bowels were opened, to be seen the next morning.

15th. On our visit, we found the *uterus* relaxed; but, on account of the parts becoming so very sore and sensitive, we could not extract the *placenta*, which was attached at or near the *fundus uteri*. The only alternative which seemed to be left us now was to use emolient and soothing injections up the *vagina* and into the *uterus*, and to meet whatever contingencies might happen, and to trust the case to the efforts of nature until the next day.

16th. Rested pretty well during the night ; with some fever, though the pulse soft and compressible ; seems revived ; complains of great soreness to the touch, and the parts much swollen. No portion of the *placenta* could we extract. Directed equal parts of milk and warm water injected three times a-day ; dose of castor oil and morphine at night.

17th. The bowels were moved, and she rested tolerably well during the night ; pulse 130, weak, and much fetor in the room. Efforts made to extract the *placenta*, but failed. Same injection continued, and morphine to be given at such intervals as to compose.

18th. Did not rest well the preceding night ; pulse about the same as the day before ; great thirst, and some portions of the *placenta* have come away, putrid and very offensive. Emollient injections with weak soap-suds and morphine directed.

19th. Much worse, with an enlargement of the abdomen and great soreness about the *uterus*, as well as the other soft parts ; pulse 120, and weak ; small portions of the *placenta* continue to be discharged. Directed the bowels to be moved with castor oil, and the same injections and morphine.

20th. Considerable distension of the whole abdominal region ; pulse quick and weak ; about two table-spoonfuls of the *placenta* have been discharged of the color and consistence of molasses, and so very offensive as scarcely to suffer any one to remain in the room. Being insensible to all passing events, we left her, with the expectation of her dying during the day or night. She died about 4 o'clock, P. M.

ARTICLE XI.

Non-congenital Talipes Equinus. By L. A. DUGAS, M.D., &c.

Cases of accidental or non-congenital Club-foot, are so rare that no apology is necessary for reporting the following. It is the second only that the writer remembers to have seen.

The subject of this case was James H., of this city, about nine years of age. When three years old, his limbs being perfect, he took scarlet fever and was very ill with it. After the eruptive period, he suffered a great deal with the pains

which sometimes attend this disease; and during the three succeeding years there seemed to be occasionally an impediment in his walking, which was attributed to weakness or to something of a rheumatic nature. When six years of age, however, there was still no deformity whatever about the foot nor leg. Indeed, at that time, none but his parents could perceive any thing peculiar in his gait. But his lameness became gradually more and more evident, although he suffered no pain; his left heel was perceived to be a little elevated in walking, so as to throw the weight of the body upon the anterior part of the foot; and this peculiarity went on increasing for a couple of years, until he walked upon the extremity alone of the metatarsal bones, being utterly unable to reach the floor with the heel or to flex the foot in the least.

I saw him for the first time on the 6th of April, 1852, and was then furnished with the above history. I found his left foot presenting all the features of talipes equinus in the extreme; the muscles of the left leg were very much atrophied, and, although there was no ankylosis, the tension of the tendo achillis was such as to prevent any motion of flexion; the extension of the foot seemed to be carried to the utmost possible degree, the instep being drawn back beyond the axis of the tibia, and the toes turned up towards the instep. On carefully measuring the two limbs, the left ankle was found to be about one inch above that of the right limb. In other words, the left limb was an inch shorter than the right one.

It is probable that in this case, the immediate cause of the talipes is to be found in the gradually increasing disproportion in the length of the limbs, the child instinctively elevating the heel to compensate for the difference. Why the left limb did not grow as fast as the right is another question, which need not occupy us at present.

A sub-cutaneous section of the tendo achillis was made, and four days afterwards an apparatus applied for the purpose of rectifying the deviation of the foot. This consisted of the sheet-iron splint and curved bar proposed some years ago by Dr. Chase. It answered admirably in this case, as it has repeatedly done in the several varieties of talipes in which I have used it. After wearing this about three weeks, my little

patient left it off, was able to walk, and his heel gradually came down to the ground—so that he was well in two months, and has remained so to this time.

Augusta, Feb., 1853.

ARTICLE XII.

Monstrosity.—[We are indebted to Dr. D. McKINNON, of Pike Co., Alabama, for a letter, from which we make the following extract:]

“Mrs. — M., on the 3rd of November, 1852, after a tedious labour, was delivered of a male child, weighing about seven pounds, and presenting the following appearances:—The head and limbs well developed, the stomach, duodenum, jejunum, ilium, cæcum, colon, rectum, liver and spleen, were all on the outside of the abdomen, without any covering. The viscera protruded from an opening extending from the pubis to the umbilicus; the cardiac extremity of the stomach and the rectum were closely attached to the abdominal walls at said opening, the scrotum forming a part of the attachment of the rectum. There was no penis nor anus. The infant lived four hours.”

PART II.

Eclectic Department.

Letters upon Syphilis. Addressed to the Editor of L'Union Medicale, by P. RICORD. Translated from the French, by D. D. SLADE, M. D.

[Continued from Page 112.]

SIXTH LETTER.

My dear Friend,—Let us continue this review of facts and arguments which have been opposed to my doctrines.

There is an observer upon whose works my antagonists place great value, and they are in fact worthy of much esteem. I have cited them honorably in my preceding letter, and you see me disposed to accord to them the value which they merit. This observer, whose results have been opposed to me without cessation, is M. C. Martins. Well, what do the results of M. Martins prove in the elucidation of the great question of the

consequences of blennorrhagia as cause of syphilis? Remark that it is precisely on account of the accuracy of the observation, of the scientific method employed by this observer, and in fine of his statistics, that they have made so much noise about his figures and his conclusions. What, then, do his figures say? I find them very favorable to my doctrines. Is it by complaisance? Judge of it?

M. Martins gives a statement of 60 observations of syphilitic eruptions. Now how many times has the chancre been noted as antecedent? 46 times, my dear friend. In 14 cases only, M. Martins assures us that he has found no other antecedent than simple blennorrhagia, two of which were accompanied with bubo, and two with orchitis. But M. Martins adds that he had not the opportunity to make the diagnosis of these cases of blennorrhagia, and that he trusted to the testimony of the patients. You know what I think upon this point. There are some testimonies, without doubt, that we ought to believe; but I shall always maintain that when there is a question of diagnosis as difficult as that of chancre in the urethra, the testimony of people entirely strangers to the profession, often ignorant and narrow-minded, and who understand neither the sense nor the bearing of the question, is of very little value. Without doubt we accept testimony in some questions much more grave, in those of life and death; but it does not follow that the testimonies are always true, and the judgments always equitable.

Permit me to offer you a general remark, which finds its place here. In many of the observations of M. Martins, as in several of those of M. Cazenave, and as in almost all those of a great number of authors, you find in their summary these words—*many primary accidents*. These primary accidents which have necessarily produced the constitutional verole, are the chancre and the blennorrhagia. If my antagonists, by some reasonable motives, attached the consecutive infection rather to blennorrhagia than to chancre, we should have to examine this doctrine. But no, you know and you have read it, and you ought not to be much astonished, that it is together that they group these primitive accidents; that it is without considering the distance which separates their appearance one from the other, and that it is in giving to them all the same value, the same consequences and the same results. In truth, is this good science, is it strict observation? What should you think of a physician who should tell you, here is a man suffering from hydrophobia; he has been bitten ten times; it is three years since, two years, one year, six months, and very recently. But his disease is evidently owing to the successive inoculations which he has undergone. Or, here is a varioloid patient, who

has gone through five or six epidemics of variola—at the last one the disease manifested itself; it is but the consequence of contagious and successive infections.

I confess that it is not thus that I understand science. I am astonished that a mind as strict as that of M. Martins, who agrees with me that blennorrhagia is due to causes entirely foreign to syphilis; who logically is forced to admit that the blennorrhagic antecedents as causes of syphilis are extremely rare, and that the chancre consequently is the most frequent antecedent of the verole—I am astonished, I say, that in order to arrive at the conclusion that a simple blennorrhagia can produce syphilis, he is content with his sixty observations, of which he chooses three, and particularly one, which I ought to bring forward here.

“An apothecary, aged 23 years, contracts a blennorrhagia, but it troubles him so little that he continues at his occupations. He goes hunting, and even has sexual intercourse. Then follows an orchitis, which forces him to take care of himself. The blennorrhagia is cured, after having lasted six months. Seven years afterwards, an *ulceration appears at the opening of the left nostril, another one at the internal surface of the lower lip*. These ulcerations extend; the two lips are attacked upon the entire left side, then they are partially cured, and ulcerations follow at other points. *The ulcerations have rounded borders, and are cut perpendicularly*; the cicatrices are delicate, rosy and pliable. The patient, admitted into the wards of M. Bielt, is cured in a month by the use of the protoiodide of mercury. Shall we say that this patient, half physician, who examined himself carefully, as we have seen him do at the hospital, had chancres without perceiving them?”

Yes, certainly, I will say that that patient had very well-marked chancres, from the description which M. Martins gives, and that the patient had not recognized them, on account of the unusual seat which they occupied. As to the manner of the contagion, M. Martins will not ask me, and I shall not take it upon myself to point it out. He knows, however, as well as I, how these accidents can follow, and without seeking malice therein, in the exercise of the duties even of this good apothecary.

You are aware, my dear friend, that the chancres, unusual in their situation, and difficult to discover, are less rare than is thought to be the case. I cited to you an example in my last letter. Here are others.

Some years ago, M. Lustermann, professor at Val-de-Grace, brought to my house a lawyer, having a tumor upon the lower eyelid, at the inner angle of the eye, hard, resistant, elastic,

with a red granulating surface, and in process of cicatrization. This tumor had been already seen by many physicians, and, if my memory serves me, some oculists had been consulted, but its nature had been until now unknown. I was asked if it was connected with some venereal antecedent more or less distant. Pushing my examination further than my brethren, I found the glands about the ear, those of the parotidean region, and the sub-maxillary, enlarged, indolent and elastic. The posterior cervical glands were already tumefied. The surface of the body was covered with exanthematous spots proceeding from the best characterized syphilitic roseola; lenticular spots of a dull red, leaving in some places, under the pressure of the finger, a tawny yellowish color; absence of fever and of pruritus.

To the great astonishment of M. Lustermann, this was my diagnosis: *Indurated chancre at the inner angle of the eye (successive engorgement of the glands about the ear, also of the parotidean and sub-maxillary); secondary affection of the cervical glands; syphilitic roseola; precocious secondary accidents.*

To the great astonishment of the patient, I said to him—It is two or three months, sir, more or less, that you conveyed to your eye the contagious matter, which inoculated you with syphilis. Recovered from his surprise, the patient said to me, "In truth I remember having slept with a woman, and after certain contacts, I was seized with much itching about the eye, where I carried my hand, and rubbed it during a considerable time. It is from that moment that my eyelid has become diseased."

Is it not true, that if this gentleman had been attacked with a blennorrhagia, either antecedent or accompanying, it would have been to that, that the chancre of the eye and the secondary accidents would have been attributed? Very well, I must say that the nose of M. Martin's apothecary found itself probably in the same condition as the eye of our lawyer.

M. Cazenave ought to recollect the history (no longer ago than 1847) of a very intelligent student in medicine, in whom he diagnosed a constitutional syphilis *d'emblée*, characterized by a roseola without antecedents. This young man presented himself at the Hospital du Midi, and there we were able to show the existence, before all the students, which had passed entirely unperceived, of an indurated chancre extremely well marked, seated upon the left cheek, and concealed under a thick tuft of whiskers. The sub-maxillary glands—unobjectionable witnesses—were engorged and indolent, with that character of resistance peculiar to these glandular enlarge-

ments, symptomatic of indurated chancre. This ulceration, to which the patient had attached no importance, being revealed to him, he was able to state with precision the origin and the date of it, which agreed perfectly with the appearance of the secondary symptoms.

At this same time in the wards of the Hospital was a patient having a chancre (primary accident) upon the sinciput. I showed, at my clinical lecture, a woman who had an indurated chancre upon the left eyebrow, with a symptomatic enlargement of the glands about the ear, which had two months preceded a nocturnal cephalalgia, enlargement of the posterior cervical glands, and a roseola.

I should never finish, if I attempted to indicate those cases only which have passed under my eyes, of chancre seated in unaccustomed places, and which would be confounded with secondary accidents attributed to a blennorrhagia of shorter or longer standing, by observers little accustomed to accuracy. I have at this moment even, in the first ward of my hospital, a patient, affected at the same time with a simple blennorrhagia of the urethra (inoculation negative), and with indurated chancre of the upper lip, accompanied with an indolent enlargement of the submaxillary glands, concomitant affections, but independent one of the other.

Here is sufficient, it appears to me, to prove to you how frequent and insidious are the causes of error under similar circumstances, and to legalize my scepticism as regards certain observations.

But I ought not to forget that my learned brother of Lyons is waiting for me with five observations which he opposes to my doctrines. I ought the more to return to them, as these five observations have sufficed to convince the strict and reserved mind of M. Legendre.

First, as I have already told you, one of these observations is done away with, for the patient who is the subject of them had had previous chancres. Four cases of simple blennorrhagia followed by syphilis remain. But of these four cases I shall permit myself to do away with two, for M. Baumès did not practise inoculation. These cases ought, then, to enter into the numerous category of those blennorrhagias for which there has not been a strict diagnosis. One remarkable fact, which you will permit me to notice in passing, is, that M. Baumès, who is certain of having inoculated the greater portion of the patients who have presented themselves to him, has fallen precisely upon two cases of syphilitic blennorrhagia, in the diagnosis of which he deprived himself of the precious aid of inoculation. We are then reduced to two other cases, where

inoculation has been practised with a negative result, and which have been followed, nevertheless, by constitutional accidents!

In one of these cases there is question, also, of a nose, which again appears to me excessively suspicious. Here is the history, reported by M. Baumès:—

“Of the two patients inoculated, one remained at Antigua two months. His blennorrhagia was difficult to cure; he had still a white discharge when he left the Hospital. He entered it again three months after with a syphilitic eruption, in red patches, copper colored, partly furfuraceous, partly scaly, and a rounded ulcer with a greyish ground, with perpendicular borders, and with an erysipelatous circumference situated in the left nostril. At this period the discharge did not exist. This patient had had no coitus since his leaving the Hospital.”

You will find here, again, a very complete description of the primary ulcer; and how does it happen that in presence of a fact so important, in the point of view of a question so litigious, M. Baumès did not try the inoculation of this chancre? I regret it sincerely, but in the absence of all strict diagnosis, I ought to place this nose in the same category with the nose of the apothecary.

Here I am, then, face to face with one observation only of M. Baumès, and that the last one. My learned colleague well says that he inoculated from the seventh to the tenth day of the appearance of the discharge; but how much time had passed since the infecting coitus? M. Baumès knows perfectly well that a knowledge of this is not unimportant. He knows, also, as well as I, that the chancre which is ordinarily followed by secondary accidents, generally extends itself but little: that it is perfectly indolent: that its suppuration is so little, that it can pass unperceived. Upon all this, M. Baumès is as well edified as myself, I am very sure. These ulcerations do not in any way prevent a blennorrhagia from being produced, a short or a long time after, and it is not astonishing that the one in question did not furnish inoculable pus, the chancre having arrived at the period of reparation, or having completely disappeared. It is moreover necessary to suppose, that before his first entrance into the Hospital, or after his departure up to the time of his return to it, the patient had not undergone another contagion, and by a way which escaped the sagacity of our colleague.

All these objections apply equally to the observation of M. Lafont Gouzy, in which secondary accidents came on after a blennorrhagia which had been inoculated without result. He does not say any thing of the time which separated the coitus

from the manifestation of the symptoms, a period sufficiently long for the cicatrization or reparation of a chancre.

It appears to me, after all this, that my colleague of Lyons, who maintains that the simple blennorrhagia can give rise to the same accidents as the chancre, can permit me to send back what he addressed to me, viz., "that he establishes as principle that which is in question, and advances an hypothesis devoid of strict foundations."

Thus fall to the ground, one by one, the objections so grave in appearance made against my doctrine. Thus, I continue to believe—

With Girtanner, "that syphilis recognizes most generally for cause, chancres and buboes, and that it very rarely follows a blennorrhagia."

With Swediaur, "that the symptoms of syphilis are rarely manifested after blennorrhagia."

With M. Rayer, "that the secondary cutaneous eruptions with blennorrhagia are rare; that we observe them in a much smaller proportion, than after superficial and deep venereal ulcers."

These opinions, as you see, agree very well with the relative scarcity of the chancres of the urethra with symptoms of blennorrhagia."

I could still cite many other authorities. But I have not finished with the objections. In my next letter I shall examine some of another nature.

SEVENTH LETTER.

My dear Friend,—From this fact alone, viz., that chancres have been submitted to a treatment called methodical, it has been thought that the consecutive accidents of a constitutional infection, which ought to be the result of chancres, could be attributed to a blennorrhagia which came on afterwards. M. Baumès pretends to prove it in one of his five observations. But what is a methodical treatment? What is the treatment upon which we can absolutely depend for neutralizing effectually the syphilitic diathesis? For myself, I do not know of an infallible one. I well know that a great number of very distinguished practitioners think that with a certain dose of mercury, administered during a given time, we ought to consider the patients as radically cured. And in order not to go beyond the limits of my hospital, I shall cite my very honorable colleague, M. Vidal, who has recently given out, that with one hundred and ten of Dupuytren's pills, neither more nor less, we ought to put an end to syphilis.

As regards creeds, I am the most tolerant man in the world. Nobody more than myself respects the religion of others; but I have the right, it appears to me, to refuse a participation in all their convictions, when I see every day the proof of the great errors into which a blind faith may conduct one.

M. Vidal ought to have seen many patients return; and if this has not happened to him, let him permit me to say, that I myself have seen a great number of those, who have not only taken the one hundred and ten sacramental pills, but even 120, 150 and more, all of which has not prevented the symptoms from reappearing.

I shall no longer insist upon this point, for I shall have occasion to return to it later. What I want to establish here, is, that those persons are often deceived who have thought that they ought to ascribe accidents of constitutional syphilis to a blennorrhagia which has come on after a chancre, from the simple fact that the chancre which had preceded, had been submitted to a mercurial treatment.

Here is a point more astonishing, something which will surprise your reason and baffle your logic.

My opposers have established several categories of veroles, according to their origin and their source.

Thus they admit, and in this they are perfectly right, that constitutional syphilis can be transmitted by way of inheritance.

They assert, and they have pretended proofs for this assertion, that constitutional syphilis can be taken *d'emblée*.

They assert, and they publish facts for the support of this assertion, that sometimes no kind of antecedent to constitutional syphilis can be found, although they do not dare to ascribe it to the syphilis *d'emblée*.

They pretend that an individual under the influence of a syphilitic diathesis, without present manifestations, without apparent symptoms, can, however, under certain circumstances, transmit syphilis.

They maintain that the duration of the incubation of syphilis should be unlimited, that the manifestations of the contagion should appear as well after a few days as after a few months, as after several years, twenty, thirty and more.

All these categories, all these distinctions, you will find established particularly in the writings of M. Cazenave; but upon what grounds? Here is what I in vain ask myself. I inquire by what process, by what means of diagnosis, we can come, in a patient affected by a constitutional verole, to attribute this disease to one of these circumstances rather than to another.

Has hereditary syphilis, after early infancy—and we shall hereafter see that its effects can be prolonged—a special symp-

tomatology? Can constitutional syphilis, *d'emblée*, be distinguished from the other kinds by any pathognomonic sign? Do the cases of verole in which the antecedents have not been made out, give rise to disorders different from those in other cases? What is a verole without antecedents, unless it is a *verole d'emblée*? Do we find that those cases of syphilis which have succeeded to simple blennorrhagia, assume forms less grave, or have less extended seats, as M. Baumès pretended to find in writing his book, but which he has not been able to meet with in his practice?

I answer boldly, no, to all these questions. Constitutional syphilis presents a symptomatology alike in all cases; and it is not I who prove it, it is my opposers themselves. Read again their writings, and see if you can find in the descriptions given by MM. Cazenave, Baumès, &c., one single characteristic trait which justifies these arbitrary distinctions.

Again, one thing in my opponents astonishes me. How does it happen that in these cases of constitutional syphilis, whether *d'emblée* or without antecedents, when it has been impossible for them to be assured of the conditions of the contagion—to state precisely the when and the how—if it is well proved that the patient has presented no primitive accident, they having found no door of entrance to the verole; when they are well convinced that the patient is not mistaken, and that he has no motive in deceiving; when, in fine, they have the certainty of not being themselves deceived; I am astonished, I say, that they do not admit what Cullerier admitted to explain the inexplicable cases, viz., spontaneous syphilis in man.

M. Richard des Brus has made this great step. Among other facts which brought him to this conviction, he cites one which is very curious. A young man and a young woman yield themselves to the pleasures of love. In his ardor the young man scratches himself with a hair of his mistress. He does not stop for such a trifle, and he does so well, that he communicates his *écorchure* to his mistress. The amorous couple are soon simultaneously affected with constitutional verole. M. des Brus, who had examined neither of them, did not the less admit a previous good state of health; but not being able to explain the appearance of the verole, he declares it spontaneous.

I am not as far advanced as this learned colleague, and the so frequent opportunities that I have of seeing constitutional affection succeed to a well-determined primitive accident, causes me to rank the exceptional cases, where the patient does not know or does not wish to enlighten me, and those in which I arrive too late to find the entrance of the syphilis, in the cate-

gory of observations which M. Cazenave entitles *unknown antecedents*, and which I call *overlooked*. Alas! is it not more satisfactory for the mind, more conformable to our manner of reasoning in medicine, to admit in those cases where syphilis has really succeeded to a blennorrhagia not symptomatic of chancre, that the antecedent has not been *recognized*, rather than to lose one's self in that crowd of subtle distinctions, of arbitrary categories, and of sterile explanations? How, otherwise, will my contradictors undertake to prove to me what they say, and to convince me of error? It is not my habit to challenge any one; this sort of argument ought to be banished from scientific discussions; but I much wish that they would engage to prove to me once only, yea, once, that, in those cases where all my researches having been vain I have said *antecedents overlooked*—that they would prove to me, that something more affirmative could be substituted for this formula.

From this long discussion, my dear friend, it will appear to you without doubt legitimate to conclude—that if in this immense majority of cases, blennorrhagia is simple and benign, there exists also a virulent blennorrhagia; and that the blennorrhagia is virulent when there exists a concealed chancre in the urethra.

Now does the means of making the diagnosis of concealed chancre exist?

Is it possible to distinguish a simple blennorrhagia from a blennorrhagia with concealed chancre?

Here is the grand question. I commence the discussion of it.

Some persons have made light of the diagnosis of blennorrhagia. Hecker, and some others who have followed him, have not thought that the diagnosis was necessary. Very recently I read in your valuable Journal that the diagnosis had no relative importance. A certain number of physicians have retained ideas which have been in vogue, and which ought much to astonish the public.

Have you caught blennorrhagia from a wife who was not yours? Virulent blennorrhagia. The blennorrhagia is virulent for the lover, for the husband it is benign. You have contracted a blennorrhagia, and you ought to remain bachelor. Simple treatment. But you wish to marry. Antisyphilitic treatment. The position of bachelor, or of future husband, has the privilege of causing the blennorrhagia to pass from the benign state into a virulent state.

In a question as serious and as important as this, I do not wish to insist upon the ridiculousness of these contradictions. All have understood the necessity of a more strict diagnosis. The latest of my opponents, M. Vidal himself, with whom my

proceedings in diagnosis have not found favor, has made some attempts in this matter. In the first edition of his Treatise upon External Pathology, he gave out the hope that it would be possible to distinguish a virulent discharge from a benign one, by the *odor*. This appears, and it is to be regretted that his hopes were not realized, for this passage disappears in the second edition.

I hold rather more to my ideas than M. Vidal appears to hold to his. Will you, then, permit me to give out once more, both my ideas and my experience upon the diagnosis of blennorrhagia, and to examine the objections which have been made to them.

But I cannot treat of this subject in the short space which remains for me, not wishing to abuse to-day the generous hospitality which you afford my letters. This point will be the subject of my next epistle.—[*Boston Med. and Surg. Journal*.

On the use of Quinine in Tetanus. By E. A. P^YE, M. D., of Louisiana.

In the spring of 1850, Dr. N——, a retired physician of Catahoula parish—a gentleman of intelligence and information—called on me to know “if I had seen any thing, in the course of my late reading, worthy of a trial in Tetanus.” He had a negro boy, about 14 years old, laboring under the most violent attack of Traumatic Tetanus that he ever witnessed. The boy had fallen from a horse some weeks before, receiving a wound in the face. The wound was apparently slight; little attention was paid to it, and it healed in the usual time. Symptoms of Tetanus, however, soon made their appearance, and had gone on constantly from bad to worse, in spite of the treatment. He had been purged, blistered, had taken opium, whiskey, spirits turpentine, calomel, the hot and cold bath, but with no relief. “In short,” said the Doctor, I have gone through the routine. I have tried the old practice; unless something else can be done the boy must die.” I recollected that in the New Orleans Medical and Surgical Journal for 1849 I had noticed a case, reported by Dr. Brickell, of the Charity Hospital, in which chloroform and quinine had been given, and in which the cure was attributed to quinine, in 30 grain doses. It seemed to me, at least, worthy of a trial—the quinine.* The sug-

* NOTE.—[A case of Tetanus treated successfully in 1848, with quinine and morphine, was reported by us in the 6th vol., p. 340, of the Southern Med. & Surg. Jour.—[Edr.

gestion was at once adopted. We decided to begin with 30 or 40 grains, and increase the dose until some effects were produced. The Doctor was rather doubtful of finding his patient alive on his return: but promised, if he was still living, to give the quinine a fair chance. On his return he found all the symptoms increased in intensity. The intervals between the paroxysms had dwindled to but a few moments of partial ease; with these transient exceptions, he was in a state of constant and most violent opisthotonos; and it was evident that unless relief could be procured, death must soon close the terrible scene. Taking advantage of the first opportunity, the Doctor got down his throat 30 grains of quinine—examining his watch at the same time. In one hour he again visited him—he could perceive no change; repeated the dose. In the course of the next two hours the Doctor thought or fancied he perceived slight—the slightest possible diminution in the intensity of the paroxysms; at any rate the boy thought himself relieved, and begged for the medicine. He got 30 or 40 grs. The improvement in the next two hours was evident. The paroxysms were not only less severe, but the interval was also decidedly longer and freer from pain. The boy's sensation of relief was yet more decided, and he clutched at his quinine and swallowed it with an evident "*gusto.*" He threw his arms heavily about, saying that he felt "drunk" and "happy!" The case went on regularly improving; the only other medicine given being an occasional dose of oil, to keep the bowels open.

At the end of two weeks I met the Doctor. The boy had taken two ounces of quinine; was entirely free from all symptoms of Tetanus; had experienced no bad effects from this enormous quantity of quinine; no tinnitus aurium—deafness—fulness of the head. The muscles had become relaxed, the skin was acting finely, the bowels were free. The only peculiar effect of the quinine being the one mentioned above, which he frequently expressed—the feeling as if he were about half drunk or "happy." Here a prudential regard for the "man with the poker," or some other analogous event, from the sudden withdrawal of an agent, whatever its *modus operandi*, which had kept our patient "happy" for two weeks, despite of Tetanus, induced us to taper off; which we accordingly did, giving him about half an ounce of quinine, in gradually diminished doses, in the course of the following two weeks.

I saw him at the end of this time, well, fat and hearty. The Doctor promised to give me the notes of this case, which would have rendered it more satisfactory. Circumstances having prevented my getting them, I have thought it might not be

altogether without interest as it is. In this age of heroic doses of quinine, the writer records his wholesale administration of the drug with much diffidence. He not only disclaims the unhallowed ambition of "out Heroding Herod," in this regard, but deprecates truly, on the contrary, the "spirit of enormous dosing" so rife through the land. One may, however, sometimes find himself so situated that the maxim "*occasio præceps*" must stand in the place to him of all precedent. In the present instance, the result of treatment, the circumstances of the case, the character of the gentleman by whom the medicine was administered—all combine to assure the writer that the practice, though seemingly ultra, was in reality only so much so as the occasion demanded.

Watson says there is no cure for Tetanus. Perhaps he is right; but Dr. Brickell records a case in which unequivocal symptoms of the disease yielded to quinine. I give you my facts for what they are worth. Let others contribute. "*Ex multis veritas.*"—[*N. O. Med. Journ.*

A Valuable Substitute for Ergot. By B. H. WASHINGTON,
of Woodburn, Kentucky.

I lately attended a midwifery case, and deem it advisable to report the treatment for further investigation. The pains commenced at 4 A. M., and continued at intervals of 15 to 20 minutes, until 6 P. M.; were slight, and produced but little effect. Upon examination, found os uteri dilated; head presenting, but high up; scarcely any effect perceptible during the pain. Not willing to leave the management of the case any longer to nature, I concluded to dry-cup her; applied a cup as low down on the sacrum as possible so as to cover the origin of the nerves to the os uteri and produce relaxation. Previous experiments had shown me the uterus would not contract unless applied higher up. My design was to produce relaxation of the os uteri, and then dry-cup higher up, so as to cause the uterus to contract. The result was most satisfactory to all interested, for complete relaxation ensued; at the next pain the head descended to the outlet, and at the second pain she was safely delivered, and that too in less than ten minutes from the application of the cup. No hemorrhage resulted; the placenta came away with scarcely any inconvenience in about three-quarters of an hour, and every thing went on well.

This is the second case in which the above plan has been tried: in the first, the patient was safely delivered in about 15 minutes, and with three pains. She had been suffering upwards

of twelve hours, without effect; was induced to try the plan from two facts—first, the partial paralysis of the arms while the cup was over the origin of the brachial nerves, and the successful application of dry-cupping in a case of dislocated shoulder joint, as mentioned in my first article on dry-cupping; secondly, the cups had been applied on a patient after delivery, to relieve the disagreeable feelings resulting therefrom, and the patient told me she could feel the uterus contracting very strongly. Putting these facts together, it occurred to me that as contraction was the legitimate function of the uterus, while expansion was the legitimate function of the os uteri, by the application of the cups over the origin of the nerves to the defective parts, the appropriate results would follow. The plan has been followed with the most complete success, as manifested by the cases above mentioned. I would therefore recommend a trial of it in all tedious cases. Apply, first, a cup as low down on the sacrum as possible, and if in the course of ten or fifteen minutes the patient was not delivered I would recommend the application of another cup higher up, so as to cause the uterus to contract; *the lower one should always be on when the upper one is applied, so as to insure relaxation of the os uteri when the pains come on.*

In cases of retained placenta, I would recommend a contrary course: apply the cups higher up, so as to cause the uterus to contract at once; the placenta can always follow the child.

The great advantage of this method of causing the os uteri to relax and the uterus to contract, over the plan of giving ergot, needs no other recommendation, I presume, than a simple statement of facts:—When ergot is administered, the woman is delivered by main force and in opposition to the usual proceeding of nature, without any relaxation except that produced by the most fearful and agonizing pains—by dry-cupping such a complete relaxation is produced, that two or three pains are sufficient, and the amount of suffering is not more than ordinary. Indeed, the amount of suffering is so much lessened, one would feel tempted to try it in ordinary cases, where every thing was going on well, merely to shorten the period of suffering. Another advantage is, that as soon as the delivery is over, the pains are over too; the placenta comes away with scarcely any inconvenience. Whether it would be justifiable to apply it in an early stage of labor, merely to lessen the duration of suffering, I leave to be decided by future experience. I certainly should not recommend it at that stage without further trial. In my first article on dry-cupping, I omitted two important items—first, that it breaks up the chain of nervous sympathy during pregnancy, and, regularly applied, *keeps it*

broken; the patient suffers very little, and that little more from imprudence than from nervous sympathy. Let the patient be dry-cupped every third or fourth night, (her husband can easily do it with a tumbler,) and sponge herself two or three times a week with water, cold or warm, according to fancy, and my word for it she will suffer as little as the heartiest Indian female, unless broken down by some organic disease. The other item is, that if there is a scant secretion of milk, or none at all, apply three or four cups to the spine, especially over the origin of the mammary nerves, and there will soon be a plentiful supply. These items are not theoretical assumptions, but the result of actual practice, and I sometimes censure myself for not making them public sooner, as an immense amount of sympathetic suffering can be easily prevented. As mentioned in my first article, when it is desirable to produce an effect on a given part, the cups should be more strongly applied over the origin of the nerves distributed to that part; and they should be larger than those in common use; a common tumbler with a thick rim will answer very well, except in female cases; in those I use the pump to produce a vacuum, as they can be applied under the covering without offending the most fastidious.—[*Nashville Journ. of Med. and Surg.*]

New Method of Treating Still-born Children. By T. Wood, M. D., of Cincinnati, Ohio.

Mrs. C—— was brought to bed in her first confinement, and had a very protracted and tedious labor, from a rigid and unyielding vulva. The child on delivery was in a state of syncope, so profound as to leave but little hopes for restoration to life. Full five minutes had been lost in fruitless efforts to excite breathing, and the only sign of life in the child was a slight convulsive effort while its lower limbs were yet in the vagina, after which it lay flaccid, ex-sanguineous, and in appearance dead. Cold air, cold water, and brandy had been thrown on its chest without producing the slightest effect, and I was about to inflate its lungs, when I noticed that the vessels of the cord were much distended with blood, and a very feeble pulsation in its arteries. Finding this condition of the cord, suggested the idea that, perhaps, if the blood it contained could be forced into the circulation of the child, it might afford the required stimulation. Instantly acting on the suggestion, I took the cord between my thumb and fingers, and drew its whole length between them, so as to force the blood into the child, when it immediately cried lustily, and animation was completely restored. It had no more difficulty in beginning life, and is now doing well.

I report this case under the impression that this mode of treatment is original.

Since having the above case, I tried the same treatment in a child that was delivered by a long labor, embarrassed by convulsions. Animation was at once restored on forcing the blood from the cord into the circulation of the child, but there was not, previous to resorting to this means, such complete prostration of the child as in the first case, and though effectual, the result was not so striking.

My friend, Dr. A. M. Slocum, informs me that since I related my case to him, he has tried it in a similar prostration of the child, with the same happy result.—[*Western Lancet*.

Tapping in Hydrocephalus.

The intractable nature of chronic Hydrocephalus is such as to warrant, in some cases, a resort to tapping; for, although this operation has usually proved ultimately unsuccessful, it has repeatedly prolonged life, and may yet be so regulated as to give permanent relief. In 1836 we tapped a child's head seven different times, drawing off sixty-three ounces of fluid without the slightest unpleasant effect—yet the child died, apparently from inattention on the part of the mother to directions. This case was reported in the first volume of this Journal, (old series,) 1836, p. 440.

The following is an extract from a case reported in the *Transylvania Medical Journal*, by M. Howard, M. D., of Louisville.—[Ed. S. M. & S. J].

“The head went on enlarging until it attained an extraordinary size—measuring 23 inches in circumference and 14 inches in a vertical direction, from one auricular orifice to the other. The pressure of the accumulated serum at length produced total blindness, and a partial paralysis of the muscles. The face became contorted, and the extremities hung flaccid and motionless.

“It now became apparent that the little sufferer was reduced to great extremity. I abandoned all hope of relieving it with the aid of mere pharmaceutical agents, and intimated to its parents that I was willing to give up the case as a hopeless one. On being urged to further effort, I proposed the operation of puncturing the membranes, as a dernier resort, at the same time warning the family that I had but little faith in its efficacy. The pressure of the enormous accumulation of se-

rosity on the extended membranes and the brain, induced an intolerable amount of suffering, which was indicated by the constant moaning and restlessness of the child, even in sleep. This painful pressure I could alleviate by decanting off a portion of the fluid, but it was at the risk of accelerating the final issue—*death*. I frankly stated this fact, and warned the family that the operation, though simple, was hazardous; that death might possibly ensue at once, and that the possibility of its effecting a radical cure was too remote to afford much ground for hope—the only certain recommendation in its favor being that it would effect a temporary remission of the suffering, if the patient survived it. Notwithstanding these representations, the child's father urged the performance of the operation. The mother, however, had conscientious scruples; he was, therefore, induced to defer submitting his child to the risk, until the sanction of a clergyman (the relative of the child before alluded to) could be obtained.

"In the mean time I had prepared a close-fitting cap of gum elastic, which I purposed to use, to maintain an equal pressure during the evacuation of the serum. I also obtained a minute trocar and canula, the blade of the trocar not being longer than the ordinary couching needle.

"Experience has taught us, that the principal danger to be apprehended in puncturing the head for cerebral dropsy, is the production of fatal syncope, from the sudden removal of pressure from the brain and its appendages. I therefore had stimulants at hand, and determined to decant the water slowly, watching narrowly, in the mean time, its effect on the pulse.

"The trocar was introduced carefully, a little to the right of the inferior angle of the fontanelle, in a perpendicular direction, to avoid the longitudinal sinus. The depth to which the instrument penetrated before the fluid was reached, seemed to favor the belief that the convolutions of the brain had been unfolded by the pressure of the fluid, and lay in a thin layer immediately in contact with the membranes.

"Some high European authorities have denounced this operation as cruel and worse than useless. But the result in this case does not sustain this view. The patient suffered very little from the introduction of the instrument, and apparently none at all afterwards. Upon the withdrawal of the trocar, the water was forced for a moment in a violent little stream for several feet, owing to the pressure of the tense membranes. I allowed it to flow freely for several minutes, no symptoms of exhaustion supervening until about twelve ounces had been withdrawn. Immediately after the operation the patient sunk into a profound and tranquil slumber, from which she awoke in about five hours, apparently much refreshed.

"The most important result of the first tapping was the removal of the paralysis. The countenance regained its natural expression, and the muscles of the limbs appeared to have become more subject to volition.

"To aid the effect of the tapping, I renewed the counter irritation, and the exhibition of alterative medicines. Three days after the first tapping, I drew off about eight ounces of water. After the second operation, long slips of adhesive plaster were passed around the head in various directions, to support the bones, and to assist the action of the elastic bandage. The little patient now appeared to be progressing finely. The head was diminished considerably in size, her appetite and command of muscle appeared to increase, and, better than all, the retina appeared to be regaining its sensibility. Previous to the first operation she was totally blind, and the muscles of the eyes appeared to work without concert. Strabismus divergens, and convergens, had occurred at irregular intervals; but now the movement of the eye-balls obeyed the will, and the nerves appeared to have become sensible of external impressions, though the sight was far from being perfect. These happy results encouraged me to attempt too much on the third operation. I decanted the serum a little too rapidly, and the patient sunk into profound syncope, from which she was restored with some difficulty. No untoward consequence ensued, but the mother of the child became so terribly alarmed at the effects of the last tapping, that she refused to allow its repetition. In her opinion, the object in view would not have justified the result, if her child had died from the effects of the operation.

"I had the satisfaction of finding, however, that no further effusion took place. The head was much reduced in size, and did not subsequently enlarge. She gradually gained a perfect control of the voluntary muscles, and her sufferings from the serous accumulation appeared to be entirely removed, so that she became even playful. Several months after I had removed from the county, I learned from Dr. William Dunham that the child had died from an attack of typhoid fever, (typhoid pneumonia, I think.) Up to the time of her last illness no change for the worse had taken place in her condition."

Wry Neck Cured without Cutting. By GURDON BUCK, M.D.,
Surgeon to New York Hospital.

The success obtained in the following cases of Distortion of the Head, commonly known as Wry Neck, induces the writer to make them known to the profession, in order that the treatment employed, which it is believed has not hitherto been

applied in such cases, may be fully subjected to the test of experience :

CASE I. Hester Higgins, a native of Ireland, aged 25 years, unmarried, was admitted into the New York Hospital on the 6th Nov. 1848, at which time she had suffered from rheumatism already about seven months ; all the larger joints of the body having been sufficiently affected. About four months prior to her admission, she suffered a relapse, after having nearly recovered, and since then she has experienced but little alleviation of her ailments. Her neck as well as most of her larger joints are painful, though not much swollen. Her tongue is slightly furred, her pulse is 85 and soft ; her skin moist, and bowels regular.

On the 19th of January following, she had nearly recovered from her rheumatism, under treatment, except rigidity and contraction of the muscles of the right side of the neck, by which the head was drawn downwards and towards the right shoulder. To relieve this distortion, frictions with stimulating and oleaginous liniments were diligently employed, and, subsequently, sulphuric ether was applied to the neck. Some slight improvement resulted from the use of these means. On the 18th of April, however, the condition of the neck had for some time been stationary, and all hope of further benefit was abandoned. The motions of the head were very much restricted, and any attempt to overcome the resistance of the rigid muscles by stretching them occasioned severe pain. The rigidity did not appear to reside in the sterno-mastoid muscle, inasmuch as this muscle did not grow hard and stiff when efforts were made to elevate the head ; the resistance was evidently seated in the deeper muscular and tendinous parts.

At the request of my colleague, Dr. Swett, of the Medical Division, I saw this patient and proposed to make cautious attempts to overcome the resistance by force, the patient being first subjected to the influence of sulphuric ether. Considering the resistance to depend on contracted muscular and tendinous fibres, my object was either to stretch or rupture them, and in doing this, no danger was apprehended to the important nerves and blood-vessels of the neck ; since the forced movement necessary to accomplish this object would fall far short of the extensive motions in every direction to which these parts are accustomed naturally to accommodate themselves.

Dr. Swett assenting to my proposal, the patient was laid upon her back in bed, with her head resting high up on a pillow, so as to be easily got at from the head of the bed. Taking the head between my two hands placed one on either side, I cautiously stretched it with a very moderate degree of force in

the direction opposite to that in which it was distorted, that is, upwards and to the left side. Almost immediately every one standing round the bed (of whom there were at least eight or ten pupils and medical men), was startled by a loud snapping sound of some thing rupturing, and at the same time I perceived that the head yielded, and could be brought almost to its natural position. It was thought prudent to proceed no further at this operation. The patient on recovering her consciousness was not sensible of any new soreness in the parts, and could bear the head to be moved much easier than before the operation. She was directed to lie as much as possible on her left side. On the following day there was considerable soreness on the right side of the neck. On the 25th of April, one week after the first operation, the soreness of the neck having very much diminished, the operation was repeated a second time.

The proceedings were the same as in the first operation, only the stretching was carried to a much greater extent, and with a much less timid hand. Several times resisting fibres were felt to yield with a rupturing sensation, till, at length, no further resistance was encountered, and the head could be carried to the full extent in every direction. After the effects of the ether had passed off, the head was bandaged down towards the left shoulder. On the first of May, the bandage being dispensed with, the head showed no disposition to resume its distorted attitude. On the 10th of May (1849), the head could maintain unaided its erect natural position, though rotation and flexion were still limited in extent, and performed awkwardly; the patient, however, was sensible of the progressive improvement in these respects. She took her discharge from the hospital for the purpose of returning to her friends in Ireland. About one year afterwards she was heard from as continuing well, and free from any distortion or rigidity of the neck.

CASE II. In January, 1852, Maria P——, of Guilford, Connecticut, aged 12 years, and of a healthy constitution, came under my care, with the head very much distorted from being drawn down towards the chest, with the face turned to the left side. The motions of the head were also very much restricted. In the month of July preceding, she had been attacked with sore throat and stiff neck, that left her ever since in the condition just described. She had never suffered from rheumatism in any other part of her body, and had generally enjoyed good health. I at once decided to employ the treatment which had been so successful in the preceding case; and on the 15th of January, having first etherized my patient, I performed the first operation. In order to carry the extension of the head to the requisite degree, it became necessary to have her supported in

the sitting posture in a chair, and to place myself in front of her. Grasping the head between my hands, I acted on it in the various directions in which resistance was encountered, but felt no sensation of rupturing fibres, in this or in any of the subsequent operations. The resisting parts, however, yielded in some measure, and allowed the head to be brought more nearly into its natural position. No pain was experienced from the operation on recovering her consciousness.

On the 19th, no effect was observable from the first operation; it was therefore repeated a second time, with the aid of ether. On the 21th, 26th and 30th of January, and on the 4th and 7th of February, it was also repeated, each time with the aid of ether. Though a gradual improvement was perceptible from these repeated operations, it became evident that a complete cure could only be achieved by a patient and persevering repetition of them for a long time; it was therefore judged most prudent to continue the operations without the aid of ether. The patient's courage and endurance, though put to a severe test, proved adequate to the trial. Once every day she submitted with the most admirable fortitude to the stretching process, for about ten minutes each time. This was continued up to the 1st of March, after which it was repeated twice every day. The manner of manipulating was as follows: The patient was seated in a chair, and her body steadied by an assistant standing behind her and holding her shoulders firmly with both hands. Placing myself in front of her, I grasped her head with my hands in such a way as to perform most efficiently the different movements I wished to execute. These movements were varied in every direction in which resistance was encountered, my object being to stretch to the utmost the contracted muscles, and to maintain them on a stretch for a certain length of time. The process was painful only during its actual performance, and ceased to be so the moment it was discontinued. On the 24th of March, the operations were suspended, while the patient made a visit to her family, and were resumed again on the 8th of April. During this interval no relapse took place. The same course of treatment was continued till the 10th of May, when she returned to her home, highly gratified at being able to maintain her head by her own efforts in its natural erect position, and to turn it in different directions almost as well as ever she could. She was advised to continue for a long time the daily practice of performing the various motions of the head as extensively as possible. On the 13th of January, 1853, I conversed with an aunt of my patient, who had recently visited her, and who reports that she holds her head in a very natural manner, and can move it at pleasure

freely in every direction. In a word, she considers herself quite well again, and without any disposition to relapse.

[*New York Medical Times.*

Mr. BOWMAN'S *New Operation for the Division and Removal of False Membrane or Opaque Capsule from the axis of Vision.*

A new and very simple method of effecting the above object has been recently introduced by Mr. Bowman, and been attended with complete success. It is well known how tough and resisting these bands and portions of membrane often prove, so that a cutting needle fails to divide or tear them, though it may drag them from one side of the pupil to the other, the structure yielding to a certain extent, and then returning to its place on the withdrawal of the instrument. The iris and ciliary body to which these membranes are usually attached, are liable to be stretched and injured under such handling, and we have even seen the iris detached from its ciliary connexions; serious inflammation is also prone to follow the operation.

The peculiarity of Mr. Bowman's method consists in the use of two ordinary cataract needles simultaneously. They are introduced through different points in the cornea down to the band of membrane, and their points having entered it at the same or neighboring situations, as convenient, are then separated, tearing it across, and carrying it in two fragments to opposite sides of the pupil. Thus there is no drag put upon the vascular parts, and the only tissues touched are the cornea and the opaque membrane itself. One needle may be made to furnish a fixed point of resistance from which the other can act with advantage. During the last ten days this method of operating has been successfully adopted in seven instances, three of them were cases in which, after the removal of the lens by drilling, the pupil still continued closed by false membrane; and in the other four the sight was more or less interfered with by bands of lymph or tough opaque capsule passing across the axis of vision behind the iris.—[*Western Lancet.*

ESCALIER'S *Proceeding in Asphyxia from the Inhalation of Chloroform.*

Professor Rigaud relates in the *Abeille Médicale* of Nov. 3d, 1851, the following case:—

Six months ago I was going to operate upon a woman for a tumor in her breast, and caused her to take chloroform. After a few inspirations the pulsation ceased suddenly, and the pa-

tient gave no sign of life. The chloroform was immediately removed, cold water thrown into the face of the woman, and frictions made over various parts of her body. After two minutes, which seemed so many long hours, these manœuvres produced a few feeble contractions of the heart, which soon ceased again, and were not accompanied by a single appearance of respiration. In this fatal condition it occurred to me to resort to the treatment of Dr. Escalier. I introduced my right index finger into the mouth of the asphyxied patient, made it pass along the base of the tongue, and lift the epiglottis. Then I pulled the tongue out of her mouth. This rapid motion caused an inspiration, which I made use of to let her inhale Ammonia. But as soon as I let go the tongue it slipped back, and the respiration ceased again. I repeated the same manœuvre, and produced again respiration. But this time I retained the tongue outside of the mouth, and respiration continued to go on; after which, the woman recovered very soon, and all functions resumed their normal actions. Then I made the intended operation, without chloroform, and every thing went perfectly well. I have no doubt, that in this case, the life was saved by the treatment recommended by Dr. Escalier.—[*Ibid.*]

The Effects of the Oil of Turpentine on Persons exposed to its Vapor. By ELISHA HARRIS, M. D., Physician to the N. Y. City Dispensary.

During the past few years there have come under my observation and care, several interesting cases of strangury, hæmaturia, &c., resulting from continued exposure to the vapor of the oil of turpentine. Some points of interest connected with the history of these cases, may justify the following brief account of them.

CASE. A laborer employed in unlading a cargo of turpentine, began to suffer from vesical irritation and dysuria, at the close of his second day's labor in the ship; and at the end of the third day, when he applied to me for relief, he presented all the symptoms of acute inflammation of the bladder. The urinary organs were in a state of excessive irritation; there were almost constant efforts of micturition, with the passage of but a few drops of bloody urine—or more frequently, of blood only. The patient complained of severe pain in his back and loins, and he suffered from nausea, vertigo, and impaired vision.

The poor fellow had no idea of the true cause of his sufferings, but was confident that he had been maliciously poisoned. Upon inquiring, I learned the nature of my patient's employment, and advised him to desist from his labors in the ship. I

prescribed copious draughts of an anodyne and demulcent drink, a tepid bath, &c. In the course of forty-eight hours he had fully recovered. Immediately after this, there occurred in the same ship three similar cases.

By inquiry, I have since learned that the occurrence of such cases on board of vessels freighted with turpentine, and also among laborers employed in unlading such cargoes, is no unfrequent event in warm weather. The late Dr. F. P. Colton reported an interesting case that came under his care in the N. Y. Hospital. (*Vide Colton's N. Y. Hospital Cases. N. York Jour. Med., Vol. V., No. 3.*)

Recently I have had occasion to treat several cases of strangury, hæmaturia, eczema, ovaritis, &c., occurring in artisans whose labors expose them to the vapor of turpentine. They were employed in an extensive manufactory of India Rubber goods, where camphene is used as the solvent of the gum caoutchouc. Two of these patients were young females, and in them turpentine seemed to have induced very grave constitutional irritation. Long continued insomnia and *malaise* had induced a state of nervous and mental irritability bordering on mania, and almost characteristic of the difficulty. During the first three months that these girls were employed in the factory, they suffered from *dysmenorrhæa* and *menorrhagia*; and subsequently, at the time they came under my care, they suffered from *ovaritis* and *leucorrhæa*, all of which had undoubtedly been caused by the terebinthine vapor which they were obliged to respire, while in the discharge of their duties. The bodies of both these patients were extensively covered with an eczematous eruption, which soon disappeared, with all the other unpleasant effects of the turpentine, on their leaving the factory.

One of the young men employed in this factory, suffered a higher degree of irritation of the urinary organs than I ever before witnessed. After repeated attempts to continue at his labors in the factory, he was obliged, ultimately, to leave the establishment.

The interest excited by these cases has led me to make inquiries, in reference to the prevalence of the irritating and poisonous effects of turpentine among artisans who are exposed to its vapor.

The intelligent conductor of an extensive manufactory of India Rubber goods in this city, informs me that about two thirds of all his employees suffer considerable vesical irritation, strangury, vertigo, nausea, &c., during the first few months of their employment in the factory, and that after a time they nearly all become "*seasoned*," so that the terebinthine air no

longer affects them. In this establishment it is the practice of those who suffer from the effects of the turpentine, to take large and frequent doses of olive oil.

In the India Rubber factories of New Jersey, where camphene is used as the solvent of the caoutchouc, the same facts are observed as in this city.

In an extensive manufactory in New England, in which camphene is no longer used, I learn that when the terebinthine preparations were used, the workmen suffered very much. In this establishment the workmen were in the habit of anticipating the effects of the terebinthine vapor, by subjecting themselves to a process of "seasoning," by taking repeated doses of camphene. In this and other manufactories, I find that females do not seem to have suffered as much as males from the peculiar *local* effects of the turpentine, but they have suffered much more from its effects upon the nervous system and the skin.

In connection with this subject, I would record a case in which a large quantity of camphene was swallowed by a child, with almost fatal consequences.

D. H., a boy aged three years, stealthily seized a small can which he supposed to contain molasses, and drank nearly or quite *six ounces* of camphene.

I saw the boy within an hour and a half after the accident. He was then in a state of profound *coma*,—the eyes were suffused, the pupils largely dilated, conjunctiva injected, the pulse 130, the skin hot and dry, the mouth and fauces red and parched, the features were somewhat corrugated and anxious, respiration hurried; the abdominal muscles were very tensely contracted, and he lay curved, with his arms closely folded and pressed on his abdomen. Urine that had been voided a short time previous to my arrival had the odor of violets. I administered a large quantity of olive oil, and followed it with an emetic of ipecac. and warm water. The matters ejected had the odor of turpentine, as did also the dejections from the bowels.

The child could not be kept awake, even while vomiting. There were frequent and ineffectual attempts at micturition, and there was *constant priapism*.

I directed a tepid bath, and prescribed demulcent drinks, with ipecac. gr. j, every fifteen minutes. This treatment was continued for about eighteen hours, when the little fellow awoke to consciousness for the first time in nearly *twenty hours*. Less than four ounces of urine had been secreted during this comatose period; and what was voided was tinged with blood. For several days the patient continued to be excessively nervous and irritable; but at the end of a week he had to all appearances fully recovered.

The facts in this case might be of some importance in a medico-legal point of view, as there are but few such cases on record. One very similar but less severe case, is mentioned by Mr. Taylor in his treatise on Poisons. (Vide "*Taylor on Poisons*," by Griffith, p. 426.)

The history of my case, as well as that of Dr. Evans, quoted by Mr. Taylor, exhibits striking proof of the energy with which the oil of turpentine acts upon the nervous system of persons exposed to its action.—[*New York Jour. of Medicine*.

New Facts relating to Percussion in Pneumonia, Emphysema, etc. By H. ROGER.

1. The pulmonary parenchyma, even *containing less air* than in the healthy state, gives, on percussion, a more or less evident tympanic sound: a portion of lung infiltrated with serum, or blood, or tubercular matter, and *not entirely deprived of air*, furnishes a tympanic sound more or less hollow or dull, in proportion to the quantity of air. In percussing the thorax, tympanic resonance will there be obtained in some cases of pneumonia, of tubercular infiltration, and of pulmonary œdema and apoplexy.

2. The natural pulmonic sound is not tympanic. The lung, distended more than its healthy state (vesicular emphysema), furnishes a sound sometimes tympanic and sometimes non-tympanic. *Partial emphysema* surrounding an engorged and non-aërated parenchyma (which may happen in pneumonia), gives rise to a resonance which is usually tympanic; while this rarely occurs in *general emphysema*, and is never present in *interlobular emphysema*. When, in emphysema, the pulmonary tissue is excessively distended by the air contained in the air-cells, and at the same time the thoracic walls are very tense, the thoracic resonance, instead of being exaggerated, as might *à priori* be expected, is diminished; thus is explained (and not by the presence of false membranes covering the lung) the small amount of sound which is sometimes found on percussing the chests of patients, who are nevertheless highly emphysematous.

3. In *pneumothorax*, the pectoral resonance is tympanic when the chest on the affected side is moderately distended, but scarcely ever when the tension is extreme.

4. When the *abdomen* is percussed, the varying tension of the abdominal wall may cause the resonance to vary: the more tense the wall is, the more obscure is the sound. The same occurs with regard to the resonance in cases of *pneumatoxis*.

[*Archives Gén. de Méd. London Jour. of Med.*

Researches on the Pathology of Rheumatic and non-Rheumatic Pericarditis. By Dr. ORMEROD.

Recently before the Royal Medical and Chirurgical Society of London, Dr. Ormerod read a very interesting paper on these affections. He commenced by a reference to the researches of the late Dr. Taylor, who had satisfactorily shown that acute rheumatism was not exclusively the cause of pericarditis, and who had also called attention to the importance of granular disease of the kidney in reference to this morbid condition. The author desired to limit the use of the word pericarditis to present inflammation of the pericardium; and this analysis referred exclusively to cases of this nature. The means of investigation comprehended complete records of 1410 cases observed under nearly similar circumstances; that is, in the wards of different hospitals. Of these, 1249=88.59 per cent. were not cases of rheumatism; 161=11.41 per cent. were admitted on account of rheumatism, or suffered from it while under observation. Of the whole number, 85=6 per cent. had recent pericarditis, observed during life, or discovered after death, and were thus distributed:—

24 = 1.92 per cent. occurred among	1249 non-rheumatic cases.
61 = 37.88 per cent.	161 rheumatic cases.
85 = 6 per cent.	1410

The mean age of 61 subjects of rheumatic pericarditis was about 21; the mean age of 24 subjects of non-rheumatic pericarditis was 42; the extremes being 7 and 63 years. As to the different causes of the pericarditis—

Rheumatic.....	61 cases coincided with acute rheumatism.
Non-Rheumatic of local origin,	<div style="display: inline-block; vertical-align: middle;"> <div style="font-size: 3em; vertical-align: middle; margin-right: 5px;">{</div> <div> 7 ensued on inflammation of lungs or pleura. 2 ensued on malignant disease of the pericardium. 1 ensued on old cardiac disease. </div> </div>
Non-Rheumatic of constitutional origin,	<div style="display: inline-block; vertical-align: middle;"> <div style="font-size: 3em; vertical-align: middle; margin-right: 5px;">{</div> <div> 6 coincided with granular disease of kidneys. 4 coincided with hæmorrhage or exhaustion. 2 coincided with scarlatina or irrisipelas respectively. 2 were inexplicable. </div> </div>

85

The date of the accession of pericarditis was determined in 33 of the rheumatic cases. The mean of these observations gave the 10. 5th day of rheumatic attack as that on which the pericardial complication most commonly supervened. The question, whether a first or second attack of rheumatism was more likely to be accompanied by pericarditis, was beyond the reach of hospital statistics. This source of information was silent also on the question, whether pericarditis be more likely to occur in severe or in the slighter cases of rheumatic fever.

It might, however, be safely inferred, that the severity of the articular and pericardial affections bore no very close relationship to each other. It was certain that the most severe, even fatal pericarditis, might occur where there was but faint evidence of articular affection, and this latter condition might exist in the most aggravated and intense form without involving the addition of pericarditis to the other sources of distress. The author then entered upon the consideration of the subject of non-rheumatic pericarditis of local origin; and a question of importance here presented itself—What was the influence of preëxistent cardiac or pulmonary affections in inducing inflammation of the pericardium? The question was of equal importance in relation to acute rheumatism. The relation of pulmonary inflammation to pericarditis was thus illustrated:—In the 1410 cases, the basis of this inquiry, some form of pulmonary inflammation—that is, pneumonia, pleuritis, or pleuropneumonia—was ascertained to exist, either by auscultation or dissection, in 265 cases. Of these—

117 had pneumonia,	of which 19 had recent pericarditis.
86 had pleurisy,	" 6 "
62 had pleuro-pneumonia	" 8 "

265

33 = 12.4 per cent.

In the rheumatic class, pericardial inflammation commonly preceded, yet sometimes, though rarely, followed, pulmonary inflammation. The non-rheumatic class told quite a different story; here pulmonary inflammation had apparently a distinct influence in inducing pericarditis, and this influence was most evident in cases of pleurisy; and clinical observation bore out the conclusion, that the pericarditis was subsequent to, and probably contingent on, the pulmonary inflammation. The author then referred to the comparative fatality of non-rheumatic compared with rheumatic pericarditis, and also to the desirableness of instituting an exact comparison between Bright's disease of the kidney and acute rheumatism, in respect to their tendencies to induce inflammation of the pericardium. In conclusion, the author desired to ascertain how far the results obtained by his present analysis agreed with those of the published cases of Dr. Taylor, who had made the subject of non-rheumatic pericarditis so peculiarly his own. The deductions seemed identical, and one rose from the perusal of those elaborate clinical reports with a conviction that non-rheumatic pericarditis was more within the province of the anatomist than of the physician. It was a disease with few or no symptoms, its physical signs recognized more often by a chance discovery than on the suggestions of the disease, and its morbid changes

small in amount and apparently inactive; and where opportunity had occurred of watching the disease some time previous to death, it had been apparently without effect on the general symptoms, its presence or absence being determined by the ear alone; and still, in these, its connection with the fatal termination had appeared to be that of a coincidence rather than of a cause.

Dr. Copland felt obliged to the author for the very practical and literary way in which he had brought his paper before the Fellows of the Society. The association of disease, as exemplified in Dr. Ormerod's communication, showed us that, in practice, we must not look at cases of disease as always simple, but frequently as complicated as those under discussion. All these were connected with the morbid condition of the blood, and to this we must look as the cause of the articular rheumatism, the pericarditis, pleuritis, &c. All these were evidences of the blood being in an abnormal condition, from whatever cause it originated. Several organs became affected, and when an important disease existed, it masked the minor one. We saw the same train of phenomena in Bright's disease, in which inflammation of the serous membranes was liable to occur from the non-elimination of morbid matter from the blood, and its consequent circulation through the system. These combinations of disease should be viewed in our routine of practice as the result of the morbid action in the system.—[*Dublin Medical Press.*

Case of Total Removal of both Radius and Ulna. By Dr. COMPTON. (Reported by A. THIBAUT.)

Thomas Harris, æt. 15, admitted during the month of February, for a lacerated wound of scalp and ear; fracture of inferior maxillary and humerus; compound comminuted fracture of radius and ulna.

These injuries were received on board the English ship Manchester. It appears that the boy was sleeping on the anchor chain; and that the anchor was suddenly let down; in its progress, the chain caught the arm and produced the injuries above mentioned. When the boy was admitted, the wounds had been dressed for several days, and from want of proper attendance and care, were in a very filthy condition. The arm, especially, was in a sloughing state, and both radius and ulna were actually shattered to pieces, and protruding several inches out of the mass of muscles.

By the 18th of April, the boy being well of all the other inju-

ries, Dr. Compton determined to remove both the radius and ulna. He made a straight incision the whole length of the inner side of the radius, and a counter-opening opposite the olecranon process. Having dissected out both bones carefully, and disarticulated them at the elbow, he removed them entire with the exception of a portion of the lower end of the radius. A great portion of the periosteum was detached from the bones, and left in the wound. The usual treatment for such operations was then followed and the patient improved rapidly, and the wound had nearly healed, when several abscesses formed on the fore-arm. These abscesses were, according to Dr. C.'s opinion, produced by pieces of bones which had been left in the arm. His opinion was well grounded, for several spicula of bone came out of the wound, and the arm immediately assumed a healthy condition, and is now well. The arm is about two or three inches shorter than the other and is perfectly firm. It remains at a right angle to the humerus, and can be flexed and extended so that the hand moves through eight or ten degrees of an arc of a circle. He has entire use of the hand—he can both open and shut it, and he grasps objects quite firmly. The pulse in that arm can be felt as well as in the other.—[*New Orleans Med. Register.*

Case of Dislocation of the Eye. By WILLIAM JAMESON, M.D., F.R.C.S., Surgeon to Mercer's Hospital. (From the Proceedings of the Surgical Society of Ireland.)

Dr. Jameson said he would detain the Society but for a very short time in detailing a case of dislocation of the eye, which had lately come under his observation in Mercer's Hospital:—Peter Nowlan, ætat. 30, a powerfully able and muscular man, a corn porter, was admitted into Mercer's Hospital on the 3d of November, at half-past 12 at night. His wife informed me that he came home that evening at ten o'clock in a most intoxicated condition, and while staggering about his room, struck his right eye against a small iron hook or nail that was in a dresser, which entered at the outer angle of the upper eyelid of that side, and when she went to his assistance discovered his eye protruded from its socket. She was most anxious to remove him at once to hospital, but could not succeed in prevailing on him to go until half-past twelve at night, when in a few minutes after this I saw him.

He was very boisterous and unruly, had a large check apron held close up to his eye, which he kept constantly rubbing and pressing against it. On its being removed, he presented a most peculiar, and I might add, frightful appearance. There

was the right eye protruded out of the orbit, firmly fixed and immoveable, staring, elastic to the touch, and devoid of all power of vision. The cornea was dry, cloudy, and rather opaque, pupil moderately contracted, and uninfluenced by the light of a candle. There was no extravasation of blood, nor was there any vascularity of the conjunctiva, although its reflection from the upper lid on the globe of the eye was partially torn through. The inferior margin of the upper lid was not visible, as it was placed behind the globe and spasmodically closed.

With difficulty I could get him restrained, as he was such a powerful man, but having accomplished it, I then, with two fingers of my left hand, elevated the upper lid, at the same time, with the finger and thumb of my right, pressed the ball of the eye, and immediately it was drawn back with a distinct snap, and the lids closed over its anterior surface. I now, for the first time, observed the small wound before alluded to at the outer angle of the upper lid, but could not ascertain or form any conjecture at the time what amount of injury he might otherwise have sustained. I therefore had him conveyed to bed, and ordered cold to be assiduously applied to the part for the remainder of the night.

4th. The following morning, at visiting hour, we found him sober, but recollected little of what had occurred. His eyelids were a little swollen; there was some slight vascularity of the conjunctiva; the *cornea was clear, shining*, and moist, and the tears ran down the cheek; he could distinguish the daylight; complained of pain in the head, and a deep pain in the globe of the eye, with full pulse. He was ordered to have $\frac{3}{4}$ xvi. of blood taken from his arm, bowels to be freely opened, and the cold to be continued to the part.

5th. Lids less tumid; pain and vascularity of conjunctiva almost gone; complains of the sensation as if gravel were between the lids; vision improved, but sees objects imperfectly, as through a thick haze. Ordered the tart. ant. mist., low diet, and the cold application to be continued.

6th. All pain gone; conjunctival vascularity less; sensation as if gravel were beneath the lids gone; vision nearly restored; has complete power over all the motions of the eye. Continue all.

7th. Convalescent; no suffusion; no pain; vision complete.

9th. Discharged cured.

The foregoing case I consider to be one of great interest, when we reflect on the novelty and nature of the accident, and the mode of its being inflicted. In the first instance, the great escape the orbital plate of the frontal bone had of being

pierced, and consequent injury to the anterior lobe of the brain. Again, the length of time the cornea was left uncovered by the palpebræ, being two hours and a half, and all that time coarsely rubbed by the apron. The great state of tension the optic nerve must have been kept in without permanent loss of vision. The escape the muscular attachments had of being torn from their origins, which evidently must have been the case from the subsequent perfect control retained over all the motions of the eye, as soon as the very slight amount of inflammation produced by the accident was removed. The powerful contraction of the orbicularis muscle behind the globe, with the complete restoration of vision. And finally, the trifling amount of constitutional disturbance and local inflammation that followed what appeared to be at first sight so very grave an accident to so very delicate an organ. These, I say, are points which add considerably to the interest of the case.

Dr. Jacob said the Society were greatly indebted to Dr. Jameson for the very interesting case he had just brought under their notice. As far as he could recollect, one of exactly the same kind was scarcely to be found on record. He thought it was a case which ought not to be dismissed from their consideration, without some suggestion to account for the occurrence of such an accident. He would solve the matter in this manner: Some persons were born with very large eyes and shallow orbits, and often, while examining the eyes of such persons, he found that by pressing the lids above and below, he could with ease get a view of the back of the eye. It was not that he merely saw one-half of the eye, but by a little manipulation of the eyelids, in persons with a shallow orbit and a large eyeball, he could obtain a view of the posterior part of the ball. Now, if by means of violence, the lids were tucked in above and below, they would grip the back of the eye and produce a downright protrusion of the organ from the orbit. He could not conceive any other way in which the accident could happen, because, as they might recollect, neither the muscles nor the optic nerve were torn in the case described by Dr. Jameson.

Dr. Jameson—The circumstance of the eye being drawn back with a distinct snap, shows that the muscles were at least on the stretch.

Dr. Jacob—Yes, that is quite evident.—[*Dublin Med. Press.*]

On the Prevention of Puerperal Mania. By Prof. SIMPSON.

Recently, before the Edinburgh Obstetrical Society, Dr. Simpson gave an account of the use of chloroform in patients

predisposed to puerperal mania. He stated the particulars of three cases in which chloroform was used in patients, who in former confinements had been the subjects of puerperal mania. In none of these cases did the disease return. One of them had borne several children previously; and after each labor, had been attacked with puerperal mania, but had entirely escaped from any recurrence whatever of the disease after the last labor, in which he had employed chloroform.—[*Monthly Journal of Medical Sciences.*

On the Transformation of the Encysted Entozoa into Tape-Worms. By JNO C. DALTON, Jr., M. D.

The following report of some experiments by Prof. C. Th. von Siebold, will be found of considerable importance, not only from their general zoological interest, but more particularly from their bearing on the question of spontaneous generation. Notwithstanding that the idea of spontaneous or equivocal generation is repudiated by the great majority of physiologists at the present day, it seems to be still entertained in some quarters, even by men of considerable scientific attainments. The defences of this theory, however, have one after another given way before the advance of zoological science. It was demonstrated long ago by Redi and Valisnieri, that the worms, which appear in putrefying flesh, were not produced, as previously supposed, from the decomposing animal matter itself, but were the larvæ of winged insects, which had been led, by a curious instinct, to deposit their eggs in such places as would afford the necessary food to the young and imperfect progeny during the earliest stages of its existence. The Italian observers thus explained, in the most natural manner, what seemed to be a very puzzling circumstance, viz., that these worms, which always showed themselves in great numbers in decomposing animal substances, were not to be found in any other situations. The species seemed to be confined to putrefying substances; and as they often appeared under circumstances which precluded the idea of their having been transferred from other collections of decomposing matter, it was not easy to understand how they could have originated by the ordinary mode of generation. Redi and Valisnieri, however, demonstrated that they were the progeny of perfect insects; and that the *species* was not in reality confined to decomposing substances, but existed elsewhere, though in a different form.

The existence of the infusorial animalcula, again, seemed for a time explicable only on the supposition that they were produced spontaneously in the animal or vegetable infusions which

they inhabited. But this supposition has been set entirely at rest since the experiments of Schultze, at Berlin, in 1837, proved that though the production of infusoria was almost invariable when the infusion was kept at the proper temperature and exposed to the access of atmospheric air,—yet that the animalcules were not generated if sufficient care were taken to preclude all possibility of living germs being introduced into the infusion from without. We take it, therefore, not even the most determined advocate of the theory of equivocal generation will place much reliance on the celebrated experiment of the production of the *acarus Crossii* from the continued action of galvano-electricity on a crystalizable saline solution ;—particularly as the acari so produced possessed generative organs, and the females, soon after their appearance, provided for a continuance of the species by an abundant production of ova. The chances of the accidental introduction of living germs into the machine during the course of so long-continued an experiment are too great to allow any one to remain satisfied without a personal inspection of the apparatus.

The only remaining defence of the spontaneous generation theory, is the existence of *entozoa* ; and it is behind this last, and apparently most impregnable barrier, that its partizans have finally entrenched themselves. The entozoa, like the infusoria, are confined to certain situations. They are never detected out of the living body. Particular species of animals are even infested by particular species of parasites, and by no others. The *Taenia solium* inhabits the intestines of the human subject, the *T. serrata* those of the dog, the *T. crassicollis* those of the cat. In the same individual, even, different organs are occupied by different entozoa. We must look for the *Trichocephalus dispar* in the cœcum, for the *Strongylus gigas* in the kidney, and for the *Distoma hepaticum* in the liver. “These facts,” it has been said,* “seem to show that some extremely local concurrence of circumstances is essential to the production of the several entozoa.” But it is very easy to see that these strictly local conditions may be not at all necessary for the *production*, but only for the *development* of the entozoa. It is certainly no more surprising that one species of *Ascaris* should inhabit the large, and another the small intestine, than that the *Lobelia inflata* should grow only in dry pastures and the *Lobelia cardinalis* by the margin of meadow-brooks. The Lichens flourish on the exposed surfaces of rocks and stone walls ; while the Fungi vegetate in darkness and moisture on the decaying trunks of dead trees. Yet no one imagines

* Stillé's Pathology, Philadelphia, 1848, p. 473.

these vegetables to be spontaneously generated from the soil which they inhabit. The fact is simply this: that if the animal or vegetable germ be deposited in a locality which affords the conditions necessary for its development, it becomes developed; otherwise not. The grains of wheat which had remained for centuries, wrapped up in the cerements of Egyptian mummies germinated freely when exposed, in an appropriate soil, to the influences of light, air, warmth and moisture. The circumstance, therefore, that particular parasites are confined to particular localities presents no greater difficulty as to their mode of reproduction, than the same fact regarding other animal and vegetable organisms.

Every articulation of the *Tænia solium* contains, when in a state of maturity, many thousands of ova; all of which are necessarily expelled from the body when the articulation drops off. Now though the chances are enormous against any particular one of these ova being accidentally transported into the intestinal canal of another individual, it is easy to see that there are many causes in operation by which *some* of them might be so transported. By far the greater number undoubtedly perish, from not meeting with the conditions necessary to their development. One in a thousand, or, perhaps, one in a million, is accidentally introduced into the body of another individual, and consequently becomes developed into a perfect animal.

The greatest difficulty, however, was presented by the *encysted entozoa*, which are not only confined to particular organs, like other parasites, but which are also destitute of any generative apparatus. Now, a species which is destitute of generative organs evidently cannot reproduce itself; and the encysted entozoa have therefore been regarded as presenting at least one undoubted instance of equivocal generation, i. e., a progeny without parents. The fact that no similar animals were found external to the body might have been got over; but that those existing as parasites in the parenchyma of living organs were themselves destitute of any generative apparatus, seemed to exclude the idea that they had been produced by the ordinary modes of propagation.

It is strange that those who advocated so strenuously the doctrine of equivocal generation could not see that this circumstance might be explained in the same way with the production of maggots in putrefying meat; a point which had been settled so long ago by Redi and Valisnieri. These maggots differ in structure from their parents because they are as yet incomplete. They may be considered, to some extent, as still in a fœtal condition. For the same reason the generative organs are not yet developed. The larva is incapable of repro-

ducing itself as a larva. But after it has passed through the natural transformations, and its organization is completed, a sexual apparatus appears, and the *species*, though not every individual belonging to it, is found to be perfect. Now, by watching the growth of any one individual, from the egg to the state of a complete insect, we get a history of its transformations, and comprehend that the animal may be destitute of sexual organs at one period of its existence, and provided with them at another. But if the conditions necessary to the later stages of development are wanting, the larva will not be transformed into an insect, but will remain a larva; and, of course, so long as these conditions are wanting, so long will the sexual organs remain absent. And this is precisely the case with the encysted entozoa. They bear very much the same relation to the *Tænia* as the undeveloped larva to the perfect insect. It is, perhaps, unnecessary to remind our readers that the *Tænia* is not now considered as a single animal, but as a colony of animals; every articulation being a distinct individual. These articulations are multiplied by a process of budding, which takes place just behind the "head," or most anterior individual of the colony. As new ones appear, those which were previously produced are pushed farther and farther from the "head;" so that the oldest articulations are those situated at the posterior extremity of the chain. The young and imperfect individuals compose the "neck" of the *Tænia*. They are, as yet, without sexual organs; but as they increase in size, and are gradually removed farther from the head, they become provided with a sexual apparatus, each articulation containing both male and female organs; so that the posterior portion of the chain is composed of completely developed hermaphrodite individuals. As these individuals arrive successively at maturity, they become detached from the chain, and pass out of the intestine with the *faeces*, after which the ova are probably set free by the death and decomposition of the parts which enclose them. A portion, then, of every "tape-worm" is destitute of reproductive organs, and yet it has itself undoubtedly been produced from ova, and will hereafter, if circumstances are favorable, produce ova in its turn.

Now the following experiments, by Prof. Von Siebold, demonstrate the very important point that those parasites which have been regarded as incapable of reproduction as a species, are really incapable of it only as individuals, because they have been prevented from arriving at their mature condition; and that the sexless encysted entozoa are in reality only undeveloped or diseased *Tænia*. We have ascertained by personal inquiry, what might have been anticipated from the previous

reputation of the observer, that both the experiments, and the results derived from them, are regarded by scientific men in Germany, as entitled to complete confidence. The following account of the experiments is translated from a report, in the "Silesian Times," of the Transactions of the "Silesian Association for National Instruction, Scientific Department, Session of July 7th, 1852:"

REPORT.

Professor Von Siebold made a report on the experiments which were undertaken some months previously, in the Physiological Institute, under his direction, for the purpose of showing the possibility of a transformation of the cystic parasites into tape-worms. He had already, in the year 1844, in the second volume of the Encyclopædia of Physiology, expressed the opinion that the parasitic cysticercus (*C. fasciolaris*), found in the liver of rats and mice, was nothing else than an abnormal, dropsical tape-worm; and that it was, in reality, identical with the tape-worm of the cat (*Tænia crassicolis*.) He maintained further that the *Cysticercus fasciolaris*, like all cystic worms, was invariably destitute of sexual organs, and could not multiply its species by generation, unless it were transferred to a favorable locality, where it might lose its dropsical condition and develop its sexual organs. These changes actually take place when a rat or a mouse, with a *Cysticercus fasciolaris* in its liver, is devoured by a cat. The parenchyma of the liver, according to Siebold, is digested in the stomach of the cat; but not so the entozoon. The parasitic animal loses only its dropsical appendage, and passes, with the digested food, from the stomach of the cat into the small intestine. It then finds itself in a favorable locality, and becomes developed into a perfect tape-worm, with articulations and sexual organs (*Tænia crassicolis*.) This idea had been first suggested to Prof. Siebold by the perfect resemblance between the cephalic extremity of the *Cysticercus fasciolaris* and that of the *Tænia crassicolis*; and by the fact that there are often found, in the intestine of the cat, several specimens of the *T. crassicolis*, in different stages of development. His opinion was adopted by many naturalists, but its correctness had also been called in question by others. Some years ago Dr. Kuchenmeister, of Zittau, had made use of the *Cysticercus pisiformis*, a species of encysted parasite very common in the peritoneum of hares and rabbits, for a series of experiments in which he caused these parasites to be swallowed by dogs and cats; in the expectation that they would become developed into tape-worms in the intestine of these animals. The trial succeeded perfectly with

dogs; and the same thing that Prof. Siebold had previously inferred from a comparison of the *Cysticercus fasciolaris* of rats and mice with the *Tænia crassicollis* of the cat, would seem to have been definitely established by these experiments of Kuchenmeister. But Kuchenmeister's experiments, and the conclusions he drew from them, did not prove satisfactory either to naturalists or medical men. He had committed the error of publishing his investigations before they could properly be considered as terminated. He was consequently obliged, in the various communications which he published on the subject, one after another, in the medical journals of northern and southern Germany, to correct many of his former statements, and even to retract some of them; and he entangled himself, finally, in so many contradictions that it is to be feared the doctrine of a close relation between cysticerci and tape-worms was rather retarded than advanced by his activity; particularly as he himself several times acknowledged that he was not sufficiently familiar with the study of intestinal worms to distinguish them with certainty. Such a confession certainly was not calculated to increase the confidence of naturalists in his experiments. He also exhibited his incapacity to distinguish the entozoa by giving, in succession, several different names to the newly produced tape-worm, which he described at first as the "*Tænia crassiceps*" of the fox, afterwards as the "*Tænia serrata*" of the dog, and finally as an entirely new species, under the name of the "*Tænia pisiformis*." Prof. Siebold then determined to undertake himself similar experiments. They were tried principally on young dogs, not only with the *Cysticercus pisiformis*, but also with the *C. cellulosa*, *C. tenuicollis*, *Cænurus cerebralis*, and *Echinococcus veterinorum*.

The following results were obtained from the experiments with the *Cysticercus pisiformis*. These entozoa, which are usually about the size of a pea, were given to young dogs, mixed with milk, still enclosed in their peritoneal cysts, and in quantities varying from thirty to sixty. The dogs were afterwards killed with chloroform, at various intervals of time, and the contents of the stomach and intestine being carefully examined, the entozoa were rapidly discovered in various stages of development. Two hours after being swallowed, they were almost all found still in the cavity of the stomach. The cysts, however, which had enclosed them, were gradually digested and destroyed, and at the same time, the greater number were not only freed from their envelope, but had also lost the vesicular portion of their posterior extremity. This vesicle was either entirely digested or else hung in shreds attached to the end of the body. All the entozoa, which were found in the

stomach, whether they had lost the vesicular appendage or not, invariably had the head and neck drawn back into the body.

When the dogs were killed after an interval of three hours, no entozoa were found remaining in the stomach. They had all passed, together with the digested food, into the small intestine. Their cysts and vesicular appendages had all been destroyed by the digestive processes in the stomach; but the head and neck were again, without exception, protruded, and the body, which had been before contracted, was stretched out longitudinally. In all of them were to be seen marks of injury at the posterior extremity of the body, where the vesicular appendage had been attached. When the dogs were suffered to live several days after the commencement of the experiment, the entozoa were found to have considerably increased in size. The largest had attained the length of three inches, the smallest that of one inch. The body, which had previously shown only transverse wrinkles, now exhibited very plainly articulations in its central portion. The posterior portion was still wrinkled transversely, and the lacerated spot at its extremity had assumed the appearance of a cicatrix.

After twenty or twenty-five days, the entozoa were already several inches long, and perfectly articulated quite to their posterior extremity, where the cicatrix was still evident; and on the posterior articulations were to be discovered traces of a sexual apparatus.

After eight weeks the cysticerci in the intestine of one of the dogs had attained a length of many inches. The largest were thirty-six to thirty-nine inches in length, and their posterior articulations were provided with a perfectly developed sexual apparatus, and contained many mature ova. Several of those a yard long had already thrown off their posterior articulations, with their mature sexual products. Von Siebold was now able to recognize, in this tape-worm, developed out of the *Cysticercus pisiformis*, the *Tænia serrata* of the dog. The cephalic extremity, the form of the articulations, the structure of the generative organs, and particularly the mature ova of this tape-worm, corresponded, in the most perfect manner, with the same parts in the *Tænia serrata*. There was no longer any doubt that the *Cysticercus pisiformis* of the hare and rabbit bore the same relation to the *Tænia serrata* of the dog, as the *Cysticercus fasciolaris* of rats and mice to the *Tænia crassicolis* of the cat. Furthermore, the *T. serrata* is rarely met with in the intestines of parlor and house dogs, but is, on the contrary, very abundant in hunting dogs; no doubt because the latter are often allowed to devour the entrails of hares killed in the chase, swallow at the same time the *Cysticercus*, and so become infested with the

Tænia; a circumstance which would naturally be less frequent with parlor and house dogs.

Although Siebold's experiments with the other species of encysted entozoa, mentioned above, were not entirely finished, he had yet carried them so far with the "*Cœnurus cerebralis*" as to convince himself that this worm, also, which is so much dreaded by sheep-breeders, becomes developed, in the alimentary canal of the dog, into a tape-worm. The tape-worm produced from this parasite had not yet, in Siebold's experiments, become developed to the stage of sexual maturity; so that he was still unable to determine with certainty its species. He hoped, however, by means of continued trial, to produce from the *Cœnurus cerebralis* perfectly mature Tænia; so that he might be able, after distinguishing their species, to determine what animal it is in whose intestine the sexless *Cœnurus cerebralis* becomes developed into a tape-worm with mature sexual apparatus. He will then, probably, have it in his power to give agriculturalists some hints how to prevent the development of this parasite in the brain of the sheep. For he is convinced that the encysted entozoa do not originate by spontaneous generation, but are produced from the microscopic ova of the tape-worms of certain carnivorous animals, which are introduced by accident into the bodies of rodentia and ruminantia. Here they are not developed into tape-worms; but degenerate into encysted worms, which exert a more or less injurious influence on the life of the animal, according to the importance of the organ in which they have taken up their residence at the expense of which they live.

The experiments which have been commenced with the *Echinococcus veterinorum* have already shown that this parasite is also to be considered as a tape-worm. The progeny of this destructive entozoon are produced, as is well known, in great numbers, by a process of budding from its inner surface. These were given in spoonfuls to young dogs, and in a few days afterward thousands of exceedingly small tape-worms were discovered, fastened by their four suckers to the mucous membrane of the small intestine. The bodies of these tape-worms consisted of only three divisions, viz., a head and neck for the first division, then a small articulation, and finally a longer one at the extremity. In both of these articulations the sexual organs had already begun to show themselves. They were not yet, however, so far developed that the worms could be considered as in a state of maturity, or their species accurately determined. It is Prof. Siebold's intention to continue these experiments; and he hopes, at some future time, to communicate their results to the Association.—[*Buffalo Med. Jour.*

On the Treatment of Typhus Fever by Sulphate of Quina. By Dr. F. M'EVERS, Physician to the Cork Fever Hospital, &c.

[In one of our preceding volumes (Part XXV., p. 17), the reader will find an interesting paper on the treatment of typhus by quinine, from the pen of Dr. Dundas, of Liverpool. There is great merit due to this gentleman in introducing this medicine as a remedy for typhus by seeking to identify the poison of this fever with the remittents and intermittents of the tropics. Dr. Skete, so far back as 1786, broached a similar opinion. This author says,]

"If the remittents of warm climates are but the continued fevers of this country, in a more violent degree, and if the effects of the bark are admitted in such remittents, does it not necessarily follow that bark would be endowed with similar powers, even in the fevers which every day occur to our notice, I mean those of the typhus kind, which are frequent in all large towns, especially London and Edinburgh."

[As we have before stated in the preceding volumes, Dr. Dundas' plan is to administer the quina in ten grain doses, until dizziness of the head, or tinnitus aurium, is produced; or until a general amelioration of the symptoms takes place. When tinnitus aurium or dizziness of the head are very urgent Dr. D. resorts to an emetic; and he says, if emetics are had recourse to at an early period, the quina is likely to be more successful.]

This treatment is resorted to in all stages of the disease, and frequently in the advanced periods, under circumstances which would be considered by the experienced physician as indicative of the worst form of typhus fever; and this mode of administering quina is almost invariably attended with the happiest results.

Immediately after having become acquainted with Dr. Dundas' views on this subject, several persons labouring under bad maculated typhus were admitted into our hospital, which gave me opportunities of testing the value of his opinions; and I must confess that I pursued the inquiry with much doubt, as I looked on some of the cures related by him to be of too marvellous a kind to justify my adoption of this treatment without further confirmation of its value: however, I have now tested the remedy in nine cases, and, with the exception of one, it has been signally successful. The first of these cases was the following, the notes of which I take from the daily reports of the hospital.

Pat Ryan, aged 28, a labourer, was admitted into the hospital on the 1st January, 1852, from Hop Island. His urgent

symptom is heacache; pulse 100; tongue foul; skin hot; had taken purgatives at home, and was treated since admission in the usual way with salines, ablutions, &c., until the thirteenth day of his illness, when the usual symptoms of bad typhus became apparent. On the previous day his skin was mottled, and now the entire surface has assumed a dusky hue. Pulse 112, feeble; tongue parched, with sordes on the teeth and lips; no sleep; bowels free; some general fulness of the abdomen, with epigastric tenderness; kidneys acting; headache increased, and raves a good deal. He was ordered to take ten grains of sulphate of quina every second hour, and to have broth and four ounces of port wine.

The changed condition of this patient at my next visit was most remarkable; the pulse was considerably reduced; the tongue was moist and cleaning, in fact it had lost the dark colour and parched appearance it presented the day before. The man slept; there was less abdominal fulness and tenderness; the kidneys acted well, and the bowels were free. He took sixty grains of quina, without its having produced dizziness or tinnitus aurium. The medicine was given in the form of mixture with a little sulphuric acid,—the two or three last doses sickening him a little. I then ordered it in the same dose in the form of pills, repeating it every three hours, which he bore without sickness. On this day he took forty grains, and on the following day he was convalescent. It is remarkable that the father of this patient, who was admitted a few days before, passed through the same type of fever, treated in the ordinary way, and died on the fifteenth or sixteenth day of his illness.

The second case was that of a young man, aged 19, admitted on the 14th Jan., for some days under my care, whose urgent symptom was headache, with great prostration of strength. On the eighth day his breathing became very much hurried, unattended, however, with cough, nor did the stethoscope elicit any abnormal sound: the man being remarkably pallid, except during two short exacerbations which occurred in the twenty-four hours. I ordered him aromatic spirit of ammonia mixture, with a small quantity of wine, and a sinapism to his chest. The following morning, on examining the chest, I discovered a purple patch occupying the part to which the mustard had been applied. His respiration was improved, but he complained of intense headache. Pulse 108, and feeble; no sleep. The purple patch induced me to make a careful examination of his body, and on turning him in the bed I observed the lower portion of the back and the nates covered with purple maculæ. The case I looked on then as well adapted for the administration of quina, and, accordingly, I ordered ten grains every two

hours, together with broth, and two ounces of port wine. The third dose produced violent headache, with tinnitus aurium, when the medicine was discontinued. At my next visit, on the following morning, all his symptoms were improved; the tinnitus aurium left him in a short time after the medicine was laid aside, and the headache was greatly mitigated; he slept, and expressed himself much better. I placed him again on the quina, when the third dose produced the same results as yesterday, but there was so great an amelioration of all his symptoms that I considered him proceeding to convalescence, and gave him only two grains of quina three times a day, after which he rapidly recovered.

The next two cases were females, both of whom presented unfavourable symptoms, and in whom the remedy was equally successful.

The fifth case was one of great interest, exhibiting other symptoms of an unfavourable nature, in addition to those enumerated in the preceding.

John Eames, aged 55, a smith, was admitted on the 23d of January, with bad typhus, having been discharged from hospital on the 3rd inst., cured of ordinary fever with bronchial complication. On the ninth day of his second attack he became mottled and talked incoherently; on the tenth day he was not improved; he had no sleep; the tongue was parched and dark, and protruded with difficulty; stools involuntary; pulse 120, and feeble. He was ordered to take ten grains of sulphate of quina every second hour, and to have broth and two ounces of port wine.

Half-past 9, p.m. The quina was commenced at 1 o'clock; he has taken fifty grains, and appears improved in every respect; he is more collected, and speaks with less difficulty; he has had two large voluntary evacuations from the bowels; the medicine was directed to be continued.

On the 11th day, I found that he had taken sixty grains since my last visit, and although he did not sleep much, and had some singultus in the night, he was much improved in other respects. The pulse had come down to 100; the tongue was still parched and brown, but he was perfectly conscious; he was very deaf and the maculae were improved in colour. On this day he took only four doses of quina, it having been omitted on account of the "buzzing," as he expressed it, which the remedy produced.

12th day. Pulse 96; tongue moist and cleaning; bowels open, and he passes water freely; to take five grains of sulphate of quina every fourth hour.

13th day. Pulse 84; tongue clean; skin cleaning and scaly; convalescent.

I will not occupy time with the details of all the cases in which I have tried this remedy, but will content myself with the recital of one more case, which occurred within the last few days, and which I consider in every way worthy of observation.

Mary Delany, aged 22, admitted from Ballincollig on the 10th March; nine days ill; headache; petechiæ; pulse 120, and very feeble; tongue parched; got the usual aperient of the hospital.

10th day. One stool; pulse 130, feeble; tongue parched and split; great thirst; respiration hurried; had no sleep; she is often flushed, and in the intervals deadly pale; countenance anxious, with the appearance of suffering; great fulness of abdomen, with tenderness on pressure, especially at the epigastrium; maculæ of a dark brown. She was ordered ten grains of sulphate of quina every second hour, and to have broth and two ounces of red wine.

11th day. Bowels not open; tongue moist and cleaning; pulse 100; kidneys acting.

12th day. At eight p. m. last evening this girl became very stupid; "did not know what to do with her head," as she expressed herself; she also became deaf, and had tinnitus aurium; this state continued until midnight, with occasional sleep, when she became "lighter," and afterwards slept well; the kidneys have acted, but there is no discharge from the bowels; she now presents a totally different appearance from that of yesterday; she is free from headache; her respiration is natural, the tension and fulness of belly have disappeared; but there is still some slight epigastric tenderness; some of the maculæ have disappeared, and the remainder are of a lighter colour. The sulphate of quina to be continued every fourth hour.

13th day. No stool; tongue clean and moist; pulse 84; belly natural, slight epigastric tenderness. The quina to be omitted; broth and wine to be continued; to have a domestic enema.

14th day. Convalescent.

Since the first introduction into Europe of the Jesuits' bark in 1649, the most celebrated writers extolled its efficacy in the treatment of various diseases, but especially in fevers; owing, however, to some cause or other, it fell into disuse; and it is probable that this was occasioned by the many failures which attended its administration; failures which I now feel assured were owing principally to two causes:—first, the bark not having been given in sufficiently large doses; and secondly, the bark not having been always of the genuine kind. Its use was revived by the celebrated Sydenham, and after the time of that eminent man, its excellent qualities were fully establish-

ed by Hoffman, De Haen, Pringle, Cleghorn, and other practitioners of eminence. And it is worthy of remark that those who were most enthusiastic in praise of this remedy gave it in very large doses. Dr. Clark, a celebrated physician who wrote on fever in the year 1770, was in the habit of prescribing bark in two drachm doses every two hours, and at the same time of exhibiting it by the rectum in the form of enema; he relates that on one occasion in mixing a dose of bark for a patient, he discovered that the apothecary had by mistake put half an ounce of bark into each paper instead of two drachms, which he had ordered; the patient had been taking this large dose for a considerable time, and got cured of a bad typhus in consequence. Now, when it is estimated that two pounds of good bark will yield about two hundred grains of sulphate of quina, it must be admitted that Dr. Clark's treatment of typhus was not far short of the heroic method of the present day introduced by Dr. Dundas. It is unnecessary to advert to the fact that Dr. Clark's patients were taking a different preparation, possessing, however, the same active base.

One great objection to its general use in this country, which may be advanced, is the high price of the salt; but should the remedy be found as efficacious in the practice of others as by Dr. Dundas and myself, I feel assured that its use, when applicable, would eventually constitute the true economy, of our fever hospitals.—[*Dublin Quar. Journ.* Braithwaite.

Nocturnal Incontinence of Urine. By M. TROUSSEAU.

We doubt the necessity or propriety of resorting to the apparatuses suggested by Mr. T. We have often relieved cases of this kind with Tr. of Iron, and other tonic medication.—[*Ed. S. M. & S. J.*

M. Trousseau observes that the nocturnal enuresis of children and young persons takes place after they have been asleep one or two hours, and that they retain the urine during the rest of the night. As a possible explanation of this, he refers to the fact that there is erethism of the genital organs during the commencement of sleep; and asks then, whether there may not be a tenesmus of the bladder? But, he further asks, why should not the sphincter then tend to retain the urine?

M. Trousseau speaks highly of the treatment by belladonna, as recommended by MM. Brettonneau and Morand. In almost every case in which he has used it, he has found it successful.

The organs must be caused to lose their bad habits; and this may be done by awakening the children after they have

slept an hour, in order that they may urinate. On the next night they are awaked a few minutes later, and the time of awakening is made later every night, until at last it is desisted from. This has sometimes effected a cure.

At the same time, pills containing a *centigramme* of extract of belladonna are given; a week after the quantity is raised to two, and then to three, rarely to five *centigrammes*. The child is now roused only on alternate nights, and if it has not wet its bed on those nights when it has been left asleep, it is not awakened; if it continues free from enuresis for a fortnight, the dose of belladonna is diminished. If the case goes on favourably, the treatment is alternately resumed and intermitted for some weeks, until it is finally left off. This is necessary, as the disorder may return after some months, in the manner of marsh fever.

In cases of incontinence of urine, eczema of the vulva or prepuce may be produced by irritation of the urine: it may then extend to the meatus urinarius, and cause tenesmus of the bladder.

If belladonna fails, the syrup of sulphate of strychnine is sometimes found useful. If these fail, flagellation and stinging with nettles may be useful, as instruments of intimidation, or perhaps they may have some reflex power.

M. Trousseau finally describes an oval compressor, which he has devised for the treatment of nocturnal enuresis.

An elastic band is fixed round the body, and supports at the back a spring which reaches to the anus. To this is fitted a metallic plate, on which is fixed a truncated cone of caoutchouc, two or three *centimetres* (about two-thirds to an inch) in diameter at the base, according to the age of the patient, and the amount of compression required. The pressure may be increased or diminished by introducing the cone more or less deeply; and the size is varied by adding perforated shields of caoutchouc, in greater or less number, so as to diminish the length of the cone. Straps are passed under the thighs; and thus we have a light bandage, in no way inconvenient, which in boys acts in some way as a sphincter, by compressing the neck of the bladder and preventing the urine from escaping, and imparts to the bladder the habit of remaining full during the night as well as during the day.

But as this method could only be applied in girls, *per vaginam*, and then with great difficulty, M. Trousseau employs a modification of the instrument contrived by M. Gariel for arresting uterine hemorrhage. It consists of a caoutchouc bag, which is introduced beyond the hymen: by means of a tube it is inflated with air, and a stopcock attached to it is closed,

The back presses on the lower part and neck of the bladder; and in the morning the instrument, having been emptied of air, is taken out and cleaned.—[*London Jour. of Medicine.*]

Remedy for Indigestion. By JOHN SPURGIN, Esq.

Mr. Spurgin having from numerous trials upon his own case, which was one of most obstinate indigestion, and those of numerous patients and friends, found the following recipe of eminent service in their relief, he was determined, if possible, to give it a public recommendation, hoping that it might prove an instrument in our hands for the expulsion of quackery from some of the domains of our art. He says:

This latter consideration weighed strongly to induce me to give the article a proper trial; and I allowed the late Mr. South, of Hunter street, who was a general practitioner, to prepare and sell it, after having satisfied myself in my person, and with several of my dyspeptic patients, of its efficacy and undeniable utility. The widow of this gentleman has continued to prepare and vend it, under the title of the "liquid condiment," for her own benefit, according to this formula:

℞. Liq. potassæ; sodii chloridi, aa ʒ i.; sodæ phosphatis, ʒ iss; aq. puræ, ʒ iii.

As much of the solution may be taken at a time as will not affect the taste of the beverage disagreeably.

In a great many instances the article has proved most useful; many families have it on their tables for daily consumption; and even make their tea by first putting two or three teaspoonfuls into the tea-pot with the tea.

It is not intended to be resorted to as an occasional remedy for various forms of indigestion, but constantly, in like manner as common salt.—[*Med. Times and Gazette.* Braithwaite.]

Treatment of Jaundice.

Dr. Budd recommends the following treatment in cases of jaundice arising from suppressed secretion:

From ʒ ss. to ʒ j. of sulphate of magnesia, in conjunction with gr. xv. of carb. of magnesia, and ʒ ss. of aromatic spts. of ammonia three times a day,—the sulphate of magnesia to keep up free action of the bowels; the carbonate of magnesia to neutralize any excess of acid in the stomach or bowels; and the aromatic spirits of ammonia to support the nervous system, and to keep up the action of the skin.—[*Med. Chir. Rev. Ibid.*]

On the Administration of Sal Ammoniac in Enlarged Prostate.

By M. VANOYE.

M. Fischer, of Dresden, has long been in the habit of administering this medicine, in large doses, in chronic enlargement of the prostate; and since he published an account of his success with it in 1831, various other German practitioners have supplied corroborative testimony. Seeing that surgical treatment is so inefficient, this means would seem to supply a valuable resource, and this especially as it may be employed in conjunction with other remedies. In the present paper, M. Vanoye relates two cases in addition, in which the success obtained seems quite to justify former encomiums. In both, the gland became progressively and greatly diminished in size. Large doses are, however, required. We may commence with fifteen grains every two hours, and go on to double or treble this quantity, so that nearly half an ounce is taken *per diem*. When the dose given is too great, we are admonished by disorder of the digestive organs, a miliary eruption, profuse sweats, and especially by scorbutic symptoms. These ill effects may be prevented or diminished by the employment of mucilaginous vehicles, bitter extracts or aromatics, and a good animal diet. Persons suffering from hemorrhagic disposition, or affections due to poverty of blood, should not be subjected to this treatment.—[*Bulletin de Thérap. Med. Chir. Rev. Ibid.*

Miscellany.

Quinine and Veratrum Viride in Typhoid Fever.—The gradual extension during the last few years of Typhoid fever from its former northern and mountainous habitations, to the warmer and lower regions of the Southern States, and even into Louisiana and Florida, is a remarkable fact in the history of this disease, for which it may be difficult to assign any satisfactory reason. Be this as it may, it is an interesting fact to the practitioners of these sections of country, many of the oldest of whom were never before called upon to treat this formidable affection. The discovery of the jugulating effects of quinine in the management of our fevers, whether designated as intermittent, remittent, congestive, country or malarial, had robbed this most extensive class of Southern diseases of all its terrors. The physician rode triumphantly with quinine in his saddle-bags over the most pestilential districts, administering relief and cutting short with great certainty the most threatening attacks of the malady. The planter had learnt to do the same, so that by applying the antidote

upon the first indication of the effects of the morbid agent, this was at once neutralized, if we may use the expression, and our old-fashioned bilious fever (so called) had disappeared and become obsolete. Protected against the fatal effects of malarial fevers—unharmful by the scourges of colder climes, consumption and typhoid fever—the sunny South might well have boasted of its delightful and salubrious atmosphere. Such was the state of things when a new form of fever, gradually coming down from the mountains of North Carolina, South Carolina and Georgia, and reaching the terminus of the primitive geological formations, spread itself over the diluvial plains, invading with more or less malignancy, cities, villages, plantations, pine barrens, prairies, valleys, and finally reached the sea-board of the Atlantic and Gulf of Mexico.

Accustomed to the controlling influence of quinine in the management of the former types of fever, the physician and the non-professional citizen naturally resorted to this potent agent with great confidence, and did not abandon its use until they became convinced that the new form of fever would not be *jugulated*, but would run its course, despite of quinine and of every thing else. Yet, now that the expectant method of treating typhoid affections is generally adopted in this region of country, almost every medical journal bears to us intelligence from various points of the Union, as well as from Europe, that a great discovery has been made in the treatment of Typhoid fever, and that to quinine must be assigned the additional glory of arresting this as well as malarial fevers.

We feel that our readers must be gorged to satiety with disquisitions upon Typhoid fever; for very few of our numbers have been issued during the last two or three years without something on the subject, in the original or in the eclectic departments. But, when any one tells us that he can arrest the progress of Typhoid fever, whether by quinine, by *veratrum viride*, or by any other agent, we feel bound to listen to the facts and to place them before an enlightened profession, who may test them, and whose experience we would be pleased to learn, whether for or against the new remedies, whenever sufficiently extensive and systematic to be of real value. Drs. Dundas, McEvers, Hayward, and others of Great Britain, have published their advocacy of the quinine treatment of Typhoid fevers in strong terms. In our country a number of practitioners have done the same, among whom we may name Prof. Thos. D. Mitchell and Dr. Fenner. Dr. Norwood and others are equally decided in favor of *veratrum viride*. It is a question to be decided at the bed-side, and

without reference to preconceived notions or theories. It matters not whether Typhoid and malarial fevers be regarded as identical or dissimilar in origin, cause or nature ; what we need is a remedy—a controlling agent. Let us then weigh the question calmly and without prejudice—try fairly the plans proposed—and the truth will soon be determined. As conductor of this Journal, we will side with neither party, but endeavor to keep our readers posted up as to facts.

Reward for the Discovery of Anæsthetic Agents.—From the documents we are continually receiving, it is evident that the contest for Congressional remuneration for the discovery of the anæsthetic effects of Sulphuric Ether is actively carried on by the respective friends of Drs. Jackson, Morton, and Wells. We have taken some pains to look into the relative merits of the claimants, and would unhesitatingly, were it in our power to do so, confer the reward upon Dr. Charles T. Jackson, as the first who really discovered and proved by direct experiment that insensibility could be induced by the inhalation of Sulphuric Ether. That Dr. Morton was very active in promulgating the knowledge he derived from Dr. Jackson, seems to be well established—but we feel equally well assured that if Dr. Jackson had never made the discovery, Dr. Morton would never have been heard of in connection with it.

Transactions of the Am. Med. Association.

The Fifth Volume of the Transactions of the American Medical Association contains upwards of nine hundred pages of matter, about four hundred of which are allotted to Reports upon the epidemics of various sections of the Union. These are valuable contributions to the history of disease, but they will not interest the great mass of readers. These reports are preceded, however, by papers of intrinsic merit and practical value, which all will read with pleasure and advantage. The first is the "Prize Essay," by Prof. Austin Flint, of Buffalo, on the "Variations of Pitch in Percussion and Respiratory sounds, and their application to Physical Diagnosis." This, like the other writings of the distinguished author, evinces a discerning and logical mind of high order. The second is from the gifted pen of Prof. S. H. Dickson, of Charleston, "On the Blending and Conversion of Types in Fever"—an exceedingly interesting document. The third is "On the Action of Water on Lead Pipes, and the diseases proceeding from it," by H. Adams, M. D., of Waltham, Mass., and ought to be carefully read by all who reside in communities which

use water from lead pipes. The fourth paper is by Dr. George Hayward, of Boston, and treats of the "Permanent Cure of Reducible Hernia," but unfortunately teaches us nothing new upon this important subject. Prof. Pope, of St. Louis, is the author of the Report upon "Water; its Tropical uses in Surgery," which will be found full of practical utility.

We regret that our limits will not permit us at present to notice these Transactions more at length. The price of this volume is \$5. Complete sets of the Transactions can be obtained by remitting \$25 to Messrs. Blanchard & Lea, Philadelphia.

Privileges of the Graduates of the Medical College of Georgia.—The Faculty of the Medical College of Georgia, having been informed that a doubt was entertained by some persons as to the exemption of the Alumni of this institution from the necessity of obtaining a State License to practice medicine, deemed it important to procure and to publish the opinion of an eminent jurist on the subject. The following communication from the Hon. A. J. Miller will, it is hoped, settle the question. We add the section of the Statute now in force, setting forth the penalties incurred by those who may practice medicine without either the Diploma of the Medical College of Georgia or a State "License."

"Sec. 11. If any person shall hereafter presume without such license to practice physic, surgery, or in any manner prescribe for the cure of diseases for fee or reward, he or they shall be liable to be indicted, and on conviction shall be fined not exceeding the sum of \$500 for the first offence, and for the second, be imprisoned not exceeding the term of two months, one half of the fine to enure to him who shall inform, and the other half to the use of the State."—COBB'S DIGEST, p. 886.

AUGUSTA, February 15th, 1853.

I have been requested to give my opinion upon the question, whether the Graduates of the Medical College of Georgia are required to undergo an examination before and to receive license to practise from the Board of Physicians.

This inquiry, I think, is plainly and satisfactorily answered by the ninth section of the Act of 1828 (Dawson's Compilation, 196) incorporating the Medical Academy (now the Medical College) of Georgia. That section declares that—

"The Graduates of the Medical Academy shall be allowed to practise Medicine and Surgery in this State, in the same manner as they would have been, had they been examined and licensed by the Board

of Physicians of the State of Georgia; any law, custom or usage to the contrary notwithstanding."

Considering the act of incorporation a contract between the State and Institution, and the section in question as giving a privilege conducive to the prosperity of the latter, I do not believe it to be in the power of the Legislature, by any subsequent act, to impair any part of the franchise conferred. The Legislature has not so intended; for while, by the acts of 1829 and 1833, the name of the corporation has been twice changed, and by the last an outfit provided, the powers and privileges conferred by the original charter upon the College and its graduatés have not been interfered with.

It is true, that the Act of 1825, (Cobb's New Digest, 886,) prohibiting physicians from practising without a license from the Board of Physicians, has been revived by the Acts of 1839 and 1847—yet, as the Charter of the College relieved it from the provisions of that Act, afterwards repealed, the revival of it cannot affect the exemption previously granted.

ANDREW J. MILLER,

Attorney at Law.

State Medical Society.—The Medical Society of the State of Georgia will hold its Fourth Annual Meeting in Savannah, on Wednesday, the 13th April next. Juriah Harriss, M. D., of Augusta, will deliver the annual address. It is hoped that a large meeting will assemble, as the transactions are expected to be of a very interesting character.

D. C. O'KEEFFE, *Rec. Sec.*

Greensboro', Ga., Feb. 1853.

Committees appointed by the President of the Medical Society of the State of Georgia.

"*Resolved*, That a committee of three be appointed by the President, for the purpose of proposing subjects for Essays to be presented at the next annual meeting.

"*Resolved. also*, That the President appoint committees of one for each of the Essays above referred to, whenever he shall have been furnished with the subjects selected."

In accordance with the above Resolutions the following subjects were presented to the President, who appointed as Reporters the gentlemen whose names are annexed to the questions. If the committees respond to the call thus made upon them, as we trust all will, the next meeting of the Society will be exceedingly interesting.

1. On the relative liability of the White and African races to particular diseases—especially to Hemorrhoids, Fistula in ano, Tetanus, Phthisis pulmonalis, Scrofula, Intermittent fever, Cataract, and death in case of traumatic injury of the brain. By Dr. R. D. Arnold, of Savannah.
2. On the existing Laws of Georgia, relating to the Practice of Medicine, and the sale of Drugs, with suggestions for additional legislation. By Dr. R. Q. Dickinson, of Albany, Baker Co.
3. On the Topography and prevalent diseases, during the past year, of each Congressional District of Georgia. Dr. P. M. Kollock, of Savannah, for the 1st District; Dr. Theophilus Stewart, of Columbus, for the 2d District; Dr. G. F. Cooper, of Perry, Houston Co., for the 3d District; Dr. W. N. King, of Roswell, Cobb Co., for the 4th District; Dr. Robert C. Word, of Cassville, for the 5th District; Dr. Henry Hull, of Athens, for the 6th District; Dr. C. J. Paine, of Milledgeville, for the 7th District; Dr. Marshall Andrews, of Washington, Wilkes Co., for the 8th District.
4. Dr. J. Le Conte, of Athens—On the Medical Botany of Georgia.
5. Dr. I. P. Garvin, of Augusta—Bibliographical Sketches of Drs. Antony, Abbott, and Lyman Hall; Dr. C. W. West, of Savannah—Bibliographical Sketches of Drs. Waring and Richardsone; Dr. C. B. Nottingham, of Macon—Bibliographical Sketch of Dr. Baber.
6. Dr. H. Rossignol, of Augusta—On the Vital Statistics of the City of Augusta; Dr. W. G. Bullock, of Savannah—On the Vital Statistics of the City of Savannah; Dr. John Bacon, of Columbus—On the Vital Statistics of the City of Columbus; Dr. J. M. Green, of Macon—On the Vital Statistics of the City of Macon; Dr. J. F. Alexander, of Atlanta—On the Vital Statistics of the City of Atlanta.
7. Dr. L. D. Ford, of Augusta—On Pneumonia, and Pleuro-pneumonia, as they have prevailed in Georgia of late years: their amalgamation with Remittent fever; and in what respects they differ from those diseases, as described by European and Northern writers.
8. Dr. H. R. Casey, of Appling, Columbia Co.—On the Remittent peculiarity assumed by Typhoid fever in Georgia.
9. Dr. L. A. Dugas, of Augusta—On the best plan of treating Fractures in country practice.
10. Dr. D. C. O'Keeffe, of Greensboro'—On the Epidemic Jaundice which prevailed during the past year in certain sections of Georgia.
11. Dr. H. F. Campbell, of Augusta—On the administration of Remedial agents per rectum.
12. Dr. Tomlinson Fort, of Milledgeville—The *names* of *regular* physicians, and the *number* of irregular practitioners in each county.
13. Dr. J. A. Eve, of Augusta—On the use of Anæsthetic agents in Obstetrical practice.
14. Dr. W. E. Dearing, of Augusta—On the Virus of Serpents.

SOUTHERN MEDICAL AND SURGICAL JOURNAL.

Vol. 9.]

NEW SERIES.—APRIL, 1853.

[No. 4.]

PART FIRST.

Original Communications.

ARTICLE XIII.

Observations on the Value of the Microscope as a means for the Diagnosis of Disease. By W. J. BURNETT, M. D., of Boston.

It would indeed be as much out of taste as it would be uncalled for, to offer an apology for an instrument by the use of which organic science has already made such signal advances. Its claims being admitted, my object is to refer to some of the leading features of its value for the accurate diagnosis of disease both in and out of the body.

But, in the discussion of this subject, the query at once arises, does disease always have a tangible material expression, and that too of a corresponding and invariable character? A negative answer to this opening question would be regarded by many as quite *unphysical*, not to say unscientific; but, in the present state of our knowledge, I must consider it as the one by far the most correct. It is true, that it may be urged that in virtue of the great fact constantly before us, that vitality has its expression only in organization which is tangible and capable of analysis, so must it be inferred that there is always a tangible expression of any perversion of that vitality. This view may be very scientific in one sense; but in the present state of pathology it is not the one practically correct; for there are many transitory morbid changes of the vital phe-

nomena, many abnormal conditions of the economy known as functional, which leave no traces behind in the material parts or organs in which they occur—at least as far as we can now detect, after the most thorough examination. The probable existence, however, of corresponding changes, in such instances, is not denied; but, in a practical point of view, we stand the same as though they were even disproved to exist at all.

Too high an estimate, therefore, must not be placed upon the value of intimate microscopical studies for the elucidation of pathological conditions and phenomena. It should also be borne in mind that, even had we in every disease a definite product on which we could lay our hands, yet, after all, we cannot thereby go beyond the physical peculiarities of the product in question, and these may be wholly inadequate to the morbid phenomena manifested; for it is the same in Pathology as it is in Physiology, that the structure of an organ or a material part furnishes us no certain key to its ulterior vital conditions or relations.

The practical value of the microscope then, in pathology, does not consist in hunting up the traces of obscure phenomena, but in accurately determining the differential character of well marked products.

The question then arises, have the elements of these material products of disease, characteristics sufficiently constant and unvarying to make them objects of scientific study. That the correct answer to this query is an affirmative one, I shall now attempt to show by a reference to the histological relations of diseased products.

The relations of Physiology to Pathology are such, that the latter seems but an erring condition of the former. On this account the elements of morbid products appear under the same laws as those regulating the production of healthy tissues. Morphologically speaking, therefore, pathological formations have no elements differing in kind from those of health. But the difference lies in their complete want of functional relations: they subserve no adaptive economical purpose in the system, but are in one sense parasitic. These products, also, as superventions in and upon healthy parts, are necessarily *infra* formations—they are below the standard of those of

health of which they may, or may not, take on some of the characteristics. These elements are cells, celloid and granular forms, all of which wear the semblance of physiological parts. As for the cells, I may make here the general statement, founded on a pretty extended observation, that, both as to their genesis and general aspect *as simple cells*, those which belong to the abnormal cannot be distinguished from those belonging to the normal products of the economy: that is, they are cells, and in both cases have all the requisite parts of these organs. Where, then, are the distinguishing features between these two classes of objects? I answer, not so much in the characteristics of their isolated as of their aggregated forms, and in their local and functional relations to the parts in which they occur, or to the whole system generally. For an exemplification of these points we will take up the three classes of heteromorphous pathological products, viz., Cancer, Tubercle, and Pus and pyoid forms:—Of these, cancer is the highest, and has most of the general peculiarities of healthy parts.

Its earliest appearance in a distinct and unmistakable form consists of a few cells, as I have often observed. We can trace it no farther back, but all analogy is in our favor for supposing, as is the case with tubercle, that these cells are formed in an unhealthy plasma effused from the blood. Moreover, on the inner surface of the testicular tubes, as I have sometimes observed them just appearing, they seemed to replace the normal epithelial cells there situated, and to be formed like them. This cell, the first, is also the last or highest expression of this product, it being the *terminal* and not the *transitional* form. It is nucleated, and not unfrequently has several distinct nuclei; but, as to size, form, and other external characteristics, it has nothing specific, that is, constant and peculiar; and in my present opinion, no features by which it, taken alone, may *always* be distinguished from other purely cell structures.* Formerly my

* Deaenoot, in a rather a long article on the application of the microscope to the study of Cancer, (*Amer. Jour. Med. Sci.*, Jan. 1853, p. 33.) thinks that the cancer-cell has sufficiently distinctive characteristics to justify, in a given case, a decision that it is or is not such, from a single specimen. I once took this ground, but now do not think it all tenable. He alludes, also, (p. 60,) to the brilliant nucleoli and, quoting *Hall*, regards them characteristic. But these have appeared to me only as glistening fat globules, and I have met with them in all morbid cells, and especially in those of the spindle-shaped epigeneses.

opinion was, like that of most of the microscopical pathologists then and now, that the cancer-cell, as such, has physical peculiarities by which it can always be distinguished from all other cells, healthy and diseased. But from a late and somewhat careful review of the subject, based, not only on observations of the cells as physical objects, but upon a consideration of them as constituting an expression of pathological action, I am free to confess that I do not now regard my former opinions as tenable.

All, or nearly all the microscopical relations of the cancer-cell which have any claim of being peculiar to it alone, may, I think, be stated in the following brief manner:—When fully developed it is simply a nucleated cell; it may, however, have several nuclei, or even none at all; it is often of an irregular shape, fusiform, caudate, &c., &c., and its size is frequently equally variable; but the evidence of its being not only of a pathological, but of what is called a cancerous nature, cannot, and does not, lie with any one of these points alone, but rather with a combination of them all, and that, too, united with the fact of its occurring in the tissues, and under circumstances where it cannot be regarded as a healthy product. Its differential diagnosis, which I think can be made out in nearly every case, depends upon these considerations which, when fully examined, will, I believe, be found amply sufficient.

Suppose, for an illustration, a small bit of a cancerous tissue has been forwarded for microscopical analysis. I know nothing of its history, locality, or other relations; but I decide in the first place that it is not a normal product, from the fact that its cell constituents have no *type* peculiarities. As an heteromorphous product, then, the diagnosis lies between its being tubercle, a pyoid form, or cancer. But as the two former are only celloid, and not true cell structures, the decision rests with its being cancer. Put upon these grounds, its diagnosis would always be attended with no difficulty. But the matter is not thus easily disposed of: the question arises, may it not be a homœomorphous non-malignant product of an epitheloid character? In other words, can simple epitheloid formations be always discriminated from cancerous products occurring in the same locality? This question has both an histological and

a practical importance, and in giving to it an affirmative answer, I know that I am expressing more than many pathological microscopists would allow. But my own experience sustains the correctness of this view. The distinction consists in the absence of all individual cell-type in the one (cancer), and the presence of it in the other (epitheloid epigenesis). The cells of cancer taken separately are simply cells—they present no invariable special or specific characteristics, as before remarked; while those of epithelial tumors have the almost exact resemblance of those of normal epitheloid tissues. Meckel has, indeed, laid down the law, that morbid tissues assume the morphological characteristics of the normal parts with which they lie in contact; and it might be argued, that, this law being true, cancer occurring in an epithelial tissue would be so epitheloid as not to be distinguishable from a simple epithelial product. But, in my opinion, this law of Meckel holds true only of homœomorphous products, and cancer is distinguishable by its recklessness of these very governing laws. From these data, I think the experienced eye can always, or nearly always, make out the differential diagnosis in difficult cases of this kind. But here I may make the remark that, in the successful study of these forms, as is true of the study of Genera and Species in Natural History, the appreciation of differences is something not always easily described in detail,—seeming to consist in a faculty of judging from the *tout ensemble* of appearances, and which is acquired only from a somewhat extended study and observation. But we will carry out the application of these remarks somewhat further.

There not unfrequently occur cases where it is all important to know positively, whether a certain disease is of a malignant nature or not, as bearing on the expediency of an operation, or on the prognosis of the case. The question is, does the microscope afford any positive and new data for the solution of such points? In a majority of cases where a portion of the diseased tissue is accessible, I think it does; and even where its data furnished are not sufficient of themselves, they may be very great aids combined with others of a general and clinical character. Thus, cancer of the uterus may be diagnosed accurately from the escaped exudations which pass off through the

vagina. This is true likewise of its occurrence in other organs where there is an escape of its elements externally. But in cases where the disease has no external communication, but lies concealed beneath healthy tissues, as is true with many tumors of the surface of the body, and must be reached by an exploring needle, the results thus obtained are less reliable and trustworthy. In the first place, one cannot be positive that the minute portion thus brought away is the real primitive disease; and in the second, the quantity is so small that you cannot make that comparative examination necessary to afford a positive result, at least such a one as would allow you, in any given case, to assert an opinion as to the nature of the disease, at variance with that founded on its locality and general history. But such examinations will furnish at least highly auxiliary data, and in many cases may serve as the turning point in this or that diagnosis. The practical bearings of such examinations are sometimes strikingly prominent as I have myself observed and wrought out in three or four cases; but, unfortunately, they are generally less so than those of a scientific character. Usually the line of action of the practical surgeon is clear and distinct, based upon a resumé of the general conditions of the case compared with past experience. He is, moreover, generally called upon in the advanced stages of the disease and when all other resources have failed, and then his decision to use the knife or not, is based more upon the constitutional condition of the patient and the amount of disease, than upon an exact knowledge of its histological nature.

But the chief value of microscopical studies of cancer, is after the diseased product has been removed. It is true, that such examinations have few or no practical bearings for the patients from whom the disease is taken, but they are of eminent scientific importance for our most comprehensive knowledge of this morbid product. If, with a full history of its general and special relations, is united, in every case, an exact account of its histological nature, the whole has a scientific completeness, exactly as when a chemist with any mineral, gives, besides its locality, &c., its qualitative analysis. And when the annals of science shall have been made rich with such records, the practical surgeon will proceed with an inductive certainty in his

movements, quite unknown at the present time. The necessity of such a line of strict inquiry will perhaps be the better appreciated, if we bear in mind that the splendid labors of the older surgeons want a strict scientific value in this respect, from the absence of such histological analyses. Recent microscopical studies have shown that the general or gross characteristics of a disease form no invariable and certain index of its histological and real nature. Then again, depending entirely on gross appearances, why should the opinion of the most distinguished surgeon be preferred to that of the most humble, as to whether a tumor is hard, soft, or grey, white or brown? And surely, when Sir Astley Cooper or Velpeau have unhesitatingly and authoritatively pronounced this or that tumor to be a cancer, their opinion must, in reality, have been founded upon such arbitrary data as these.

Many of these same considerations just made are applicable to the second morbid product in question—*Tubercle*. This is, however, a much inferior formation, being *celloid* rather than cellular. Moreover, it is connected with a much lower constitutional dyscrasia, and its local appearance, however small, is usually attended with general conditions of an unmistakable character. On this account, therefore, microscopical examinations are not very frequently required for the accurate determination of this product in the living body. There are cases, however, when careful histological analyses are necessary to decide between it and the products of so-called chronic inflammation. But here, as is the case with cancer, these analyses have now more of a scientific than a practical value; for in all such cases, whatever may be the scientific, yet the therapeutic diagnosis would be the same. This, the best of clinical experience has shown to be true. But the question occurs, has tubercle a characteristic element by which, with the aid of the microscope, it can be distinguished from other morbid products. To this query, I return an affirmative answer, in the same way and with the same qualifications as I have with cancer; but at the same time, a diagnosis of this kind, with the same data, would have much more assurance than is true of the last mentioned product. This is so because its elements are *celloid*, and the product of a low inflammatory action, so that there are few others with which it can be well confounded.

The distinguishing characteristics of the tubercle corpuscle belong to a low vital condition. When first formed, as I have observed in several instances in the lungs, it consists of a celloid body of about one two-thousandths of an inch in diameter, non-nucleated, but containing numerous granules: it is then perfectly spherical and has a very uniform aspect. But at this early period, and before they have formed a distinct visible product, these corpuscles are rarely seen. Most observations upon them are as found in distinct tuberculous matter taken in masses of various sizes from different organs. They then have a different, though not less uniform aspect; they are smaller, their contour less regular, and on the whole they have a withered, shrivelled appearance. This change is due, I think, to a partial exosmosis of their previously contained liquid contents. Treated with acetic acid, the membrane or involucrum becomes more transparent, thus rendering more distinct the granular contents of the corpuscle. Thus characterized they lie in the midst of broken parts of their like, together with a crowd of granules, all of which combined, form the solid constituents of the tuberculous mass.

I think I do not estimate too highly these points, when I affirm, that with such data the experienced microscopist can pretty positively decide whether or not a product in question is tubercle. But even were he in any given case, unable to return a positive decision, this would have very little *practical* importance, for the products at all likely to be confounded with it, are those of also a low inflammatory nature, and referible to the same line of pathological action.

There are cases, however, though confessedly few, where, during the life of the patient, the microscope may be of much service in fixing positively the diagnosis of tubercular disease. Such are cases where this disease has some internal seat, but communicates externally by a fistulous or other opening. The best instances of this kind are cases of tuberculosis of the bones, and several times I have had the satisfaction of accurately determining this disease thus concealed, simply from its exuded puriform portions escaping externally through long fistulous canals.

But in truth, as has just been said of cancer, microscopical

studies of tubercle have, at present, more of a scientific than a practical value. Occasions are constantly occurring during post-mortem examinations, where it is both interesting and important to accurately determine the nature of larger or smaller diseased products accidentally or otherwise met with in the various internal organs. Such decisions are often of much scientific value, as bearing upon the doctrines of the concomitancy or antagonism of certain diseases. Such examinations are all the more worthy of attention, from the fact, that on the surfaces, or in the parenchyma of internal organs, there are constantly formed from various accidental inflammations, lymph-like and other products so closely simulating in gross aspects tubercle, as to be easily taken for it. But I need not pursue this line of consideration farther, for the various important bearings of such analyses will be readily suggested to those at all familiar with these studies. I will add only, that the confidence and satisfaction which the necrotomist has under such circumstances, constitute one of the pleasantest relations in this department of microscopy. Like the chemist in his laboratory with minerals before him, he need not dwell with uncertainty upon the doubtful aspects of a product at his hand.

In regard to pus and pyoid corpuscles, they need not claim much of our attention with the relations under discussion,—they are the products of inflammation alone, and, in my opinion, inflammation never occurs without their being formed to a greater or less extent. Their microscopical appearances are too well known to be here described. The highest form, the pus corpuscle, however, is very uniform in its size and general appearance, full as much so as that of tubercle. It is the highest or ultimate product of common inflammation, and is observed in its most perfect form in the so-called “laudable” pus which is richest in plastic material. In the majority of instances its gross characteristics are sufficiently indicative of its exact character. But there not unfrequently occur cases in which the microscope serves as a happy and positive aid. Such are instances where it is concealed in some other product, as the urine, the feces, and effused liquids into any of the cavities of the body. I need not here enter into detail to show exactly how this aid is rendered, but instance after instance might be

quoted illustrating how, by such examinations, an internal latent inflammation has been positively diagnosed when wholly inaccessible by any or all of the common symptoms, if perchance they were present.

As to the so-called "pyoid forms," their practical microscopical relations are still more prominent: They consist of rudely-made granular corpuscles of variable size, though in other respects of a pretty uniform aspect; they are easily distinguished from the well-formed pus-corpuscle by this ill-conditioned structure. I regard them as the transitory forms between the tubercle and pus-corpuscles, if the present state of our knowledge of these products and their relations will allow the admission of any such transition. Indeed, the study of the *tout ensemble* of their appearances in comparison with tubercle, has had no small weight in the formation of my opinion that this last is truly an inflammatoid product. Wherever occurring they are the results of an inflammatory process alone, and that too where the effused blastema is of a very low order. On these accounts great value may be attached to their presence which, by the way, generally occurs under circumstances otherwise obscure; and then the question of inflammation on other premises cannot be raised. They are met with in dropsical fluids of any source, as those of hydrocele, cystic disease of the ovary, ascites, &c.,—also in organs laboring under a chronic inflammatory process, at least where the inflammation is of an indolent instead of a suppurative character. A well-marked instance of this last, and where too the microscope is of signal aid, is that low form of nephritis known as Bright's disease: for, according to my own observation, their successive presence in the urine is almost invariably diagnostic of this disease.

A consideration of the practical value of the microscopical study of the homœomorphous products of disease, as well also as that of the morbid changes of the solid constituents of the blood, must here be omitted for another time.

ARTICLE XIV.

Fracture of the os Frontis—loss of cerebral matter—Recovery.

By R. T. FOOTE, M. D., of Society Hill, Ala.

On the 18th of August, 1851, 4 o'clock, A. M., I was called in haste to the plantation of Mr. M. J. C., of Macon county, Alabama. On arriving, I found a negro woman in the following condition: Total unconsciousness, not being able to arouse her by the loudest calls; stertorous breathing; general paralysis—evidencing no sign of sensibility even when severely pinched; coldness of skin; pulse slow and feeble; pupils dilated, insensible to the strongest light, etc.; bloody water oozing out of ears.

One and a half inches above the left orbital plate there was an external wound of two inches in extent; superiorly to this, one inch below the coronal suture, another, about one and one-fourth of an inch long—each extending in a line parallel with the above mentioned suture, that is, transversely across the cranium. A considerable portion of cerebral matter was lying in immediate contact with her head. Beneath these wounds the frontal bone was severely fractured and much depressed. By way of explanation, I will add, that this injury was inflicted by a negro man giving her two heavy blows with the pole of an axe: the woman was asleep at the time.

This being a very serious case, I requested a consultation. Doctor W. was accordingly called in. The treatment, of course, was to remove the compression as soon as possible; we therefore commenced removing the detached pieces of bone, and soon took away seven pieces. Two of these were very large, the others small—putting them together they will measure at least three inches square. We ligated but one small artery, a branch of the external temporal, the loss of blood not exceeding 20 oz. After removing all detached spiculæ of bone and cleansing the parts of blood, we found the dura and pia membranes punctured in several places; in fact, the whole appeared to be reduced to a pulpy mass. The external wound was then closed by the uninterrupted suture. Immediately after removing the largest pieces of bones, the patient's sensibility began to return, evidence of which she

gave by her deep groans and movements. We ordered cold cloths to be kept constantly applied to her head, renewing them every fifteen minutes; perfect quietude, &c.

The following is merely a transcript of notes recorded at each visit:

August 18, 6 o'clock, p. m. Appears to be a little conscious, making some attempt to answer questions when interrogated; passed urine; pulse 85; restlessness; continual moving of extremities. Same treatment continued.

19th, 8 o'clock, a. m. Nurse reports she slept six hours during the night; drank water and some water-gruel. Pulse 80, no fever perceptible; left eye blood-shot. Her infant child being presented to her, she took and nourished it. 6 o'clock, p. m., sensorial functions remain the same, pulse varying from 70 to 75. Treatment continued.

20th, 8 o'clock, a. m. No special alteration from the 19th, only in the pulse, which is now 60. Same treatment, with the addition of a bolus of calomel and jalap aa 10 grains.

21st, 9 o'clock. Pulse 62; voice more distinct; appears to be conscious of her condition and the cause of it; complains of great pain in occipital region.

22d, 10 o'clock, a. m. Pulse 70; sensorial functions the same. Having noticed a free flow of saliva and difficult deglutition for several days, I examined her particularly, and found upon pressing on the left side of the inferior maxillary bone, it gave her great pain; breath quite offensive; tongue and cavity of the mouth covered with yellow pimples. Ordered her to have sulphate magnesia, to keep her bowels open, etc.

23d, 9 o'clock, a. m. General restlessness; pulse 75; occasional pain in sinciput region; contraction of features; water running from left eye; face much swollen.

24th, 8 o'clock, a. m. A fair night's rest; very calm and quiet; countenance more natural. 6 o'clock, p. m. Slight fever this evening, supposed to be caused from excitement of company. Again gave orders to keep her perfectly quiet, with the same general treatment, viz. cold to the head, saline purgatives, water-gruel, &c.

25th, 6 o'clock, p. m. The offensiveness of the breath increased; great difficulty in swallowing; pulse 80, rather full. Same treatment, with an astringent mouth-wash.

26th, 6 o'clock, p. m. Pulse 75, full and soft ; complains of great pain in the space between the helix of the left ear, and articulation of the inferior maxillary bone ; pus mixed with sputa, noise disturbs her much.

27th. Much better to-day. Unfavorable symptoms of yesterday moderated.

28th. Still improving ; pulse nearly natural ; no symptoms of inflammation.

29th. Conversation rational and tolerably distinct ; recollection good ; complains of but slight pain. Requests stronger diet. Healthy suppuration from external wound, with the exception of two small places, from which issued sanious fluid. A few granulations of fungous growth shooting up. These destroyed by burnt alum, cicatrized in a few days.

I continued to visit this patient for several weeks. Nothing further occurred worthy of notice. I will only add, she continued to improve rapidly, and is now in quite good health.

The space from which the bones were removed has now for its covering the common integuments of the scalp. By placing your hand in immediate contact with that portion of the head, you feel a strong pulsation synchronous with that of the heart. The edges of the bone can be sufficiently felt to trace out the extent of the fracture.

This woman suffers no inconvenience from the injury, when cool and quiet ; but when over-heated or much excited, a determination of blood to the head takes place, producing blindness, giddiness, etc.

ARTICLE XV.

Two Singular Cases—Reported by L. A. DUGAS, M. D., &c.

The following cases are related merely as accidents of a singular character :

ACCIDENTAL CASTRATION.

A negro boy about 14 years of age, belonging to Mr. L. P. D., was riding a mule, harnessed as usual upon our plantations for ploughing, when the animal took fright, started off at great speed and dismounted his rider. In endeavouring to maintain

his position, the boy clung closely to the neck of the mule, but finally slid off on one side. It so happened that the trace was suspended to the back band by means of a stout iron hook, which, as the boy glided over it, caught him by the scrotum and completely tore out his left testicle, which was found upon the hook when the mule was captured.

The boy was sent to the city, a distance of eleven miles, and placed under my charge. The injured side of the scrotum looked as though it had been laid open longitudinally with a dull cutting instrument; the cord was retracted and yielded no hemorrhage; the boy stated that it bled freely at first, but soon ceased to do so. The edges of the wound were carefully brought together and secured with adhesive strips. No unpleasant symptoms ensued; union was effected by first intention; and in a few days the patient returned home perfectly well.

MOUTH RIPPED OPEN BY A COW'S HORN.

A negro woman owned by Mr. B. P., was in the street near the market-house in this city, when a cow, in endeavoring to escape from dogs who furiously pursued her, encountered the unfortunate woman, thrust a horn into her mouth and tore open the cheek of her left side, from the angle of the mouth up to the masseter muscle.

The edges of the wound were brought together by means of interrupted sutures and adhesive plaster, and union by the first intention was obtained without farther difficulty. The face now shows a merely linear cicatrix and is not disfigured.

A friend, on hearing of this case, informed us that one of his servants was standing before an ox and endeavoring to adjust the yoke, when the animal suddenly throwing up his head, caught one of his horns into the man's nostril and hoisted him into the air, very much to the merriment of the by-standers, for he was not much injured. Sancho Panza was assuredly never in a more ludicrous predicament.

PART II.

Eclectic Department.

Letters upon Syphilis. Addressed to the Editor of L'Union Medicale, by P. RICORD. Translated from the French, by D. D. SLADE, M. D.

[Continued from Page 154.]

EIGHTH LETTER.

My Dear Friend,—It is my purpose to-day, as I promised you, to see if it is possible to distinguish a simple blennorrhagia from one with a chancre concealed in the urethra. You see that I lay down the problem as boldly as my opponents.

In the study of this diagnosis, it is important to establish two conditions; the one a diagnosis absolute, unequivocal and undeniable; the other a rational diagnosis.

An absolute diagnosis cannot be obtained but by artificial inoculation. Every time that muco-pus furnished by a mucous surface will give the characteristic pustule, which we will soon have to examine in studying chancre, we can affirm, whatever has been the duration of the disease, that this muco-pus is virulent, that there is a chancre somewhere; the chancre alone being able to give rise to the positive results of inoculation. Here is the incontestable fact established by my researches, and the absolute and unequivocal diagnosis in all its strictness.

When by the inoculation of muco-pus from the urethra you obtain the characteristic pustule—pronounce boldly, and without the possibility of error, it is a virulent blennorrhagia. But only ask of inoculation, as of all the other means of investigation, what we have the right to expect from it. We must have variolic or vaccinal virus to produce the effects of variola or vaccine. If at the side of a variolic or vaccinal pustule an abscess is developed, and you should take the pus from this abscess to inoculate, you would not obtain the specific effects of the vaccine nor of the variola. Take some muco-pus in the neighborhood of a variolic pustule developed upon the Schneiderian membrane, and this muco-pus will not give the effects of the variolic pus.

If you have, then, a patient actually affected with an urethral chancre, and at the same time with a simple blennorrhagia (a frequent complication), and in the place of taking the pus from the chancre, we take it from the blennorrhagia, the result will be necessarily negative. It does not require much mind to understand so simple a thing, and I am astonished that M. Vidal, who has much esprit, should make of this an objection against

inoculation. I have too high an esteem for his understanding to admit that he could believe that pus furnished by chancre of the urethra, when a blennorrhagia coëxists, ought necessarily to be mixed with all the blennorrhagic pus; or that a drop of pus from a chancre, acting after the manner of leaven, renders the other necessarily virulent. Without doubt, the complication of morbid elements, as regards the diagnosis, often renders the analysis difficult, but an exact knowledge of each of these elements permits us, under any circumstances, to distinguish between them.

The chancre of the urethra, which can never have a very great extent or large surface, can furnish but a very small quantity of virulent pus. Even in the indurated chancre the secretion is sometimes almost nothing, generally insufficient to stain the linen of the patient. A very fine example of this can be seen at this moment in No. 15 of the first ward of the Hospital du Midi.

Every time, then, that we have to deal with a very abundant discharge, we have the right to suppose that there is something else besides the product of chancre. We must guard against concluding upon the absence of chancre in the urethra, from the negative results of inoculation. But if the inoculation is repeated several times—if, moreover, care has been taken to press out the secretion of the urethra in order to obtain the more immediate product of the ulcerated surfaces—and the results have always proved negative, there is a very great probability that it is a simple blennorrhagia and without the complication of chancre. Without doubt the diagnosis here is neither absolute nor complete; but does it not present at least something more than the diagnosis which is generally made?

In order to draw a conclusion from the negative results of inoculation, the epoch at which the experiment is made must be kept strictly in view. We shall see later, in studying the chancre, that the virulent secretion has a term, and that there is a moment when the ulcer passing into the state of simple ulceration ceases to furnish specific pus. If, then, experimentation is made too late, less can be concluded from the negative results, than if the inoculation had been made during the first or second week following the infecting coitus.

In examining inoculation under this point of view, does it not offer all that strict reasoning can demand? If the results are positive, this gives you the most absolute sign that diagnosis can give. If they are negative, the results conduct then to a rational diagnosis of which they may be the most valuable elements. Let a more sure or a more fruitful sign in human pathology be found. What! would not that be a sign of great

importance, which, when it exists, permits us to affirm, in a necessary inevitable manner, the existence of a lesion with grave consequences, and which, when not existing, can conduct us with a sort of certainty to a rational diagnosis!

And because this sign has also its uncertainties, shall we not pay attention to those circumstances in which it presents a value and a mathematical precision? Are we, then, so rich in absolute diagnosis, that we ought to show ourselves indifferent, sceptical, or scorers with regard to a sign the existence of which smooths over so many difficulties?

What other means but inoculation, in a case of legal medicine, will permit us to state strictly that a blennorrhagia is or is not symptomatic of chancre?

But is it asked of me if inoculation is always applicable? Do we always arrive in time? Can we and ought we always to count upon it? Must we always have recourse to it? Certainly not; I have written this and repeated it a hundred times in my lectures, and it is incredible that objections should be again sifted over that I have myself so many times made. Inoculation, since it is again necessary to repeat it, is an excellent means of diagnosis, but of which we are often deprived. Is this a reason for renouncing the research into the distinctness between simple and virulent blennorrhagia? Without doubt, no; and fortunately, a well-directed minute study of all the elements of the disease, gives, in the great majority of cases, whatever my opponents say, a diagnosis sufficient to enable us to conclude upon the prognosis, and to furnish the indications of a treatment truly methodical.

It is not sufficient, as we shall see later, merely to have a primitive ulcer in order to fear the constitutional verole, and to necessitate a mercurial treatment; other conditions are ordinarily sufficiently well marked to enable us to recognize them.

Permit me then, to pass over again very briefly in review, the ordinary elements of the diagnosis of blennorrhagia, of which there has already been a little question, on account of the etiology.

You recollect what I said of women considered as a focus of infection, and the value which we can attribute to the source, as regards concluding upon the virulence or the simplicity of blennorrhagia. The patients have a singular naïveté upon this point, and entertain a strange idea of morality. How many times have I seen young people enter my office and say to me—the blennorrhagia which I have caught cannot be otherwise than benign, for I contracted it from a married woman, the wife of one of my friends, and I am very sure that it cannot be anything more than an *échauffement*. At this I am accus-

tomed to answer—Sir, if your wife had a lover, would you consider her as a very honest woman? This question troubles almost all of them, and they see very quickly that in order to settle upon my diagnosis, I have recourse to means rather more certain than the morality of the source. A woman perfectly healthy, I have already said, may be a source of infection.

Among the curious and singular facts which have passed under my eyes, permit me to relate to you the following, which has also its morality, as you will see. A young and small household had invited to breakfast a friend of the husband. The repast was almost terminated, and the appetite was not satisfied. It was decided that a morsel of cheese should be added to the feast. The husband leaves the table, descends four pair of stairs, and runs to the neighboring grocery to seek the complement of the friendly repast. Alas! he does not return sufficiently quick. During his short absence, and between the pear and the cheese, his unfaithful better half committed adultery with his perfidious friend. The husband returns, the repast is finished, coffee and its accompaniments are taken, the friend retires, and the husband in his turn consummates the conjugal act. Three days after, the husband comes to me with a chancre of the urethra, with symptoms of blennorrhagia. He was accompanied by his wife, and he assured me that he had had relations with no other woman. The most careful examination of the genital organs of that woman did not permit me to discover any thing suspicious. My prescription made, these individuals went away, leaving me without explanation of this virulent blennorrhagia of the husband. But the next day the wife returned, to ask me if I was very sure that she was not diseased. I examined her anew, and again I affirmed to her that she was perfectly well. Then she related to me the history which I have just told you, and she added that the delinquent was there, and begged me to examine him. I found upon him a magnificent chancre on the corona glandis, in the specific period.

This fact confirms the curious experiments made at the Lourcine by my young and learned colleague M. Cullerier. He placed some virulent pus in the vagina, let it rest there during some time, took it again upon his lancet, and inoculated with positive results, and the vagina, submitted to the treatment of injections only, was not infected.

You will conclude with me, my dear friend, that the source from which the cause of the blennorrhagia has been taken, cannot give a great value to the diagnosis.

I shall not return to what I have said of incubation as a means of diagnosis. The chancre of the urethra is sometimes devel-

oped very quickly, and can furnish pus at an early period. So that, far from considering the blennorrhagia as virulent which has taken more time to appear, it is the contrary that we must very often admit.

The *violence* of the blennorrhagia has been made a synonyme of the *virulence*. In truth it is just the contrary. As a general rule, it is those cases of blennorrhagia which are the least violent, the least painful, which ought to give us the most fear of the existence of a chancre in the urethra. The duration of the discharge is a sign not to be neglected. It is not the discharges the most tenacious which make us fear the existence of a chancre in the urethra. The nature of the secretion has great value when we know how to appreciate it. The secretion which is the result of an ulceration of the urethra, is much more purulent than mucous; it is ordinarily sanious, rust-colored, and charged with blood; the least pressure, moreover, upon the urethra, renders these characteristics very sensible. But to give to this symptom (the presence of blood) all its value, we must be certain that the patient has not previously used a caustic injection, that foreign bodies have not been introduced into the urethra, or that he has not had a rupture of the canal during chordée; and that, moreover, sanguinolent matter is not expelled with the last drop of urine, in which case it would be the sign of cystitis with vesical tenesmus.

I do not speak to you of the speculum for the urethra as a means of diagnosis of the ulcerations of this canal. It is an ingenious method, which has not given what it promised. It is sufficient sometimes to distinguish chancre, situated even at a considerable depth in the urethra, to cause the meatus to gape by stretching open the lips. Wedkind had thought that he found in the enlargement of the follicles in the neighborhood of the urethra, near the frænum, a symptom of virulence; but these enlargements are generally only phlegmonous, and independent of every other complication.

The most important symptom consists in the engorgement of the canal, especially in the region of the gland, the most frequent seat of chancre in the urethra.

I have already said, that it is not so important to be able to state the presence of an ulceration, either by the aspect and the nature of the secretion, or by inoculation, as it is to know if one is concerned with an ulceration capable of determining the syphilitic infection. It is this that all authors have had in view, when they have spoken of virulent blennorrhagia.

Well! as we shall soon see, it is the indurated chancre which is the fatal antecedent of the constitutional verole. Now nothing is generally more easy to prove than the presence of an

indurated chancre of the urethra with symptoms of blennorrhagia. If a blennorrhagic complication does not exist, the patients scarcely suffer in micturition; the jet of urine is generally twisted and troubled by reason of the diminution of the calibre of the urethra; the erections are not painful, when the chancre is seated in the region of the gland.

In order to well ascertain the presence of these ulcerations, it is necessary to explore the urethra by the aid of pressure which is exercised from above downwards, from the dorsal face to the inferior, as when we wish to make the meatus gape. In exercising this manœuvre, we perceive a cord, more or less extended, that some writers on syphilis have designated under the name of *corde balanique*. It is easy to ascertain, in the greatest number of cases, the side of the canal upon which the ulceration is seated. Independently of the indurations plainly limited upon one side, we see that side form a convexity, whilst the healthy side separates in forming a crescent. When the pressure is exercised from right to left, nothing is felt, the induration ceases to be appreciable.

Doubtless the swelling in the region of the gland or of the follicles may be only the result of a simple inflammation without virulence; but to complete the diagnosis we must have recourse to the accessory symptoms. Thus the affections of the glands are very rare in the blennorrhagia non-symptomatic of chancre. When they take place, as I have already pointed out, they are acute, terminate easily by resolution, or when they suppurate, it is simple pus that they furnish.

With the urethral chancre, dorsal lymphangitis of the penis and the affections of the glands are much more frequent. If the chancre is non-indurated, the glands suppurate almost inevitably, and when the seat of the pus is opened, the suppuration furnishes incontestable marks of virulence. In the indurated chancre of the urethra, which is the most important to recognize, the affections of the glands are inevitable and necessary; several glands are affected at once, and they remain indolent and do not suppurate—upon all which conditions, I shall have occasion to return hereafter.

Finally, if all these conditions have not been appreciated—if these signs have not been seized upon, either because we have arrived too late or because they have been overlooked, we can have the certitude, that if the patient has been attacked with blennorrhagia symptomatic of chancre, six months will not pass without the appearance of the accidents, if the constitutional affection has taken place.

We shall have next to examine whether, as a last resource, it is not better to wait this length of time to give a diagnosis,

than to cause the patient to undergo, during the same period, a mercurial treatment which, after all, does not afford more certainty.

NINTH LETTER.

My Dear Friend,—If I could think that your readers had remarked the interruption of my correspondence, and especially if they had complained of it, I should ask of you the permission to have me excused, on account of other imperious duties which have taken up the few and short instants which I could devote to you. I could easily contract the pleasant and charming habit of these periodical conversations with the numerous public, that your talent and that of your fellow laborers have known how to invite around your Journal. But you are so rich and so varied in this respect, that my absence could not cause any loss. I shall, however, do all in my power, in order that the good will of your readers may hereafter accompany me at least as regularly as possible.

I wish to terminate to-day what concerns blennorrhagia, by some words upon its treatment. You understand that in these letters, details would be idle and useless. I confine myself to the generalization of all these questions, the developments making the subject of a special and extended treatise, which I soon hope to be able to offer to the judgment of my friends. Here, I touch upon all the doctrines of the Hospital du Midi, and I ought to conclude that which treats upon blennorrhagia by some considerations upon the treatment of this disease.

When we see the obstinacy of certain writers on syphilis, in retaining the old ideas concerning blennorrhagia, recognizing and admitting only virulent blennorrhagia, it would seem that these writers ought not to establish the existence of any discharge without applying as soon as possible a mercurial treatment. But, it is not so. The greatest number of them content themselves with a rational treatment, and among them you will range M. Vidal, who does nothing but what I do, and perhaps less, for in what he has written upon blennorrhagia, although establishing nowhere, an absolute differential diagnosis between virulent and benign blennorrhagia, he does not speak at all of the antisiphilitic treatment properly so called. Look at the Treatise on External Pathology by M. Vidal, and you will be astonished like myself, that with his ideas upon the virulence of blennorrhagia in general, the treatment of my colleague should be so benign.

I have already said a word upon the astonishing and ridiculous custom of those who give copaiba and cubebs for the

blennorrhagia of bachelors, and who reserve mercury for whoever wishes to marry. This mode of therapeutics with two aims recalls to me the history of one of my old colleagues of the Hospital du Midi. He had in his youth, like many others, contracted blennorrhagia. At a later period he was to marry the daughter of an old writer upon syphilis, who was imbued with the doctrines of the treatment of precaution; he did not obtain the hand of his intended except upon the condition of a long-continued treatment with the liquor of Van Swieten. The treatment finished, the marriage is accomplished—all those who lived in intimacy with this colleague, and even those persons who were present at his clinical lectures, might have heard his frequent and bitter recriminations against this treatment of betrothal. As to the rest, this treatment has been very useless in the case of our colleague, for he had preserved an habitual discharge from the urethra, a last and peremptory argument, which he was in the habit of presenting to the individuals whom he did not succeed in curing of a similar inconvenience.

Others, more logical in appearance, in admitting the virulent blennorrhagia, and confessing nevertheless that they cannot distinguish it from the benign blennorrhagia, give at all hazards and notwithstanding, a mercurial treatment. Hunter is of this number, and his manner of reasoning upon the treatment of blennorrhagia is very curious. If Hunter had no other title to the thanks and the admiration of the wise, his writings would not have come down to us, and M. Richelot, your learned and modest collaborator and friend, would not have gifted France with his beautiful translation of the works of the great English physiologist. Let us hear Hunter. The following passage is not foreign to the question:—

“Whatever may be the method adopted for the treatment of gonorrhœa, whether locally or internally, we must not lose sight of the fact that a certain quantity of the matter of the discharge can be absorbed, and show itself afterwards under the form of constitutional syphilis. To guard against this effect, I think that small doses of mercury ought to be given internally. It is not easy to determine at what epoch this mercurial treatment ought to commence; but if it is true, as I have before explained, that the syphilitic diathesis once formed cannot be cured by mercury, while this therapeutical agent has the power to prevent a similar diathesis from being established, it is important that it should be commenced early, and should be continued until the end of the disease, not only until the secretion of pus has ceased, but also some time after. Mercurial frictions can be employed, when the stomach and intestines cannot support the medicine.

"This practice is much more necessary, if the discharge has existed for a long time, especially when the treatment is composed of simple evacuants only. In fact, when the discharge is of long duration, the absorption has more time to exercise itself; and when recourse has been had to evacuants only, there is more reason to fear that this has taken place, inasmuch as this treatment has no faculty to expel the virus from the economy.

"To prevent the establishment of a constitutional virus, the consequence of the absorption of the venereal pus, it suffices to prescribe a grain of mercury every evening, or morning and evening; but it is necessary to continue the employment of it in proportion to the duration of the disease.

"The success of this practice can never be verified in any particular case, because it is impossible to say if the pus has been absorbed, excepting in those cases where it forms buboes; and every time that we remain uncertain as to the reality of the virulent absorption, it is impossible to affirm that a constitutional syphilis will be manifested, if mercury has not been given; for among those patients who have not taken mercury, we see few who are attacked with constitutional symptoms, consecutive upon a gonorrhœa. However it may be, it is prudent to prescribe a mercurial treatment; for it can be admitted with reason, that we shall often thus prevent the establishment of a constitutional syphilis, as takes place when we administer it to patients affected with chancres or buboes, which under this treatment would certainly determine a general infection, as experience has taught us."—(*Complete Works*.)

I ask pardon for this long citation; you know that it is not my custom; but it appeared to me so much more necessary, as this doctrine serves still as the basis for the reasonings and the practice of a great number of writers upon syphilis.

Must I first insist upon the manner in which Hunter admits the constitutional infection from blennorrhagia? It is not the part actually diseased that infects, it is the pus secreted! Evidently Hunter has never reflected upon this singular mode of infection, and those who have followed him do not appear to have reflected any more.

It is true that this doctrine has been singularly revised and augmented. Thus, you will find in a modern writer upon syphilis, that in blennorrhagia, the infection does not take place by means of that portion of the mucous surface which is diseased, but through the portion of the mucous surface of the neighboring part which has remained healthy, this alone having the power to absorb the virulent muco-pus; from whence it is necessary, my friend, to draw this absurd conclusion, that if

the entire length of the urethra was diseased, the consecutive infection would never be feared.

The *coques muqueuses* of Hufeland are also an emanation from the Hunterian doctrine. You know that he pretends that if the blennorrhagia does not oftener infect, it is because the pus is enveloped in some small mucous follicles (*coques*), from which it has not always the power to escape.

Let us return to Hunter, and be painfully surprised to see this great mind wishing to prevent infection by mercurial treatment, assuring us that the longer the disease has lasted, the more chances there will be of infection, and the more it will be necessary to give mercury: and not perceiving that if the mercury only acts by preventing the infection, its administration would be useless after a long continuance of the blennorrhagia, inasmuch as the infection would be already established and the mercury would have no power upon it. Be astonished that in spite of his uncertainty upon the action of mercury against the infection, he affirms in a manner so absolute its efficacy in doses so rigorously and mathematically determined! Be confounded, at not meeting in the passage cited but a tissue of wrong constructions and of contradictions. The mercurial treatment the most ordinarily excites blennorrhagic discharges, and Hunter wishes that it should be continued until the complete cessation of all secretion! How many patients, whose discharge does not stop, would be thus condemned to mercury forever! My colleague, of whom I lately spoke to you, would have been literally choked with mercury. What would have become, under the weight of a treatment so prolonged, of an old soldier whom I attended, who contracted blennorrhagia at the peace of Amiens, and who had it still in 1845—that is to say, for more than forty years?

This entire doctrine of Hunter is lamentable from its discrepancies. Shall I afford myself the pleasure of demonstrating this singular confession—"The success of this practice can never be verified"; and that one, more singular still—"We see few patients who are attacked with constitutional symptoms consecutive upon a gonorrhœa." Is not every question, dear friend, even from the confession of Hunter, reduced to this—that the mercury is useful only in the small number of those patients, whose blennorrhagia is due to a urethral chancre!

Thus everything, even error, comes to confirm the exactitude and the truth of the doctrine of the Hospital du Midi.

Lastly, the treatment of blennorrhagia brings us again into the presence of the theory of the half-way treatment of M. Lagneau, who regards blennorrhagia as a light form of syphilis, and advises against it a demi-treatment. We see peep out

here the demi-virus, and the demi-virulence, of our brother at Lyons, M. Baumès.

Demi-treatment! Light form of syphilis! Alas! there is unfortunately nothing light as regards the verole, unless it be the certain opinions of very grave men. Syphilis exists, or it does not exist. If there is syphilis, a treatment as complete as possible is necessary; we must make use of all the guarantees that a serious and methodical treatment can give. If the verole does not exist—good heaven, for what good is an anti-syphilitic treatment? How must we treat simple benign blennorrhagia? I repeat again, that I confine myself to the generalities of the question. First, one word upon the abortive treatment. You know all that has been said upon repercussion, upon the theory of the wolf shut up in the sheep-fold; you are aware of all the apprehensions which have been manifested in regard to the metastasis and the wandering about of the virus in the economy, occasioned by the abortive treatment of blennorrhagia.

This doctrine has always astonished me in presence of the facts which present themselves in crowds, and that, too, every day in practice.

First, it is incontestable that the greater part of the accidents to which blennorrhagia can give rise, never manifest themselves before the end of the first week; and it is from the second week, and most generally later, that we see these accidents take place.

On the other hand (and those who frequent the Hospital du Midi well know it), the greatest number of these accidents manifest themselves only in those cases of blennorrhagia where no treatment or an insignificant one has been made. Do you wish me to give you a singular proof of this? Here let me inform you incidentally that I profess a great deference for medical statistics, that precious instrument, which managed as it has been by the skilful hands of M. Louis, has rendered such incontestable services to our science. But M. Louis is the first to recognize and to proclaim that nothing is more difficult and more delicate than medical statistics; nothing which by its faults, or by its vicious application, could conduct to greater deceptions or to more deplorable errors. This profession of faith being made, I hope that no one can consider as an attack against statistics, or as a mockery of that precious instrument of research, what I am going to say relative to the causes of the accidents produced by blennorrhagia.

I said that the abortive treatment of blennorrhagia was very innocent of the accidents which may be manifested in the course of this disease. Do you know, in truth, what the statistics absurdly interpreted would teach in this respect? Why

that the most frequent antecedent of epididymitis is flax-seed tea. I possess upon this point enormous statistics, and the students of my clinique wait every day with an hilarious impatience, this final question, which I never fail to address to the patient affected with an epididymitis—have you taken flax-seed tea? The answer is inevitably affirmative.

What shall we conclude from these statistics and facts? Evidently that epididymitis, like the other accidents of blennorrhagia, is neither a repercussion nor a metastasis, nor any of those chimera by which some have desired to oppose the timely and abortive treatment of blennorrhagia.

I am profoundly convinced by my observation and by my long experience, that a blennorrhagia arrested the first days of its appearance, far from being followed by those accidents which are feared, will prevent, on the contrary, the manifestation of them. The abortive treatment of blennorrhagia is at the same time the prophylactic treatment of the consecutive accidents. Thus, in my practice, I have adopted the abortive treatment applied at the first moments of the appearance of the blennorrhagia. This is a point of doctrine upon which I cannot too much insist—the commencement of the disease is known, its end and its consequences are always uncertain. It is, then, of great importance for the patient to disembarass himself of his discharge as soon as possible. In spite of an old prejudice of which the practice of Bell could be the pretext, I profess, that the injections which constitute one of the most important parts of the abortive treatment, far from producing strictures of the urethra, as has been said and still repeated, form the best abortive treatment for these strictures. We can be assured that the quicker a discharge shall be arrested, the less shall we have to fear the organized alterations of the urethra; these latter are, as for all other mucous surfaces, the consequence of the duration of the inflammation. I well know that here, again, statistics have been invoked, and that cases sufficiently numerous have been brought forward, in which strictures have manifested themselves after injections. But this is a little like the flax-seed tea in the cases of epididymitis. From this fact only, that injections are found among the antecedents of stricture, we must not infer a relation of cause to effect.

Analyze well these observations and you will see that it is a question of long-standing cases of blennorrhagia which have resisted every thing—even injections; it is precisely because these injections have not cured the inflammation, that the stricture has followed—which fact does not necessarily imply their unskilful or untimely employment.

I do not wish to terminate this letter, my dear friend, without saying a word upon the prize which my honorable colleague and friend, M. Diday, of Lyons, has just established. You know that he offers the sum of 300 francs to whoever shall bring to him ten observations upon simple blennorrhagia which shall have produced constitutional syphilis. This idea is good, but do you think it sufficiently generous? Thirty francs for each observation so difficult to find—frankly, is it enough? I consider as beyond price one single fact of syphilis coming on without syphilitic cause; thus I shall not establish any price upon this point. Let my wise and spiritual friend permit me to say to him, that he would neither compromise his present nor his future fortune, in increasing a hundred fold the value of the observations which he demands.—[*Boston Medical and Surgical Journal*.

On Scrofulous and Rheumatic Inflammation of Joints. By EDWARD STANLEY, Esq., F.R.S., Surgeon to St. Bartholomew's Hospital.

[Mr. Stanley remarks upon the difference in the frequency with which certain joints are attacked by disease, and how exempt others seem to be from the like affections. An example of the former fact may be taken in the hip and knee-joints, and of the latter in the lower jaw, the sterno-clavicular articulation, or the heads of the ribs with the vertebræ. Some explain this by their greater or less exposure to external influences; but this cannot be the case, else why should the hip be more frequently attacked than the ankle-joint? Others say that joints are more susceptible from the activity of their functions; but few joints are more exercised than the lower jaw, and yet few are more free from disease. After a few observations on some cases of ankylosis of the lower jaw he proceeds:]

I now come to the consideration of strumous inflammation of joints, and before proceeding to investigate its phenomena, the following questions demand attention:—1st. What are the circumstances which would lead us to regard the disease as strumous, when brought to the bedside of a patient? 2nd. In what condition should we expect to find the structures, viz., the bones, cartilages, and synovial membranes of a joint, provided the disease be strumous? With reference to the first question, I am unacquainted with any local symptom, any precise condition in the affected joint itself, which would enable us at once to decide on its strumous nature. We must look

elsewhere. The age and aspect of the patient, the past or present existence of scrofulous disease in other parts, such as enlargement and suppuration of the cervical absorbent glands, strumous ophthalmia, tubercle in the lungs and other organs,—any of these, especially if actually co-existent, would justify us in regarding the disease as scrofulous. Often, indeed, these cases are obscure, and sometimes we are led to a wrong conclusion. The aspect of the patient is delusive, and should not be too much relied upon. Many instances occur in which the patient's appearance seems indicative of the existence of scrofula, whose subsequent progress and favourable recovery prove that such evidence is fallacious.

We have now to answer the second question. What is the state of a joint invaded by strumous disease? The morbid specimen I now exhibit shows the condition of the articular extremity of a bone in an extreme attack of this nature. The end of the bone is softened from absorption of its earthy matter, and its cancelli are filled with tuberculous deposit. It is, however, according to my experience, rare to meet with so complete an example of strumous disease as this specimen furnishes. In the majority of cases, I believe that no tubercular matter is found deposited, and when found, it is only in the last stages of the affection. Such a condition of bone, when it does exist, is, in my opinion, irreparable; and, when the surgeon is summoned to a case exemplifying the disease in this its latest stage, he can do nothing to restore the bone to its natural state, nothing to accomplish a cure. There is, however, an earlier stage in these affections, which you will often have to treat in private, although it is seldom seen in hospital practice—a stage amenable to treatment, a stage in which, generally speaking, the morbid impairment of the bone may be arrested, and its integrity restored. It is characterized by increased heat, and enlargement of the bone, immediately above the joint. There is, indeed, increased vascularity, and low inflammation of the bone, which is quickly followed by expansion of the cancellous texture, and absorption of earthy matter. Ultimately in bone thus degenerated, tubercle is sometimes deposited. Such, then, is the state of the bone in a joint affected with struma. The other structures,—the cartilages, synovial membrane, &c., are in a state of low inflammation, inflammation which has commenced either in the bone or the synovial membrane itself, and which, if suffered to advance, is followed by its usual consequences,—exudation, thickening of tissues, and sometimes suppuration. Now the appropriate treatment for an attack of this sort is, perfect rest for the limb, and removal of all weight and pressure from the inflamed joint, so as to insure, as far as possible, its

complete tranquillity. If inflammation exists in any activity, the judicious application of leeches will be beneficial; but it should be borne in mind that leeches must not be lavishly employed, as strumous patients cannot stand depletion. The remainder of the treatment is constitutional, and should be directed to the restoration of the general health, if that has failed; to its maintenance if it has not. To this end country air, or, where it is practicable, a resort to the sea side should be recommended; a light, nutritious diet enjoined, and the state of the stomach and bowels be carefully attended to. The following particulars of a case which occurred to me some years ago, illustrates forcibly the truth of my observation, that the articular ends of bones rarely become the seats of tubercular deposition, even in well-marked examples of strumous disease.

A boy, ten years old, was under my care for scrofulous enlargement and suppuration of the cervical glands. While in the hospital, hip disease supervened, evidently of strumous character, which ultimately wore out the patient. Examination of the body showed that the joint was disorganized; the soft tissues around were infiltrated with tubercular deposit; the capsule and articular cartilages partially destroyed by ulceration; the bone was dislocated on the dorsum ilii; the acetabulum widened, and containing tubercular matter. The mesenteric and other absorbent glands were infiltrated with tubercle. Tubercular ulceration was present in the intestinal canal. However, when a longitudinal section of the head of the femur had been made, no tubercular deposit was found in the interior of the bone. Absorption of earthy matter, and widening of the cancelli, had taken place, but no interstitial tubercle existed.

Not infrequently disease in the soft tissues around a joint, inflammation, and abscess, are mistaken for disease inside the joint; and, in some instances, eminent surgeons have amputated limbs under the impression that an irremediable articular affection existed, while, in reality, the exterior tissues alone were involved, the joint itself being sound.

Joints are liable to another form of inflammation, differing from that we have just reviewed,—“rheumatic inflammation.” Examples of this disease occur generally in combination with rheumatic fever, and are, therefore, more prevalent in the medical than in the surgical wards of the hospital. The diagnosis of articular rheumatism is not usually difficult. When rheumatic fever is present, it is, of course, obvious; but when it is not, the implication of other joints, the cause and symptoms of the attack, and the history of prior rheumatism, will generally guide us to a right decision: the implication of other joints, because it is extremely rare to find rheumatism affecting one

joint only ; it attacks two or three simultaneously, or flies about from one to another : the cause and symptoms of the attack,—because we shall almost invariably find that the patient has been exposed to cold, or dampness, and because muscular pains are generally precursory to the articular inflammation. Rheumatic disease thus induced, is commonly marked by pain in one particular spot ; the patient does not complain of general pain in the joint, but points to one especial locality, and describes it as the seat of all his suffering.

Articular rheumatism is, moreover, intractable, leaving one joint and assailing another, or departing and recurring in the same joint. Joints are attacked by rheumatic inflammation in two ways ; either their fibrous structures, their ligaments, suffer, or their sinovial membranes. Now the consequences of rheumatic inflammation of the ligaments may be serious, such, indeed, as may terminate in dislocation of the bones of a joint. For, under its influence, the ligaments become soft and elongated, so as to permit the bone to slip out of the cavity in which it is naturally fixed. In this way the head of the femur may be displaced upwards on the *dorsum illi* without rupture or ulceration of either the capsula or the *ligamentum teres*. An example of such an occurrence happened some years ago in the practice of Mr. Lloyd.

A painter, in the enjoyment of good average health, was in the habit each morning of taking a warm bath. After having done so on one occasion, he experienced a pain in the hip-joint ; one of the joints of the fingers also became swollen and inflamed. He consulted a medical man, who gave him hopes of speedy recovery. Nevertheless, he remained in bed five weeks, after which, the pain having subsided, he was told to get up : this he found himself totally unable to do, and, on examination, the limb was found to be shortened and inverted, the head of the bone having been dislocated on the *dorsum illi*.

A case has also been related to me by Dr. Latham, in which articular rheumatism of long continuance produced dislocation. Some years ago, a young woman was in the hospital, under the care of Mr. Lawrence, suffering from rheumatism in the hip and wrist joints. She was confined for some time to her bed, and when permitted at length to get up found that she was lame, and that the lameness grew gradually worse. After a while she experienced a sensation as if the bone slipped from the socket when she walked. On examination, the limb was found of natural length, and its movements complete ; rotation, however, was remarkably free ; and when the thigh had been flexed on the pelvis, and was then rotated, the head of the bone could be evidently felt to pass over the brim of the acetabulum.

Cases like these illustrate the unusual results of a very common affection, which, although often obstinate and tenacious of existence, generally terminates well, leaving an unimpaired joint behind.

Rheumatic synovitis commonly ends in effusion. Ulceration of the articular cartilages may, however, supervene; and I have witnessed a case in which this condition was set up within nine weeks from the commencement of the attack, so that it was found necessary to amputate the limb. More usually, however, rheumatic synovitis gives rise to ankylosis, such ankylosis as may result from the adhesion of opposite synovial surfaces by effusion of fibrin, and which, as I have explained in an early part of the lecture, is called spurious, in contradistinction to true or osseous ankylosis.

Gonorrhœal rheumatism is a form of the disease occurring in conjunction with gonorrhœa, brought on by exposure to the vicissitudes of weather, and to the development of which, a certain unhealthy constitutional state appears necessary. Unlike ordinary rheumatism, it confines itself to one or two joints, and unshifting, clings to them with remarkable tenacity. It is, in truth, an affection that has long baffled the powers of medical surgery. In many instances the patients appear to recover, but the complaint returns on the slightest exposure, and no permanent cure is effected. There is now under my care, in Lazarus, a Pole suffering from gonorrhœal rheumatism of the wrist joint. In him the disease has yielded for the present to three grain doses of the iodide of potassium, given three times daily; and I am informed that the gonorrhœal discharge, which had become scanty, has reappeared since the mitigation of the articular disease. The best possible termination in these cases, — a termination which has ensued in the instance I have mentioned, is serous effusion into the joint; for when the fluid is absorbed, it is not unlikely a useful joint may remain. Some time back, a young man, aged twenty-one, was my patient in the hospital, in consequence of a most acute attack of rheumatism in the shoulder-joint, following gonorrhœa. Though he was in a reduced state, I ordered him to be bled from the arm; mercury was administered; in fact, very active treatment was adopted. Serous effusion in the joint resulted, and within five weeks I had the gratification of seeing him leave the hospital with the functions of the joint in a great measure restored.

We occasionally meet with examples of rheumatic synovitis occurring after parturition, which may originate ankylosis. The affection differs in no shape from ordinary rheumatic synovitis; but it requires gentle treatment, as the patients attacked

by it are generally much debilitated, and frequently suffering from some uterine complication.—[*Medical Times and Gazette. Braithwaite's Retrospect.*]

Simple mode of applying Pressure in Chronic Enlargement of certain Bones, &c.—[Dr. INMAN, of Liverpool, records the following case :

M. C., a delicate girl, three years of age, had a swelling of the left knee-joint, which was three-quarters of an inch larger than the right, due to an enlargement of the head of the tibia and condyles of the femur.]

Considering this to be a case more likely to be benefited by steady pressure than by more severe remedies, I had recourse, at first, to strapping; but finding this of no use I directed the mother to procure a piece of thin vulcanized India-rubber cloth, which she was to shape to the knee, allowing an interval in front of about three-quarters of an inch. The edges were to be bound by a piece of thin leather, and a piece of wash leather was to be placed as a tongue between the laced portion and the knee. Holes were to be made in the cloth behind the binding, and the whole was to be laced like ladies' stays. The elastic was to be worn during the day and night. Exercise was not prohibited, and no medicine was ordered. I saw her again in ten days. The bandage had been worn for fourteen hours daily, and taken off at night. The knee was reduced a quarter of an inch in circumference.

In ten days she called again, complaining of pain and œdema of the leg, in the evening, from pressure. On removing the elastic, whose edges now overlapped, I found the knee restored to its normal size and shape. The child could run about without pain or inconvenience. I directed the disuse of the bandage, unless the swelling should return; and told the mother, that, though I could not promise that there would be no return of the complaint, I considered the child as cured.

It is unnecessary to make any long comments upon this case. The fact of a morbid deposit in bone being absorbed, or, I might almost say, a threatened white-swelling, cured in three weeks, without blisters, issues, or caustics, without confinement, or even complete rest, is sufficient to speak for itself. Of course, such a plan can only be adopted when there is no reason to believe that active inflammation or purulent deposits are present, and where there is no severe pain.

The only novelty in the idea is, probably, the kind of elastic adopted, which is superior to the elastic web, both on account of its cheapness and its smoothness.—[*Ibid.*]

A New Mode of Reducing Dislocations of the Hip-joint without Pulleys or other mechanical means. By W. W. REID, M.D., of Rochester, N. Y.

On the 18th of December, 1849, Dr. Moore, having a subject in process of dissection by his students, proposed to me, that we dissect up the muscles of the hip-joints, leaving them *in situ*; dislocate the bones and then operate on them by traction in the usual way, and also by flexion, after my method, in order that we might observe the condition and action of the muscles, before and during both modes of operation.

We found it impossible to force the head of the bone through the capsular ligament, till we had made a slight incision into it. The head then shot through it, tearing it sufficiently to permit its passage, but the ligament seemed to fit close around the neck of the bone. As the head passed out, backwards and upwards, it caught the tendon of the pyriformis, tearing it off as it passed underneath and above it, which if it had remained entire, would have brought its tendon like a cord across the neck, below and close to the head, lashing it closely down to the dorsum of the ilium. We were at the time inclined to attribute its rupture rather to the decayed state of the subject than to excessive distention by the dislocation. But precisely the same thing occurred in dislocating the other hip, although we endeavored to avoid it by pushing the bone in a different direction, but as the insertion of this muscle is at the root of the trochanter, it is evident it must obey its movements, and therefore preserve nearly the same relation to it and the head of the bone; whatever direction it takes in being dislocated backwards, that is, whether a little more obliquely downwards or upwards.

When dislocated, the head of the femur rested on the gluteus minimum muscle! The gluteus medius and maximus, psoas magnus, iliacus internus, adductor, triceps and pectineus, were shortened and relaxed. Posteriorly, the obturator internus, gemelli and quadratus were greatly strained, and it was apparent that the pyriformis, if it had not been torn off, would have been more stretched than they. Anteriorly, the obturator externus was stretched, seemingly to its utmost, powerfully adducting the bone, and thus preventing abduction and rotation.

After carefully noting the relative position of the bone and muscles, we made traction on the femur downward and inward over the sound limb, as we are directed by most authors; but the moment the attempt was made the muscles already named as being in a state of tension, became more tense, and bound

the head of the bone more firmly down on the dorsum; although all the muscles about the joint were separated from each other, were loose, without vitality, and almost in a state of decomposition; yet it was with great difficulty that we could bring down the head into its socket; and when we did so, we carried away a part of the capsular ligament; and if the pyriformis had not been torn off already, it seemed impossible that it should escape rupture now. But when we abducted, flexed, and carried the limb over the pelvis, as has been described, the reduction was effected with great ease. We repeated and varied our experiments on both joints, as often as the subject would admit, and always with the same results. I was here enabled to correct one error which I had committed in operating. If we carried the knee above the umbilicus and pressed the thigh close to the body, on a line with the side, the pointing towards the axilla, as I have always done, we brought the great tendon of the gluteus maximus into strong tension, which would compress the trochanter so hard that it prevented the head from mounting over the edge of the acetabulum. The reduction was effected much easier by carrying the knee as high as the umbilicus, then abducting and rotating the thigh.

From the foregoing facts and observations, I think we may justly deduce the following propositions:

1st. The chief impediment in the reduction of recent dislocations, is the indirect action of muscles that are put upon the stretch by the mal-position of the dislocated bone, and not by the contraction of the muscles that are shortened.

2nd. That muscles are capable of so little extension beyond their normal length, without hazard of rupture, that no attempt should be made to stretch them any farther, in order to reduce a dislocation, if that can possibly be avoided.

3rd. The general rule for reducing dislocations should be, that the limb or bone should be removed, flexed, or drawn in that direction which will relax the distended muscles, and not by extension and counter-extension, for the simple purpose of overcoming the supposed contraction of muscles.

4th. Dislocation of the femur on the dorsum ili, an accident heretofore esteemed so serious to the patient, and so formidable to all surgeons, is reduced with the greatest ease in a few seconds or minutes, without much pain, without an assistant, without pulleys, without Jarvis's adjuster, or any other mechanical means, simply by flexing the leg on the thigh, carrying the thigh over the sound one upward over the pelvis, as high as the umbilicus, and then by abducting and rotating it.—[*Transylvania Medical Journal*.

Practical Observations on Diseases of the Ear, with records of Cases. By W. R. WILDE, Esq.

Condyloma of the external meatus.—M. D. L., a female, 25, a seamstress, suffering from deafness, tinnitus, occasional pain, fetid and sometimes bloody discharge from left ear for eight months; is otherwise healthy: attributes her affection to cold. The external meatus is completely closed by several condylomata which grow around its margin, but particularly from its lower edge. They are rather sensitive to the touch, lobulated on their surface, project a considerable distance beyond the margin of the aperture, and are a little more florid in colour than the natural skin. When the tragus is pressed backwards with the finger, a muco-purulent and offensive discharge exudes between these growths. It is not possible to insert even a small-sized speculum into the meatus without causing great pain and irritation. She could only hear the watch on touching. The right ear was normal. They were touched with the solid nitrate of silver, after which a poultice was applied. The subsequent treatment consisted in washing over the morbid growths with a strong solution of nitrate of silver every second or third day, and in the intermediate time, keeping a dossil of fine lint, wet with diluted liquor plumbi, applied to the concha, besides the internal administration of Plummer's pill and sarsaparilla. By persisting in this treatment for upwards of two months, the condylomata disappeared, leaving the meatus natural, when the membrana tympani was found unimpaired, and the hearing was restored.

I have seen a case of cutaneous cancerous ulceration, extending from beneath the zygoma, which had quite eaten away the tragus, completely occluding the external meatus; but the most common causes of the diseases are eczematous and herpetic eruptions, and chronic erysipelas, as detailed in the two subsequent cases.

Eczema Aurium, thickening and closure of meatus auditorius externus.—M. Q., female, 60, has been deaf, "off and on," for several years past, accompanied by noise and wandering pains in the head, with extreme itchiness in the auditory passages. The skin covering the auricle, and the scalp adjacent thereto, is of a fiery red colour, speckled with patches of yellow, formed by the exudation which has collected in thin branny scales all over it. The parts are hot, and in some places sticky, from a thin ichorous matter which exudes from the surface. The auricle has lost its natural shape, its folds and sinuosities being partially obliterated, and it has become hard, thickened and lumpy. The external auditory aperture has, owing to the dis-

ease extending into it, been lessened to a third of its natural size, and it is filled with a branny scurf. Upon removing the latter impediment, we can obtain but a very partial view of the membrana tympani, which appears to be thickened and opaque. Hearing distance, touching. The state of the parts is nearly the same on both sides.

In examining diseases of the external meatus and auditory tube, I find a small silver instrument, shaped like a blunt gorget, very useful.

Cases of this description, and, like this, of long standing, are very hard to manage, because there generally co-exists some constitutional taint, as shown in the cutaneous eruptions often manifest in other portions of the skin, and because the parts now under consideration have become so much altered in form and texture that it requires a long course of treatment to restore them to their natural condition, and thereby re-establish their usual functions. The disease principally occurs in females in middle and advanced life; but it also happens to children from six to twelve years of age. In the latter, however, it is of a much more active nature, at the same time that it is much more amenable to treatment. In young persons the eruption often co-exists with scald head, and in both young and old, if the disease is allowed to exist for any length of time, it extends into the meatus, and even over the surface of the tympanal membrane, which it thickens and renders opaque. In old persons a collection of branny scales accumulates in the external tube. In young persons a thick creamy discharge will generally be seen coating over the lining of the tube and the external layer of the membrana tympani.

Cleanliness and attention are indispensable to the eradication of these affections. In the first instance, continual poulticing with any emollient substance which keeps up heat and moisture is necessary. Linseed meal, boiled bread and milk, or well-mashed turnips will be found useful applications. Afterwards when the extreme heat, swelling, vesication, and redness have subsided, a solution of the liquor plumbi, in the proportion of a drachm to the ounce, applied with several bits of fine lint, so as completely to envelope the auricle, and the evaporation prevented by covering over the whole with a piece of oiled skin or thin gutta percha, rarely fails to lessen the irritation and reduce the parts to a healthy condition. But while we employ these local measures, we must not neglect constitutional means. Strict attention to diet should be enforced; salt meats, savoury dishes, and pastry ought to be avoided, and a sufficient quantity of fresh vegetables should be consumed at dinner. After the patient has been well purged, a course of

Plummer's pill may be prescribed with advantage—at least five grains daily for an adult; and, in a little time, some of the preparations of sarsaparilla administered in lime water will hasten the cure, and assist to eradicate the disease from the system. This affection is very apt to relapse, and we should therefore continue both our local and constitutional remedies long after the inflammatory symptoms have subsided. Old ladies think they never can have a sufficient amount of warmth about their head, and it is very difficult to induce them to leave off even one flannel nightcap; but we should at least make the attempt, as the head and ear ought to be kept as cool as possible. As the swelling and inflammatory symptoms subside, we should again turn our attention to the state of the auditory tube. If any discharge exists, the meatus should be syringed gently with tepid water daily; and both it, the concha, and the tympanal membrane, washed over every second or third day with a solution of nitrate of silver of the strength of at least twelve grains to the ounce. Still more advanced in the progress of this treatment when the exudation has completely ceased and the thickened cuticle has been quite removed, much benefit will be derived from smearing over the tube and membrana tympani with brown citrine ointment every third or fourth day. This latter remedy, of which I have great faith in all diseases similar to that which we are now considering, should be made with either rape or cod-liver oil instead of the lard or butter usually directed in the Pharmacopœias. It is then of a much darker colour, and never becomes hard or crumbly. It should be applied in a melted state with a camel's-hair pencil, and diluted by about one-third of almond oil.

Chronic erysipelas of auricle.—T. M., female, 52, has had frequent attacks of erysipelas of the head and face during the last five or six years. The effects of the disease are, however, now manifest only in the external ears, but more particularly the right. The auricle is not much enlarged, but has become hard, inflexible, and resembles a piece of wet thick sole leather; its fossæ being apparently filled up by subcutaneous deposit. It is also somewhat shortened in its antero-posterior diameter. The skin is of a dusky brown colour without any exudation, eruptions, or crusts upon it, but to the feel it is lumpy and nodulated, like what we find in certain forms of elephantiasis. The lobe in particular presents this thickened appearance. The disease has extended some way in front of the site of the tragus, which is also thickened and lumpy. The meatus is nearly closed. The treatment of this disease is very similar to that of the foregoing.

Dr. Kramer relates cases of scirrhus degeneration of the

auricle, but it would appear that he has applied the term to cases similar to that now under consideration.

Enlargement of the sebaceous follicles in the concha frequently present in pale, cachectic persons labouring under aural diseases. They are easily recognized by their dark heads, and can be pressed out with a pair of forceps. Besides these eczematous eruptions which I described in one case, many other cutaneous diseases affect the auricle, particularly in children. You are all aware of the excoriations which take place in infants behind the ears during dentition, and of the popular belief that they are salutary. Cleanliness is their chief cure when it is advisable to heal them up. We have a disease in Ireland so prevalent in some countries, that it would appear to be one of our national maladies—*pemphigus gangrenosis*, first described by the late Dr. Whitley Stokes, and of which I have given a description in the medical memoir attached to the Census of 1841. So fatal is this disease among children that no less than 17,799 deaths have been attributed to it in ten years; and as it is a disease very well known to the lower orders, I am inclined to think that the amount has not been exaggerated. It goes under different local names, but the most common are “mortifying hive,” “burnt hole,” and “black ear;” the latter from its so frequently appearing behind the ears and upon the auricles. In the Irish it is styled *ithcheadh*, or the eating disorder, from its phagedænic character. The vesicles, or bullæ peculiar to this affection, generally leave an indelible irregular lace-like depression, similar to that of vaccination.

Anomalous tumour in the external meatus.—M. N., female, aged 47, has been deaf of the right ear for many years, and is much annoyed by itching and a stuffed feeling in the meatus. Upon inspection we find a tumour in shape, size and colour, resembling a half-ripe mulberry, occupying the anterior and lower edge of the meatus, and extending some way into the auditory tube, which it almost completely blocks up. It is not unlike that disease known here as button scurvy, and to which the late Dr. Wallace, of this city, gave the name of *morula*. It is attached by its broad base throughout its whole extent, and has neither enlarged nor extended for the last three years, during which time I have been in the habit of examining it occasionally. The patient never had otorrhœa, and there is no discharge now present, but occasionally the tumour becomes painful and irritable, and its colour deepens from a florid red to a purple. It is now of a firm consistence, corrugated on its surface, and has a firm unyielding feel, quite unlike a *nævus*, for which at first sight it might be mistaken. I will not meddle with this tumour at present, as I once saw an affection very

similar in appearance in a lady about this woman's time of life end in frightful cancer, apparently from an endeavour to remove it by escharotics.

We meet with various other growths in the external meatus and auditory tube, independent of polypus or other morbid products, resulting from inflammation or its consequences. One of the most frequent of these is exostosis, of which I have seen very many examples. The projection generally grows from the posterior edge of the osseous portion of the tube, and slowly, but gradually projects forwards, so as to leave but a slight crescent-shaped fissure between it and the anterior wall of the meatus. The integuments covering such growths are generally very smooth, white, and polished. I have never seen this disease affect both ears, but I have often remarked it in connexion with inflammatory affections both of the external tube and the membrana tympani. Autenrieth has given an account of one of these growths, and lately Mr. Toynbee communicated a valuable paper on the subject to the '*Provincial Medical and Surgical Journal*.' We had lately at the hospital a young woman in whom the external meatus was closed by a dense membrane, through which, when an incision was made, a probe introduced into the wound came in contact with the bone. As the patient had never heard on that side, and had never suffered from any aural disease in infancy, as well as from the circumstance of there being no bony meatus, we must consider that case as one of congenital defect, of which there are several similar examples on record. I have on two occasions, in private practice, seen a structure of the auditory tube at the junction of the membrano-cartilaginous and osseous portions, in one instance so small as only to admit the round extremity of an ordinary dressing probe. Hardened cerumen having accumulated behind it, it was with great difficulty removed.—[*Medical Times and Gazette*.

The Use of Glycerine in certain forms of Deafness. By
DAVID STEEL, M. D., of Petersburg, Va.

Having read with some interest in the London Lancet an account of the use of glycerine in some of the most common forms of deafness, I feel it my duty, for the benefit of those who do not get the Lancet, to lay before them the result of my experience with this agent in some few cases that have come under my treatment.

July 17th, 1852. M——, aged 25, of scrofulous habit, complains of deafness, with a constant rumbling noise in both ears. He stated that he has been in this situation for the last two

years; thinks it proceeded from a severe cold taken about that time. Present condition: natural secretion wanting; aural canal dry and shining; membrana tympani slightly ulcerated in one. Ordered them to be well cleansed with warm water, which brought away some small lumps of hardened cerumen. Afterwards applied, by means of a fine camel-hair pencil, a sol. of argt. nit. grs. x, aq. distill $\bar{3}$ j, to the ulcer, and at the same time counter-irritation behind both ears by means of the ungt. cantharidin. After healing the ulcers, I directed the patient to take a small piece of cotton or wool, well saturated with glycerine, twisted into a small roll, and inserted into both ears as far as the membrana tympani. This to be continued night and morning, cleansing them well previous to its application. Patient being of a scrofulous habit, directed the following: R. Hydr. bi-chlor. gr. j; antim. et potass tart. grs. ij; ext. sarsaparilla $\bar{3}$ ss. M. Ft. Pil. mass dividend xx. One sum. ter die.

July 27th. Patient discharged, well.

July 29th. R——, aged 6: his mother stated that he could not hear her, unless spoken to in a very loud tone of voice, and that there was a continual watery discharge from both ears, which, to use her own expression, “soils his clothes so much that I cannot keep him clean.” In this case the membrana tympani in both ears were slightly ulcerated. All these symptoms supervened upon an attack of measles six months previous. Ordered, as in first case, ears to be well cleansed with warm water, and applied a solution of argt. nit. grs. v; aq. distill $\bar{3}$ j. After the ulcer healed, directed glycerine, as in the preceding case.

Aug. 25th. Patient discharged, well.

Aug. 11th. T——, aged 23, stated that about four months previous he had an attack of erysipelas in his head, which, by suitable treatment, was soon conquered, but left him in his present condition, viz., deafness in both ears, a constant, loud, rumbling noise, and a thin watery discharge from each, which, he says, “is very disagreeable.” No ulceration of the membrana tympani; directed ears to be cleansed with warm water, and the glycerine applied as in the preceding cases.

Sept. 1st. Patient discharged, well.

Aug. 19th. D——, aged 29, a native of New York, stated that when in that city he had an attack of acute inflammation of the internal and external ear, which, by appropriate remedies, was soon relieved of the heat, pain, swelling, &c., but left him with deafness and a disagreeable crackling noise in one ear. Directed same treatment as in case No. 3.

Sept. 15th. Patient discharged, well.

Remarks.—From an examination of the preceding cases, it will be seen that glycerine was the principal remedial agent used. Its use in these cases was determined on from reading a paper of Mr. Wakly, surgeon to the Royal Free Hospital, editor of the London Lancet, &c., and I can fully appreciate the high wrought eulogy bestowed upon it by that eminent surgeon. I have copied the above cases from my note-book to show the pathological conditions in which it is applicable, because it is not applicable in all cases; for in complete deafness, caused by paralysis of the auditory nerve, by displacement of the small bones of the ear or any other structural lesion, or where other senses are deficient besides those of audition, this agent offers but little hope of success; but in all cases where the natural secretion is wanting, the auditory canal dry, shining and inelastic—where sonorous undulations have been obstructed by hardened secretion—or where there is reason to believe a chronic inflammation of the mucous passages of the internal structure existed—this agent may in a majority of cases be used with success.

The preceding cases are not picked, but detailed as they presented themselves. Many more could be brought, (and many more are under treatment, the result of which I am unable to say,) to show the high value of this agent in the treatment of most of the common forms of deafness; but I trust I have adduced a sufficient number to bring the attention of the profession to a closer examination of this article, which, when done, I shall have accomplished the object for which this paper is intended.—[*Stethoscope*.]

Case of Stricture of Œsophagus. By WILLIAM JOHNSON, M.D.

I was consulted, the 23d of last July, by M—— C——, aged about 22 years, on account of stricture of the *œsophagus*. Her attention was called to it, about two years since. She was eating a piece of pickled cucumber, and was choked, and still more so very shortly afterwards, in eating a pear. Since then, the disease has been gradually increasing. Early in her disease, she could swallow nothing but liquid articles, or solid food very thoroughly masticated, and swallowed in very minute portions at a time. In fact, for some considerable time back, she has avoided taking any solid food; particularly animal, it chokes her even when well masticated, and swallowed in minute portions—liquid food sometimes does the same. She became very much alarmed, in consequence of the arrest of a very small cherry stone in her *œsophagus*, about the 30th of

June last. She was eating a cherry, and the stone accidentally slipped into the œsophagus, and produced the greatest distress by being arrested there. She resides about two miles from me. In my absence, my son speedily saw her. He directed a solution of tartarized antimony to be held in her mouth; it produced great nausea, and in an effort to vomit the stone was ejected. This patient is anæmic, probably from insufficient nutrition. Her appetite for food is good, but she is unable to take sufficient nourishment. The catamenia are regular. Ocular inspection of the fauces elicited nothing abnormal.

As tentative measures, I put her for a short time on tinct. ferri. chlor., iodid. potas., and applied an epispastic to the throat. The difficulty of deglutition was not in any degree relieved by these means. I now resorted to the bougie. My bougies were prepared, by saturating strips of muslin in melted bees wax, and when cold, cutting those pieces in such shape, as when rolled up tight they would be gently tapering. They were made of various sizes, from $\frac{5}{8}$ of an inch to $1\frac{7}{8}$ of an inch in circumference. I commenced, by introducing the smaller sized, and found difficulty in passing these, but the difficulty rapidly gave way, and in a few weeks I passed the larger sized with ease. The size that I used for the greatest length of time, was $1\frac{3}{8}$ inches in circumference. I finally passed very readily $1\frac{7}{8}$ of an inch in circumference.

The stricture, I found to be situated some inches below the termination of the pharynx. I passed the bougie some inches below the stricture, which did not appear to occupy a very large extent of surface. A sense of resistance at the stricture in passing the instrument through it, was experienced. After passing it a few times, most decided relief was obtained by the patient. She visited my office, nearly every alternate day, for three months, and the bougie was introduced at every visit, either by myself, or my son.

The sense of suffocation, produced by the presence of the bougie in the œsophagus, was very distressing to the patient, and she could bear it but a short time at each introduction. I generally passed it twice or three times every visit which she paid me, which was much facilitated by covering it well with glycerine.

In using the instrument, I departed from the directions given by Velpeau. He advises depressing the tongue with a spoon-handle or other instrument. This I did not find necessary, and he says nothing about throwing the patient's head far back, which I found very important. My method of introducing the instrument was, to have the patient supported by an assistant standing behind her. The assistant was directed to bring the

patient's head far back, so as to render the passage from the mouth into the throat as straight as possible, the instrument would then, by a very slight curve in it, readily pass down into the œsophagus. Its introduction was rendered easier by the patient protruding her tongue (which she could do with ease) from her mouth. The patient experienced no pain from the operation; nothing but the strangulation above spoken of.

The improvement in this case, was decided, from almost the first introduction of the bougie, until I pronounced her cured. I suggested, however, at parting with her, that it would be advisable to have the instrument passed occasionally into the œsophagus, but she found herself so completely relieved that she did not return, and I learn that she remains well, and has married since. Her mother stated, but two or three days since, that she swallows her food with perfect ease; the bougie has not been introduced since October last—nearly four months.

Remarks.—The chief point of interest in this case is, the rapidity with which it yielded to treatment. The case I would contrast with one related by Dr. Jameson, of Baltimore, in the 29th No. vol. viii., of the Medical Recorder, Jan. 1825; edited by Samuel Calhoun, M. D., of Philadelphia. The Doctor's case is a very interesting one, and will pay well for the perusal. Dr. Jameson passed ivory balls of different sizes, attached to a stem, upon which they were secured, nearly every day for ten months, before he considered his patient cured. My patient was cured by the bougie passed every alternate day for three months. Whether this result was owing to the different kinds of instruments employed, or in my case being a less serious one, I do not pretend to decide. Dr. Jameson's patient, too, was a female, but more advanced in life than mine; his being about 40 years of age, and his case, too, was of two years standing. For more than a fortnight he did not succeed in obtaining any advantages: but by indomitable perseverance, obtained complete success. His patient was one year under treatment. These two cases, in addition to others, give ground for encouragement in persevering attempts to dilate strictures, and it may yet be found, that well directed manipulation may obviate the necessity for resorting to the caustic, in the management of these cases.—[*New Jersey Medical Reporter*.

Veratrum Viride.

Dr. B. T. Kneeland, an intelligent physician of Livingston county, writes us in regard to the use of veratrum viride—"I am in the habit of using veratrum to quiet the action of the heart, in diseases attended with great arterial excitement, where it is

more fashionable to employ digitalis, and with far more certainty of effect than I could ever obtain with the latter remedy. I have thus used veratrum in rheumatism, pneumonia, the early stages of fever, &c. I first tried it in a severe case of pneumonia, the patient having already passed into a comatose state from what I considered defective arterialization of the blood, and with applications of cold to the head, and warmth to the feet, had the satisfaction of seeing a return of consciousness within twelve hours, and his recovery under the continuance of the same treatment. In a case of acute rheumatism, after employing the wine of colchicum, and various other remedies, without any apparent benefit, the pulse still continuing at 120, full and hard, I gave three grains of the powdered root of veratrum viride, which acted powerfully upon the circulation, causing temporary blindness, and reducing the frequency of the pulse to 85 per minute. He was now put upon lemon juice, and had a rapid and most satisfactory convalescence.

"I recollect to have read in one of the last numbers of the N. Y. Journal, a plan of treating continued fever, by giving tartar emetic until free vomiting is induced, and then relying upon large doses of quinine. Now I have ventured a step beyond this, and commenced the treatment of those cases which began with the usual symptoms of fever, with veratrum and have obtained the most gratifying results. It has seemed to *procure periodicity*, and thus prepare the case for the successful administration of quinine. With this treatment I have had no case last over five days, contrary to my former belief, that typhus fever must run until the morbid poison is eliminated. When the usual treatment is pursued the majority of cases prove fatal in this vicinity, and those which recover have a long and tedious course."—[*New York Journ. of Med.*]

Circulatory Displacement. By H. B. ORR, of Nashville, Tenn.

The process for extracting the active principle of medical drugs by circulatory displacement has until recently been regarded more as a speculation or theory than as a practical fact. Several years since it was spoken of by Mr. Alsop, of London, as convenient in making infusions; subsequently Dr. Benton suggested the idea of applying it in tincturing, and more recently Professor Procter has written in its commendation. But yet the intrinsic merits of the method have not received from pharmacologists that measure of regard which they seem to deserve.

My attention was drawn to this method of maceration by an article recently communicated to the American Journal of

Pharmacy by Mr. Laidly, of Richmond, Virginia. I was pleased with the suggestions induced by the perusal of Mr. L.'s paper, and was constrained to try the method in a variety of cases, and thus confirm, what indeed seemed to be at once obvious, its practicability: my expectations were entirely realized, and it is this that prompts me to commend its investigation to pharmacutists.

This mode of tincturation is very simple and convenient in its construction. The best form of apparatus is a cylindrical vessel with a movable diaphragm and a closely fitting top. The liquid having been put into the vessel, the diaphragm with the substance on it is then introduced so that a thin stratum of the menstruum supernates above it. The whole adjustment is then made and no further attention is required.

This kind of apparatus is very well substituted by a bottle with a large mouth, the size of the bottle depending upon the bulk of the ingredients, observing to keep always as much depth of liquid beneath the drug as practicable, and then a bag as great in diameter and as shallow as possible completes the structure.

It will be observed that this form of maceration is in all the important points different and directly opposed to the old method; they are opposite in principle also. In the old mode the drug is put at the bottom of the vessel—in the new it is kept at the top of the menstruum; in the former you arrive at an approximate saturation by shaking the mixture—in the latter saturation is obtained by the natural precipitation which goes on constantly: the one demands attention throughout, the other needs no watching; the old process is retarded by gravity, the new is facilitated by this principle, for gravity is the promoting cause of the circulation—it is really the *sine qua non* of the process. Immediately upon the adjustment of the apparatus, this principle is seen in active operation, by the colored solution descending in the vessel, (in consequence of the condensation in the union of the soluble substance with the menstruum,) affording ocular evidence that the solution is progressing. It is then a physical certainty that this mode is applicable, and we are forced to conclude that it is the only scientific mode of tincturation.

There is another feature in the process which will prove an additional convenience and abridgment in many cases, and which, so far as I am informed, has as yet been unnoticed. It is that of enveloping the substance in filtering paper before introducing it into the bag. By this arrangement the time and trouble of subsequent filtration is dispensed with. And this obviates an evil of some importance in itself; for with men-

strua of a volatile nature, the time requisite for their filtration affords opportunity for considerable evaporation, and in proportion to this evaporation the bulk of the solution is diminished, giving to it an uncalled for density, this being a condition without any provision and of evil tendency. But by effecting the filtration during the process of solution, it not only aids the pharmacist, but is of advantage with respect to the *quality* of his product.

The circulatory method is thought to be more rapid in its operation, generally, than the usual mode; in many cases it certainly is decidedly so, as an instance of which guaiacum may be adduced, as in the preparation of ammoniated tincture of guaiacum. Here at the commencement of the process the colored saturated liquid may be seen to emerge from out of the bag and then to commence its meandering descent, with a waving motion, mirroring a variety of colors, giving to the operation a most beautiful appearance. This descent of the solution and the corresponding ascent of the less dense or unsaturated particles of the menstruum, have conferred the appellation of "circulatory" upon the process. The solution of gums, resins and sugar also affords instances of the facility of this method.

In point of strength, the result must show favorably for it, for at each moment the most active portion of the menstruum is in contact with the substance, so that the depletion must be thorough as well as the action rapid. And I cannot but reiterate the expression of Mr. Laidley; I hope pharmacutists will more generally avail themselves of so simple and easy mode of extracting the active principle of medical drugs.

[*Southern Journ. Med. and Ph. Sciences.*

Chloride of Zinc Paste in Fistula in Ano. By E. LACY, Esq. Poole.

[Mr. Lacy recommends the application of chloride of zinc paste in fistula, on account of the perfect success resulting from it in three cases. It is applied as follows:]

The ordinary paste (chloride of zinc, water, and flour), being entangled in a grooved probe, is passed up to the opening in the rectum. The point of a second probe or instrument is then placed in the groove, and passed just sufficiently high up, that, by its being retained in that position while the armed probe is withdrawn, the groove may be cleansed of the paste. This may be repeated every second or third day.—[*Medical Times and Gazette. Braithwaite's Ret.*

On the cause of Decay in the Teeth of Children.

"What is the nature of that diathesis or constitutional predisposition or disorder (if any) which so often occasion decay in the teeth of our children?"—*Dr. Drake to the Miss. Val. Association, D. S.*

[We meet the following remarks upon this interesting subject in a small pamphlet entitled "A letter to Daniel Drake, M.D., on the cause of premature decay in the deciduous teeth, in which is embodied a review of the discussion of the Mississippi Valley Association of Dental Surgeons upon the same subject." Having examined, individually and collectively, the conclusions arrived at by this society, the writer says:]

"I now come to the second object of this letter, that of attempting an answer to your first question, which, according to my understanding of the subject, the 'Mississippi Valley Association of Dental Surgeons' have utterly failed to give. In making this effort I shall be very brief. I shall content myself by simply stating facts, without entering into any minute details.

The strict interpretation of your question embraces an enquiry into all kinds of decay to which the deciduous teeth are liable. Now as there are two prominent causes of this kind of decay in the temporary teeth—causes which differ from each other both in their nature and the time of life at which they commence—it is important, therefore, that they should be here referred to. The one is almost identical with the prevailing cause of caries in the permanent teeth, and usually commences its ravages in the molars, and that about the fifth or sixth year of the child's age. The other may be said to be *sui-generis*. It shows itself most frequently in the incisors, and is liable to commence at any period, from the time the teeth first show themselves through the gums, up to that when dentition is fully completed. Believing that your inquiries were intended only to embrace the last mentioned cause of disease in the deciduous teeth, I shall therefore confine my answer solely to a description of this cause; deferring all further notice of the other until I come to reply to your second question respecting the premature decay of the permanent teeth.

The 'diathesis, or constitutional predisposition, or disorder, which so often occasions decay in the teeth of our children,' is remotely constitutional in its nature, and appears to partake both of a scrofulous and of an acquired vice: of a scrofulous vice, because it is most prone to attack the teeth of scrofulous subjects: of an acquired vice, because the general system must be likewise much impaired for a shorter or longer period before the immediate cause of the caries of the teeth is induced.

The immediate cause of the decay is a malignant ulcer, most generally peculiar to the gums, but occasionally attacking the cheek.

The ulcer is most liable to occur at three particular epochs of a child's life, and appears to increase in malignancy with the child's increase of years.

The first period, and most common and mildest form of the disease, occurs during the cutting of the incisor teeth. It sometimes commences with the first appearance of these teeth—sometimes not until they have all acquired their full length through the gums.

The ulcer is generally confined to the extreme edge of the gums, revealing only a small, light, ash-colored, sigzag line, so small that it is rarely detected by either physician or nurse. Upon a close examination, the edge of the gum will be found separated from the teeth, and the body of the ulcer plainly seen between the edge of the gum and the necks of the teeth.

The gum reveals but little signs of inflammation, is not very sensitive, and may remain in the condition just described several weeks without any material increase or diminution of the ulcer.

The teeth become slightly loose, rough and dark colored, and are finally more or less destroyed, depending on the virulency and duration of the ulcer. *It is this form of the disease that occasions so many children to have dark colored, decayed, and broken off incisors, from the age of one year and upwards,* while the molars may be sometimes entirely sound. This form of the disease has never, before, so far as I know, been described.

The second period that the disease occurs, but much less rarely than the first, is about the time of cutting the molar teeth, and is sometimes very malignant. The ulcer usually appears on the gum at a point where a tooth is about to penetrate, and proceeds to a greater or less extent, often around the entire dental arch. The disease may appear in its mildest form, and run only along the extreme edge of the gums, or it may involve the greater portion of both the gum and alveolar process, loosening the teeth, turning them dark and softening them, as though they had been exposed to the strongest acids. This form of the disease has been occasionally observed and described.

The third form of the disease is fortunately still more rare, but fearfully fatal. It usually appears during the shedding of the temporary teeth, and although always commencing on the gum, it rapidly extends to the cheek, often destroying both jaw and cheek, as well as the life of the patient. This form of the disease has been described by medical writers under the name of *cancrem oris*.

All three of the forms of the ulcer just described are liable, and have often been mistaken, for the effects of mercury. But the sharp, ragged, yet well defined edge of the ulcer—the ex-coriating and corroding effects of the discharge—the slight tumefaction of the gums, and the terrible fetor of the breath, even in its mildest form—a fetor having nothing of the odor of that arising from mercurial salivation—are characteristics sufficiently marked to say nothing of the roughness and discoloration of the teeth, to prevent any mistake of that kind, if closely observed.”—[*South. Journ. Med. and Phys. Sciences.*

Hospital Sulphate of Quinine.

Mr. Edward Herring has introduced a preparation under this name, consisting of disulphate of quinine only partially purified. In its medicinal properties it is said to differ but little from the ordinary disulphate. It has a brownish color, and is, of course not admissible as a substitute for disulphate of quinine in general dispensing, but it has been tried in hospitals and dispensaries, and by some medical men who dispense their own medicines. The preparation is recommended on account of its economy. The final purification and decolorization of the salt being attended with some expense, the manufacturer is enabled to offer it in a partially purified state at a considerable reduction from the price at which it can be sold when purified in the usual way. The amount of impurity must be ascertained before its real value can be estimated. It may be a question whether the recognition of a preparation so imperfectly purified might not open the door to some abuse.—[*London Pharm. Journal.*

On Disguising the Taste of Quinine. By M. PIORRY.

A piece of chocolate should be half masticated, and retained between the cheeks and the teeth. The quinine draught is to be rapidly swallowed; and then the mastication of the chocolate is to be completed, so that it may be swallowed also. The taste of the quinine is thus hardly perceived.—[*Bull. de l'Academie. Med. Chir. Rev.*

Vesicating Oil. By E. DUPUY.

The solubility of cantharadin in chloroform, as shown by the experiments of Professor W. Procter, suggested to me the idea of using that vehicle in combination with a fixed oil to obtain a vesicating agent, freed from the disagreeable concomitants of the ordina-

ry fly blister, and retaining the cantharadin in a soluble state. I proceed thus :

Powdered cantharides, one part.

Chloroform, }
Castor Oil, } of each (by weight) one and a half parts.

To the powder was added the mixture of chloroform and oil in a close vessel; the ingredients were transferred, after some hours, to a glass apparatus, and the liquid displaced in the usual way. It amounted to about two-thirds of the original bulk of the liquid employed. A few drops of the vesicating oil applied to the arm of an adult produced a perfect blister in eight hours. Its easy application on any given surface may be of value as a vesicatory or epispastic. I would suggest the use of oiled silk over the application of it to the skin; by retaining the moisture of the skin it will favor the action of the oil.

[*Amer. Journal of Pharmacy.*

A New Remedy for Intermittent Fever.

Dr. Wm. M. Holton, of New York, in a note to the editor of the N. Y. Medical Gazette, proposes the "Prairie Dock" or "*Parthenium Integrifolium*" as a remedy for Intermittent fever.

"A decoction made by pouring hot water upon two ounces of the dried tops of the plant, has proved equal to twenty grains of quinine.

I have entire confidence in it, not having met a single failure in over thirty cases, some of which were severe.

I do not claim that it is superior to quinine, except that so far, no unpleasant nervous effects have been observed.

I claim to be the first to call the attention of the profession to this remedy, and trust that farther trial and chemical analysis will confirm my belief in its efficacy, and enable me to give to the profession proof of the value of an article, the active principle of which, I have no doubt, will equal quinine, while the supply is abundant, and obtained in our own country."

Epilepsy.

The readers of this Journal are acquainted with the views of Dr. Marshall Hall in regard to the pathology of Epilepsy, and its connection with what he denominates either *Trachelismus* or *Laryngismus*, according to the violence of the symptoms. They are also aware that he proposes Tracheotomy as a remedy for this formidable affection, and that this operation has actually been performed with temporary advantage. It seems questionable, however, whether the remedy is not as bad as the disease. Dr. Horace Green reports in the New-York Medical Gazette an interesting trial made of a less

objectionable method for modifying the innervation of the larynx in such cases. We subjoin an extract from Dr. G.'s report.

"In reflecting on this patient's case, in connection with the theory advanced by Marshall Hall, I recollected the fact, that spasm of the glottis occurring in my practice, in laryngeal disease, had, in many instances, yielded to the topical applications of nitrate of silver to the larynx; and at this interview, I proposed to Mr. B. to try the effects of *cauterization of the larynx*, before having recourse to tracheotomy. He consented, and with a sponge-probang I immediately applied a strong solution of the nitrate of silver to the interior of the larynx. At this first application of the remedy to the larynx, I remarked the unusual insensibility of the parts to the local irritant; for, although the ordinary caution, that of cauterizing the pharynx and opening of the glottis before passing the probang into the larynx, was not adopted, yet not the slightest cough, or apparent irritation was induced, notwithstanding the sponge, charged with a strong solution of the salt, (45 grs. to the oz. of water,) was carried deep into the larynx. On the following day the patient returned, and the application was repeated, and this was done daily for five or six consecutive days. It should be remembered that when this local treatment was commenced the patient was having some six or eight attacks of epileptic fits daily; and that with the occasional exceptions, of which I have spoken, these attacks had occurred daily, during a period of four years. After the first application of the nitrate of silver to the larynx, the spasms ceased, and *Mr. B. passed over a period of ten days without having a single epileptic attack*. The change that took place in this period in his mental powers was as marked as that which occurred in the diseased nervous functions. His mind, before obscured, became clear, his memory improved, and he exhibited a spirit buoyant and happy when freed from the evil spirit that had so long possessed him.

This intermission continued until the *tenth* day, when, after having been exposed to considerable excitement, in the transaction of some business, he had a slight epileptic attack. On the two or three following days he had several attacks on each of these days. The treatment was again renewed, and after a few cauterizations of the larynx the spasms again ceased and the patient escaped through another period of ten days when the attacks came on as before. It was now apparent that the disease partook of that intermittent character which has been observed by Dr. Babbington and other writers on Epilepsy.

The attacks had occurred so constantly, through so long a period of years, that I could not learn that any thing like periodicity had been before observed as a characteristic of the disease in this case. This feature, however, was so apparent now, that I determined to try the effect of quinine, in order, if possible to break it up.

Again were the applications of the nitrate renewed, and in order to produce as much effect as possible on the larynx, the strength of the solution was increased to eighty grains to the ounce of water, and sponge-probang, nearly straight, being used, the applications were

carried through the larynx and trachea to the bronchial bifurcation.*

As in each instance before, the spasms ceased after continuing the application a few days, and passing the critical period; but as the patient approached the termination of this second *decade*, he was directed to take ten grains of quinine daily, for several days. This was done, the applications were continued, and the patient passed over the critical period and on the *nineteenth* day, without experiencing the slightest attack.

In this period of immunity from the disease—a longer period than had occurred in many years, Mr. B.'s spirits and general health improved constantly, and he occupied himself in his business (that of collector) almost daily, during this interval. But near the close of the nineteenth day, or about the commencement of the twentieth, his disease again returned, and the fits occurred as before for several successive days. There was no difficulty, however, in arresting the spasms by the topical treatment, as soon as this period was passed; and this has been the case up to the present time. That is, no difficulty occurs in arresting entirely the attacks of epilepsy, for a period of from ten to twenty days, during which interval the patient appears in excellent health, in good spirits, and in the possession of his ordinary mental powers.

In three instances, since the commencement of this treatment has the patient been carried over the first decade, but in no instance, as yet, have we been able to get over the *twentieth* day, without a return of the disease. At this present time, January 27th, he is on one of his *intermission* periods, and he appears as exempt from all disease, and declares that he feels as well as he has felt at any time of his life.

As the patient has in only one instance, as yet, employed freely the anti-periodic remedy, it has been determined to enter again on its use, with the hope that by its *full* administration the intermittent character of the disease may be controlled, and the patient restored to permanent health.

Without endorsing all the views advanced by Marshall Hall, on the nature and treatment of some of the diseases of the nervous system, I am prepared to admit that the practical results in this case have already been such as to warrant the conclusion that in epilepsy not originating in organic lesion, a morbid condition of the larynx may constitute the essential connecting link between the primary exciting cause of the disease and the subsequent convulsions; and to warrant the hope that in recent cases of inorganic epilepsy, we have it in our power, by an entirely safe and facile mode of treatment, to *break up this connection*, and arrest the disease."

* At the present day, Mr. Editor, I trust this assertion can be made without having one's veracity called in question, or endangering one's *status* in any medical society to which he may belong!

Miscellany.

THE PINTO INDIANS.—We have often heard marvellous accounts of a singular race of spotted Indians called Pintos seen by our troops during the late war with Mexico. The following very interesting statement corroborates the opinion we had advanced with regard to the nature of the spots, in our unwillingness to admit the Pintos as a distinct race of men.

Account of a peculiar class of Aboriginal Inhabitants of certain parts of Mexico, known under the name of the Pinto Indians. By Col. A. C. RAMSEY, formerly of the United States Army. Communicated by EDWARD LUDLOW, M. D., of this city.

Interspersed among the native population of certain parts of Mexico, are to be found a class of Indians, in greater or less numbers, presenting characteristics so marked as at once to arrest the attention of the observer. To this peculiar appearance they owe the name by which they are familiarly known, that of "Pintos," or painted people, the word being an abbreviation of the Spanish word *pintado*, painted. It is impossible to convey by writing a correct idea of their appearance; no two of them are alike either in color or in marks on their persons. You will see a pretty formed girl with one hand white and the other black; her face spotted of a gray, blue, black, or white color. Sometimes a man will have one half of his face lead color, and the other half of a copper complexion.

Here you will see an arm partly white and partly black, as though you had "barked" it, and taken off one portion of the outer skin.

A foot or leg will be spotted black, blue, white, &c., while the other parts will be all of one color.

Many appear with dark skin on the face, with a large black spot covering the nose and cheeks.

I have often seen these people with the face all as blue as lead; again all black or red; while their bodies would be entirely of the natural Indian color.

Nothing on this earth more visibly depicts despair, and extinguishes all emotion of human sympathy within you, than the sight of some of these miserable beings. Some created a feeling of horror; but the larger portion of them are not revolting in their appearance, but only repulsive from their looks. They live for the most part in villages by themselves, speaking only the Aztec language, ignorant, poor, superstitious, and without any hope or ambition.

So little intercourse exists between the Pintos and the white people, that it is almost impossible to get any account of their origin, malady, or condition. I have heard various histories of them, some absurd enough, and some contradicted by well-authenticated facts; but have never seen nor heard of any description of them in print. Some say that they are a race of outcasts, who, from some far-off country, were ejected from the parent stock, and spread themselves along the Pacific

from Sonora to Guatemala; that their disease is not only hereditary but contagious, being of a syphilitic or venereal nature. Therefore, personal contact with a Pinto, to eat with the same spoon, drink out of the same cup, sleep on the same bed, or even breathe the same air, has been said to produce the disease.

Sanitary laws have prohibited their passing beyond their own territory.

None are permitted to enter Mexico, nor are they seen even in Cuamavaca.

In the valley of Puebla they are not found before reaching Cuetzala. In the late war with the United States, the regiments raised from among them were not allowed to defend the capital, or even to encamp with the other soldiers. The dread entertained by the people of the cities, among all classes, for these unfortunate Pintos is laughable.

It is rarely that a gentleman from the capital can be induced to make a journey to this part of the country. He lives in constant apprehension of infection, and would die of fright if among them. Hence one of the reasons why this magnificent valley is so little known.

It is said that white persons have sometimes been diseased by the Pintos; and they will tell you instances in Mexico, how these unfortunate persons have been infected by the most trivial intercourse, while studiously avoiding all contact. Of course their statements are to be taken with the same grains of allowance that are given by medical men to the most astonishing revelations of gentlemen who sometimes consult them when peculiarly afflicted.

It is not considered proper to speak to a Pinto on the subject of his disease or the cause of it. It is always good policy not to use the word in their presence, as it is a stigma of reproach.

To ascertain, therefore, something of their history, I have made many inquiries of intelligent officials who are stationed in their villages. But from the most part of these I could glean no information; they knew nothing about them, and they did not seem to desire to know anything. From others I was more fortunate; and although I may still be misinformed, yet there seems reason to believe that this strange affection originated in the last century in the following manner.

The volcano of Iurullo was formed on the night of the 29th of Sept., in the year 1759. This eruption, in which the mountain rose from the plain, and by which a large extent of ground totally changed its character, is admitted by all to have been one of the most astonishing physical changes known in the history of the world.

A large space of level country, in which sugar cane and indigo were cultivated, extended between the small streams of Cuitamba and San Pedro. This tract of land was known as the hacienda of San Pedro de Jorullo, and as one of the richest in Mexico.

In the month of June, 1759, a subterraneous noise was heard; hollow and rumbling sounds, accompanied with many earthquakes, followed in succession for two months, to the great terror of the inhabitants; and on the 29th of September, the mountain rose from the plain to the height of 224 feet. The panic stricken natives fled to the

mountains of Aguascara, to escape a terrestrial convulsion which extended beyond six miles square.

From this place of safety the flames were seen to burst forth on all sides for more than three square miles. Fragments of burning rocks were thrown up to a prodigious height. Clouds of ashes hung over, which were illuminated by the volcanic fire. The heated earth became liquid, and undulated like a tempest-tossed sea.

The streams of Cuitamba and San Pedro flowed into the burning abyss, which seemed only to add to the brilliancy of the flames. For many months this volcano was in perpetual convulsion, but finally subsided.

The natives fled forever from a spot which they believed to be cursed. But the exhalation of sulphur had affected them.

A strange eruption appeared upon their skins, accompanied with an itching and smell, which were both painful and offensive. The whole appearance of the people thus diseased underwent a complete transformation. The skin changed color; spots of every size, and on all parts of the body, began to assume a permanent hue. It was then discovered that the disease was infectious, and whole villages became contaminated.

Whenever one of these people went abroad, his person presented such strange colored marks that others could not believe them natural, but supposed them produced by paint, and hence their appellation of *pinto*, or painted.

The disease is only cutaneous, and is accompanied with eruption on the skin, and is easily cured by cleanliness and mercurial ointment. But as these people are too poor or too indifferent to resort to the well known remedy, or do not all use it in a village at the same time, the pest is not permanently eradicated, but returns to plague the whole population.

That the Pinto malady is not hereditary has been well tested by many facts. Although a man or woman *limpia*, that is, free from the taint, marries a Pinto, and in the course of time becomes Pinto, and also becomes so from continuous personal contact, still the offspring exhibits no marks or symptoms of the disease till more than two years old. If a child born of a Pinto mother be nursed from its birth by a person *limpia*, instead of by its Pinto parent, it never is affected, unless from accidental causes, afterwards. On the contrary, a child born of parents *limpia*, if nursed by persons Pinto, will, after the second year, show the usual appearances of infection.

I have found persons of intelligence, *gente de rason*, living all their lives with their families among the Pinto people, and never dreading the affection for themselves or children.

It is true their little ones are not permitted to sport with Pinto playmates, and in that respect they do not differ from careful mothers in other countries, who will not suffer their children to associate with dirty, and possibly diseased boys or girls.

For my own part, I have been much among the Pintos. I have lodged in their houses; I have slept on their *thalmé* or cane beds; I

have eaten of their cooking; taken water from the *tinaja*, or family jar; have drank the milk which their cows have given, and often using the same *xicara*, or calabash cup; and, when I could not help it, I have swallowed *tortillas*, or corn-cakes, which they have patted smooth and thin between their hands. In wading rivers I have clung with my arms around a Pinto, and have worn clothes washed and dried for me by Pinto women. By the margin of rivers or other streams, the Pinto marks are somewhat less perceptible and peculiar than in places more remote from water. The *balseros*, or men who are engaged in floating the calabash and cane rafts, and who are consequently every day swimming, exhibit but few indications of this affection upon their persons. But as they grow older, and quit that occupation, they assume one of the thousand varieties of the disease. No doubt that strict personal cleanliness would do much to relieve these people; and if the village priests would borrow a stray leaf from the Koran under this head, I think the church would wink at the interpolation.

The Pintos may be said to inhabit a mountainous country, as they are rarely found on the plains or in rich settlements. Thus we only discovered them at Cuetzala, where the mountain district properly commences; and further down the river, when the mountains are passed, they disappeared. I have observed, on former excursions, that Pintos are not found near the rich cities and open country round Tepeacuacuilco and Iguala, only 14 leagues distant from this place.* Hence I would infer that it is only their poverty that keeps them in the Pinto state.

It is to be hoped that further intercourse with these unfortunate beings will remove the dread and disgust entertained for them by the better classes of the Mexican republic. They are simple, kind, honest, hospitable, and industrious where there is any occasion to work. They are only troublesome when excited by liquor, insult, or wrongs. Their disease is a species of itch—nothing more. My advice to any one thrown among them would be to keep clean, and keep the ten commandments; with a white shirt, cotton clothing, and a clear conscience, there need be no fear of contamination.—[*N. York Medical Times*.

The best means to preserve Leeches healthy. By GUSTAVUS SCHÜLLER.—The apothecaries in Moldavia are, according to the sanitary laws, compelled to keep constantly a stock of at least 300 healthy and serviceable leeches. But, particularly as the hot summer months are the most prejudicial season for these animals, it often happens that in the time of the greatest need no leeches are to be obtained.

Amongst the various means which have been recommended to keep

* This account was written at Totalzintla, a village or pueblo on the left bank of the river Mescala. One of the roads leading from Mexico to Acapulco, crosses the river at this Pinto village. At the time it was written (last August), Col. R. was exploring and surveying the river Mescala, which takes its rise above the city of Puebla, and empties into the Pacific ocean at Zucatula, some three hundred miles west of Acapulco.—[Ed.]

leeches healthy, and to restore the sick ones, good well-burnt wood charcoal has proved, according to my experience, to be the best, as shown by the following experiment. I washed the charcoal well three or four times with fresh spring water, to separate the adhering ashes, and then laid it, while wet, and without breaking it smaller, in a large glass cylinder; put the sick leeches, recently washed, into this cylinder, but did not give them any more water, as enough was to be found in the washed charcoal. The glass was tied over with a piece of linen and placed in a cellar, where, for five days, the leeches were resigned to their fate.

After this period I found the leeches, to my satisfaction, perfectly well. They were quite in a condition to be used, which was not the case in their sick condition.

For two years I have treated my leeches in this way, and always retained them healthy and serviceable. The number of deaths amongst them has been very small. The only precaution I observe is to place all my leeches for eight days in summer on recently washed charcoal, and for two weeks in winter. The method is a cheap one, and one easy of execution.—[*Buckner's Repertorium, from Annals of Pharm. Am. Journ. Pharm.*]

Note on the Preparation of Liquid Glue. By M. S. DUMOULIN.—All chemists are aware, that when a solution of glue (gelatine) is heated and cooled several times in contact with the air, it loses the property of forming a jelly. M. Gmelin observed, that a solution of isinglass, enclosed in a sealed glass tube and kept in a state of ebullition on the water-bath for several days, presented the same phenomenon, that is to say, the glue remained fluid, and did not form a jelly.

The change thus produced is one of the problems most difficult of solution in organic chemistry. It may be supposed, however, that in the alteration which the glue undergoes, the oxygen of the air or of the water plays a principle part; what leads me to think this is the effect produced upon glue by a small quantity of nitric acid. It is well known, that by treating gelatine with an excess of this acid, it is converted by heat into malic and oxalic acids, fatty matter, tannin, &c. But it is not thus when this glue is treated with its weight of water and with a small quantity of nitric acid; by this means a glue is obtained which preserves nearly all its primitive qualities, but which has no longer the power of forming a jelly. Upon this process, which I communicated, is founded the Parisian manufacture of the glue which is sold in France under the title of "*colle liquide et inaltérable.*"

This glue being very convenient for cabinet-makers, joiners, paste-board workers, toy makers, and others, as it is applied cold, I think it my duty, in order to increase its manufacture, to publish the process.

It consists in taking 1 kilogrm. [2 and 1.5th lbs.] of glue, and dissolving it in 1 litre [2 and 1.9th pts.] of water in a glazed pot over a gentle fire, or what is better, in the water-bath, stirring it from time to time. When all the glue is melted, 200 grms. [7 oz. Av.] of nitric

acid (spec. grav. 1.32) are to be poured in, in small quantities at a time. This addition produces an effervescence, owing to the disengagement of hyponitrous acid. When all the acid is added, the vessel is to be taken from the fire, and left to cool.

I have kept the glue, thus prepared, in an open vessel during more than two years, without its undergoing any change. It is very convenient in chemical operations; I use it with advantage in my laboratory for the preservation of various gases, by covering strips of linen with it.—[*Chemical Gazette, from Comptes Rendus. Ibid.*

Sulphite Soda for Preserving Dead Bodies.—A prize of 2000 francs was awarded to M. Sucquet for his improved method of preserving bodies by the injection of an antiseptic liquid through the carotid arteries. This preservative liquid is prepared as follows: A current of sulphurous acid gas is passed through a solution of carbonate of soda of the strength of from 20° to 22° Baumé (sp. gr. 1.160 to 1.180), until the whole of the carbonic acid is displaced, and the solution contains a slight excess of sulphurous acid. The fluid should then have a specific gravity of 1.200. It is next placed in a vessel containing clippings of zinc, and allowed to remain in contact with the metal until it has become sensibly neutral, the blade of a knife dipped into it not turning brown on exposure to the air. From four to six litres of this preparation are employed to inject a subject. After twenty-four hours, dissection may be proceeded with, and continued, without any inconvenience, for twenty, thirty, or even forty days. One of the great advantages arising from the employment of the sulphite of soda, consists in the beneficial influence it exerts in cases of the accidents to which the operator is subject in the dissecting room.

During the five years in which this preservative liquid has been employed, no fatal case has occurred.—[*N. O. Med. Register.*

Discovery of Anæsthesia.—We have received a communication from Dr. C. W. Long, of Athens, (formerly of Jackson county,) who takes exception to the remarks under the Editorial head of our last number, and reminds us of the priority of his discovery of anæsthesia, as published in this Journal in December, 1849. The reader will perceive, on referring to the editorial remarks in question, that they applied to “the relative merits of the three claimants” before Congress (Drs. Jackson, Morton and Wells), and that they were directly elicited by the Congressional documents on the subject just received by us. The circumstance that the claim for compensation has been urged with great zeal and pertinacity (and consequent publicity) before Congress, by these gentlemen, for the last two or three sessions, without meeting with any opposition or counter claim at the hands of Dr. Long, was well calculated to lead one to infer that he had yielded the point, or relinquished his own rights. Moreover, as we were not

the conductor of this Journal at the time Dr. L. published in it his article, the full force of that communication had escaped our memory. We regret the omission to allude, in our remarks, to Dr. L.'s publication, for, on a reperusal of it now, we are free to acknowledge that the Doctor has very clearly established the fact that he did, in 1842, perform several painless surgical operations under the influence of the inhalation of sulphuric Ether,—and that he was thus early and subsequently seriously engaged in experimenting with this agent as an anæsthetic. We really think that Dr. Long has done himself great injustice in neglecting to bring his claims before Congress in competition with those of others. The Doctor's modesty and disinterestedness have concurred to make him rest satisfied with his own consciousness of merit. We trust, however, that he will not longer remain thus quiet, but use every exertion to throw the weight of his claims into the scales by which Congressional compensation is to be awarded—and nothing would give us more real satisfaction than to see the honor of that great discovery fixed upon the brow of a citizen of our own native State.

Medical College of Georgia.—The twenty-first Annual Commencement of this institution took place on the 1st day of March. The exercises were conducted at the Masonic Hall, in presence of a large concourse of citizens. The Dean reported to the Board of Trustees that there were in attendance upon the Course of Lectures just concluded, one hundred and seventy-one gentlemen—of whom 115 were from Georgia, 29 from Alabama, 21 from South Carolina, 2 from North Carolina, 1 from Tennessee, 1 from Mississippi, 1 from New York, and 1 from Canada.

The Degree of Doctor of Medicine was then conferred upon the following fifty approved candidates:

FROM GEORGIA.

David Adams,	-	-	-	Thesis on	Gastritis,
W. J. Arrington,	-	-	-	"	" Meningitis,
A. F. Attaway,	-	-	-	"	" Spinal Irritation,
James C. Carroll,	-	-	-	"	" Dyspepsia,
John S. Clements,	-	-	-	"	" Veratrum Viride,
R. J. Cochran,	-	-	-	"	" Pleurisy,
A. G. Couch,	-	-	-	"	" Typhoid Fever,
L. S. Cunningham,	-	-	-	"	" Gastritis,
L. W. Davis,	-	-	-	"	" Puerperal Fever,
W. H. Dean,	-	-	-	"	" Uterine Hemorrhage.
L. C. Fambro,	-	-	-	"	" Veratrum Viride,
W. T. Goldsmith,	-	-	-	"	" Dysentery.

FROM GEORGIA.

V. T. Hart,	-	-	-	Thesis on	Cynanche Trachealis.
J. L. Hamilton,	-	-	-	" "	Mortification,
Joseph Hatton,	-	-	-	" "	Scarlatina,
Henry Hicks,	-	-	-	" "	Typhoid Fever,
E. C. Hughes,	-	-	-	" "	Gonorrhœa,
John R. Humphries,	-	-	-	" "	Dysentery,
Loyd Knight,	-	-	-	" "	Lobelia,
Johnson Matthews,	-	-	-	" "	Dyspepsia,
L. S. Means,	-	-	-	" "	Dysentery,
John P. K. McWhorter,	-	-	-	" "	Measles,
L. W. Mobley,	-	-	-	" "	Fever,
R. F. Neely,	-	-	-	" "	Infantile Remittents.
D. S. Perkins,	-	-	-	" "	Injuries of the Head.
F. W. B. Perkins,	-	-	-	" "	Fetal Circulation,
Ormond Pinkerton,	-	-	-	" "	Intermittent Fever,
W. L. Rees,	-	-	-	" "	Typhoid Fever,
Aristides Reynolds,	-	-	-	" "	Conception.
L. J. Robert,	-	-	-	" "	Intermittent Fever.
John Scogin,	-	-	-	" "	Abortion,
B. D. Smith,	-	-	-	" "	Pregnancy,
J. T. Slaughter,	-	-	-	" "	Spinal Irritation.
J. W. Stephens,	-	-	-	" "	Abortion.
F. J. West,	-	-	-	" "	Typhoid Fever,
John S. Wilson,	-	-	-	" "	Retroversio Uteri.

FROM ALABAMA.

James C. Billingslea,	-	-	-	Thesis on	Amenorrhœa,
John E. Crews,	-	-	-	" "	Pneumonia,
B. C. Flake,	-	-	-	" "	Alcoholics,
R. T. Foote,	-	-	-	" "	Fracture of Cranium.
W. A. Johnson,	-	-	-	" "	Dysentery,
Wilson M. Liggen,	-	-	-	" "	The Vascular System,
James Penn,	-	-	-	" "	Leucorrhœa,
S. E. Thompson,	-	-	-	" "	Bronchitis,

FROM SOUTH CAROLINA.

S. C. Brunson,	-	-	-	Thesis on	Bronchitis,
W. W. Graham,	-	-	-	" "	Rheumatism,
J. A. K. Holman,	-	-	-	" "	Pleurisy,
G. W. A. McRea,	-	-	-	" "	Digestion,
J. W. West,	-	-	-	" "	Dyspepsia,

FROM NORTH CAROLINA.

N. H. Wiggins,	-	-	-	Thesis on	Empiricism.
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The Honorary Degree of Doctor of Medicine was also conferred upon Wm. J. Rusk, of Habersham County, and Joshua King, M.D., a graduate of Castleton Medical College, was admitted *ad eundem gradum*.

The Address to the Graduates was delivered by Dr. W. C. Ware,

of Abbeville District, S. C., one of the alumni of this college, and the Valedictory by W. J. Arrington, of the graduating class—both of which admirably chaste and appropriate productions have been requested by the Trustees for publication.

BIBLIOGRAPHICAL.

A System of Practical Surgery. By WM. FERGUSSON, F. R. S., Prof. of Surgery in King's College, London, &c., &c. 4th American, from the 3d and enlarged London edition—with 393 illustrations. Philadelphia: Blanchard & Lea. 1853.

Among the numerous works upon Surgery published of late years, we know of none we value more highly than the one before us. It is perhaps the very best we have for a text-book and for ordinary reference, being concise and eminently practical.

Manual of Physiology. By W. S. KIRKES, M. D., &c., &c., assisted by JAS. PAGET, F. R. S., &c., &c. 2d American, from the 2d London edition—with 165 illustrations. Philadelphia: Blanchard & Lea. 1853.

The general favor with which the 1st edition of this work was received, and its adoption as a favorite text-book by many of our colleges will ensure a large circulation to this improved edition. It will fully meet the wants of the student.

What to observe at the bed-side and after death in medical cases.—Published under the authority of the London Medical Society of observation. Philadelphia: Blanchard & Lea. 1853.

This little work, if carefully read by even old practitioners, cannot fail to be productive of much good. As a guide to the younger members of the profession in directing their attention specially to the best mode of investigating cases so as to arrive at correct diagnosis, it will prove exceedingly valuable. The great difficulty with beginners who have not been under the immediate training of an experienced physician, is continually found to be in the appreciation of the true condition of the organs and tissues. Let such provide themselves with this work and study it thoroughly—and they will find much of the difficulty removed.

Ranking's Half-yearly Abstract of the Medical Sciences.—No. 16.

A very good number of an excellent work—would be better if not so exclusively European.

A Clinical Phrase-book, in English and German. Containing the usual questions and answers employed in examining and prescribing for patients, &c., &c. By MONTGOMERY JOHNS, M. D. Philadelphia: Lindsay & Blakiston. 1853.

This is a little book designed for the use of physicians who practice in districts and hospitals in which the number of newly arrived Germans may render it necessary to be somewhat acquainted with their language.

MacLise's Surgical Anatomy ; with additions from Bourguery, &c. Edited by R. W. PIPER, M.D. Boston: Jno. J. Jewett & Co. Part 1.

This work will be completed in 8 or 10 parts, of quarto size, at 75 cents each—the first of which is before us. We like to see books made as cheap as possible, but unless the engravings appended to them for *illustration* be good, they serve rather to confuse than to enlighten the student. In the present instance the plates are very poor.

The American Journal of Science and Arts ; conducted by Professors B. SILLIMAN, B. SILLIMAN, JR., and JAMES B. DANA; aided by Dr. WOLCOTT GIBBS, of New-York, in Physicks and Chemistry, and Prof. ASA GRAY, of Cambridge, in Botany. Published at New Haven, Conn., every 2d month, at \$5 per year.

This is a periodical of which every American should be proud, and which ought to be taken by every lover of the natural sciences in our country—for it is as ably conducted as any similar work in the world, and it is the only repository of the contributions of our fellow citizens to the study of Nature and the Arts. The March No. contains 13 original communications, among which are the admirable papers of Jas. D. Dana on "Changes of Level in the Pacific Ocean"—of A. Bigelow on the "Mounds of the Tensaw River"—of M. A. DeQuatrefages "on the Phosphorescence of some Marine Invertebrata"—of J. D. Dana "on the question whether Temperature determines the Distribution of Marine species of Animals in Depth"—of J. L. Smith on the "Re-examination of American Minerals"—of W. Hopkins on "The causes which may have produced changes in the Earth's superficial Temperature," &c. In addition to the original articles, each No. contains a résumé of scientific intelligence.

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We are also indebted to publishers and authors for the following works, which our space will not permit us to notice more fully at present:—The Transactions of the College of Physicians of Philadelphia, Vol. 1, No. 9—Report of the Pennsylvania Hospital for the Insane, for 1852—The Prize Essay of Prof. Flint, on Variations of Pitch

in Percussion and Respiratory sound and their application to physical Diagnosis—Elephantiasis Arabum successfully treated by ligature of the femoral Artery, by Prof. Carnochan—Remarks on Osteo-aneurism by the same—New Views of Provisional callus, by F. H. Hamilton, M. D.—A Treatise on the causes, constitutional effects and treatment of Uterine Displacements, by W. E. Coale, M. D.—Smithsonian Report on Recent Improvements in the Chemical Arts, by Prof. J. C. Booth and C. Morfit—Sixth Annual Report of the Regents of the Smithsonian Institute—First Report of the Surgeons of the New York Ophthalmic Hospital—Syllabus of a course of Lectures on Diseases of the Eye at the New York Ophthalmic Hospital, by M. Stephenson, M. D.—A Discourse on the life, character and services of Daniel Drake, M. D., by Prof. Gross—Introductory Addresses, by Prof. Lawson of Cincinnati, Prof. Bowling of Nashville, Prof. Hughes of Iowa University—and sundry Reports and Documents on the subject of the Discovery of Anæsthetics.

We cheerfully place the "*Dublin Medical Press*" upon our exchange list, and hope that its accomplished Editor may be as well pleased with our efforts as we are with the specimen he has sent us. Dublin is now the seat of as much Professional ability as any city in the British Isles.

Whitlow.—Somebody proposes as an abortive treatment of whitlow the application, very early, of mercurial ointment to the finger. We doubt its efficacy, but if any one will try it, we would like to know the result.

A handsome present.—Messrs. Philip Schieffelin, Haines & Co., Druggists, of New York, have presented to the Medical College of Georgia a complete series of specimens of the *Materia Medica*, put up in large glass-stoppered bottles for exhibition to the Class. It is altogether one of the finest collections of the kind we have ever seen, and the specimens, being hermetically sealed, will long remain to attest the taste and liberality of the respected donors.

Dr. Marshall Hall is on a visit to the United States. We hope that he may visit this section of the country.

The American Medical Association will meet at New York on the 3rd of May next.

GREENSBORO, March 10th, 1853.

At a called meeting of the "Physicians' Society for Medical Observation of Greene and adjoining Counties," the following resolutions were unanimously adopted :

WHEREAS it has pleased an all-wise and mysterious Providence to remove by death from our Society, our worthy and highly esteemed brother, R. S. CALLAWAY, M. D., of Greene county, and whereas we deeply deplore his sudden and premature separation from our fellowship—

Be it therefore, Resolved, That our Society has lost one of its most zealous and devoted members, and our profession a member of unsullied character and strict professional integrity.

Resolved, That we, as a Society, most sensibly feel the void occasioned by the demise of our brother, and that we will ever cherish his memory as a forcible illustration of the true and faithful physician.

Resolved, That we sincerely condole with the family and friends of the deceased, and that we hereby tender them our heartfelt sympathy.

Resolved, That we wear the usual badge of mourning for the space of thirty days.

Resolved, That a copy of these resolutions be forwarded to his bereaved family, and that the Southern Medical and Surgical Journal, Temperance Banner, Christian Index, and Chronicle and Sentinel be requested to publish them.

JAS. Y. FOSTER, M. D., President.

D. C. O'KEEFFE, M. D., Sec'ry.

Death of Professor Horner.—It is with profound regret that we announce the demise of Prof. WM. E. HORNER, which took place on the 12th instant. He was long distinguished as the ablest American anatomist, and universally respected for his private virtues.

The British press announces the death of *Dr. George Gregory*, the author of the once popular treatise on the Practice of Medicine—also, that of *Dr. Jonathan Pereira*, the author of the very learned work upon *Materia Medica*.

Medical Society of the State of Georgia.—We would remind our readers that this Society will meet at Savannah on the 13th of April. We hope that there may be a full attendance.

SOUTHERN MEDICAL AND SURGICAL JOURNAL.

Vol. 9.]

NEW SERIES.—MAY, 1853.

[No. 5.

PART FIRST.

Original Communications.

ARTICLE XVI.

On Spinal Irritation. By JOHN T. SLAUGHTER, M. D., of
Villa Rica, Ga.

The great diversity of opinion relative to the pathology and treatment of disease, demands an investigation of all the symptoms which may supervene in the many affections which may come within the limits of our observation. Among them all, there is none that is so common, or so often escapes the physician's notice, as SPINAL IRRITATION; for the reason that there is no complaint made by the patient to call his attention to the fact that it exists.

The cause of all the phenomena in intermittent and remittent fevers, is attributed (by a number of physicians of the present day) to lesions of the spinal cord; and Professor Ford has tried to instil into our minds that it is not only always present in these, but may supervene in numerous other diseases—which doctrine has so far been corroborated by my own investigations.

The first case to which I shall call attention, is one of Intermittent fever, which, though it may not be one of much practical importance, may serve to throw some light on its obscure pathology.

CASE I. Miss M., aged about 10 years, was taken with intermittent fever. Two weeks had elapsed from the commencement of the attack when the paroxysms were entirely replaced by neuralgia of the inferior extremities, of the most excruciating character. Portions of the spinal column were tender on pressure. Topical applications relieved her, and the administration of quinine prevented a recurrence.

CASE II. Mrs. C., was seized (on the third day of her confinement in childbed) with a chill, followed by fever—with nausea, pain in the head, hurried respiration, tenderness on pressure over the uterus, tongue furred in the centre with red borders, bowels torpid, and the lochial discharge suspended. She had been in this situation two days, when I was called to see her. On examination, I found a portion of the vertebral column morbidly tender. Ordered, sinapisms to the vertebræ, warm cloths to the vulva, and a dose of the sulphate of magnesia. By the next morning she was so far relieved as to require no farther treatment.

CASE III. Mr. B. was attacked several years ago with remittent fever, which terminated in an intermittent. After its cessation there was left remaining in the right hypochondrium a sensation of chilliness, which was superseded by erratic pains along the course of the ascending colon: this eventually located itself in the right iliac region. Bowels costive; operations scybalous and mucous, streaked with blood—afterwards constipation alternating with diarrhœa; noise in the abdomen resembling the croaking of a frog—worse in the recumbent posture; a sound resembling the dashing of a liquid against the parietes of the abdomen, and skin of a dark swarthy hue. In the latter stage, troubled with hectic and pain in the left side resembling pleuritis.

This was his state for several years, during which time he was reduced to a mere skeleton. He had been subjected to treatment for dyspepsia, chronic hepatitis, and phthisis pulmonalis—exhausted most of the articles of the materia medica and patent nostrums of the day without experiencing any relief. On subjecting him to an examination, I found the third and fourth dorsal vertebræ very tender to the touch. Continued counter-irritants to the spine relieved him, without the

aid of any medicine internally. He still has occasionally a slight return of his old symptoms, but the application of a few dry cups dispels all unpleasant feelings.

Here was a disease, with all the symptoms of dyspepsia, which must have had its seat in the spinal marrow. But it is not only in this disease, but in many others which had withstood ordinary treatment for years, that I have found this the most prominent symptom. Beside topical remedies, I have also found the cold shower-bath to be of great benefit; and from observations which I have made, I am of the opinion that it is in such cases as these that Hydropathy wins her laurels. This I know, that the sufferings of many might be alleviated by the bestowal of the proper attention to this subject.

ARTICLE XVII.

A Frost-bitten Scrotum—Gangrene—Recovery. Reported by
N. D'ALVIGNY, M. D., of Atlanta, Ga.

Mr. J. W., a farmer, about 30 years of age, in travelling on horseback on the 3rd of January, 1852, had his scrotum frost-bitten, and exposed himself to the heat of a large fire, which caused itching all over the scrotum, and pain in the back and thighs. His condition becoming alarming, medical advice was procured on the 5th of the month.

Drs. S. Malone and W. McKown, of Fairburn, found the patient completely prostrated, and in the most excruciating agony. The scrotum was of a deep purple, and so much swollen that its bursting was apprehended at every moment; the slightest pressure on it increased the pain dreadfully. The pulse was one hundred to the minute.

6th. Aggravation of symptoms, the scrotum turning black.

7th. The scrotum bursting at its lower portion gave issue to a large quantity of whitish liquid.

8th. Mortification is complete, and the black color extending all over the scrotum.

9th, 10th and 11th. Mortification progressed to a fearful extent.

12th. I was called, in consultation. Found the patient very

feeble, and emaciated; the scrotum entirely destroyed on the left side—on the right side about one inch and a half remained, not yet mortified, the right testicle being adherent to it, and bathed in a mass of putrefaction. Anteriorly the root of the penis was the line of demarcation. Posteriorly the perineum was already interested in the disorder, showing a portion of the urethra naked. The left testicle was hanging by the cord; both glands perfectly sound.

I advised to continue the treatment usual in cases of sloughing, viz., the use of chloride of soda, and the administration of generous diet, wine and tonics.

I refused to perform an operation, which, under the existing circumstances, could not relieve the patient's sufferings, and would probably induce a fatal termination; but proposed to wait until the mortification should be arrested, and then, if no appearance of sound and abundant granulation took place, to perform castration.

15th. The mortification ceased, and granulations at every point of the sloughing surface formed with astonishing rapidity.

On the 29th, medical attendance was discontinued, the testicles being entirely covered with an artificial scrotum. In two weeks after the patient was able to go about, and he has since experienced no pains nor inconvenience of any importance. He is in a good state of health.

This accident has not deprived those delicate organs of the important functions assigned to them by nature.

ARTICLE XVIII.

Extirpation of Pharyngeal Tumours. By L. A. DUGAS, M. D., &c.

There are no surgical affections more frequently encountered than tumours, and yet there are none in which the surgeon's skill is so often painfully tested both as to diagnosis and treatment. If, as has been oft-times repeated, no surgeon however experienced should ever advance a *positive* opinion as to the true nature of a tumour before seeing its internal structure, the difficulty is, in many instances, not diminished when he is called upon to determine upon the propriety of extirpation.—And,

even after having advised a resort to the knife, he often finds himself surrounded by dangers to the life of the patient, and to his own reputation, sufficient to deter any one not firmly convinced of the propriety of the course to be adopted and of his ability to do justice to the patient.

Why these difficulties? Do they necessarily attach to the diseases in question—or, in other words, has the profession given to them the careful attention to which they are entitled? It is true, that we have some good monographs upon the subject of tumours, but they are far from being perfect—far from furnishing to the practitioner all the data he may need on entering upon the duties of his profession. The history of tumours to be found in the systematic works upon Surgery, is usually so meagre as to be worth comparatively but little, except in cases of the most ordinary simplicity. It becomes us therefore to accumulate and to report facts as they present themselves, in order that materials may be at hand for the construction of a work consisting not merely of generalities, but also of such details as may furnish specimens of whatever may be subsequently observed in practice. A volume that would contain nothing more than the history and treatment of individual tumours, so that we might find in it a parallel for any that we may meet, would be invaluable.

Tumours in the pharynx are comparatively rare, according to written authorities as well as our own observation. It may therefore serve the purpose to which we have just alluded to publish the following history of a formidable case recently treated.

Branch, a negro man about 35 years of age, the property of Mr. J. A. Smith, of Henry county, Ga., was placed under my charge early in February last. He first noticed about three years before, a small tumour behind the soft palate, which he represented as being very hard and painless. From that time it gradually increased in size, and was never painful, but rather inconvenient. I found the tumour filling the pharynx, extending upwards to the posterior nares, downwards as far as the larynx, and laterally from one tonsil to the other, forcing down the right one. The soft palate was carried forwards and downwards, so as to constitute a prominence the

size of a large egg, to the posterior surface of which the tumour was attached. Deglutition was so difficult that he could take no solid food—his articulation was very indistinct, and respiration considerably impeded when he would walk briskly, causing him then to breathe loudly and like a horse affected with the “bellows.”

Believing the tumour to be fibrous, I proceeded on the 10th of February to its removal, as follows:

Provided with actual cauteries, a syringe, sulphate of zinc, &c., to control the hemorrhage from the general surface and smaller vessels, I passed a ligature beneath the right carotid artery, and left it there, ready to be tied should this become necessary. The patient was then seated in a chair, and an incision made from the right angle of the mouth to the masseter muscle—which necessitated the ligature of the facial artery. In the third stage of the operation, a longitudinal incision was made from the side of the uvula to the roof of the mouth, through the soft palate, which was then detached from the tumour in the form of flaps. The tumour now presented a white glistening aspect, and was adherent, posteriorly and laterally, to the adjacent parts by strong cellular tissue. Having free access to the parts, the cutting instruments were laid aside, and the mass was seized with strong tumour forceps and drawn forwards, whilst my fingers were passed behind and tore asunder the attachments of the lower portion of the tumour. The fingers were then carried successively behind the left, the upper, and a part of the right portions of the mass, which was now removed. The entire mass thus extirpated constituted one distinct tumour; but there was still another left in the right side, apparently in intimate connection with and pressing down the tonsil with great force. It did not, like the former, present a white glistening surface to the eye, but was covered by a thin stratum of muscular fibres, derived from the pharyngeal muscles. Upon dividing this stratum with the knife, and pressing it aside, the tumour was found to be of the same character as the former—and it was likewise removed by the fingers and forceps, not however without much difficulty. It was found to be attached to the ramus of the lower jaw, near the sigmoid notch, to the pterygoid process of the sphenoid bone

and to the posterior aperture of the right nostril, and was brought away in separate fragments. Both tumours, when placed together, formed a mass about the size of a turkey's egg.

The patient bore this protracted and painful operation with wonderful fortitude. The amount of hemorrhage was smaller than could have been anticipated, but had to be checked occasionally by cold water thrown into the pharynx with a syringe. After allowing the patient to rest a little, the cheek was stitched and well brought together with adhesive strips. He was then put to bed with the wound of the neck partially closed, and the ligature was permitted to remain beneath the carotid until the following morning as a precautionary measure.

The patient's recovery was unattended with any circumstance worthy of note. He did remarkably well, and would have been sent home in about a fortnight, had he not taken cold which affected his bowels and induced considerable fever for eight or ten days more.

Will this disease return?—Microscopic examination by Dr. Harriss showed the tumours to be purely fibrous—nothing indicative of malignancy could be detected in it. Time alone will decide the question.

The structure of these tumours was very similar to that of fibrous polypi, but differed from them in not being pediculated. They were, on the contrary, closely attached to all the tissues with which they came in contact. From the history given by the patient, it appears that there was at first only one tumour, and that it was situated behind the velum palati. That in contact with the right tonsil was of subsequent growth. Would the early removal of the first have prevented the development of the second? Some years ago (in 1847) Dr. B., of an adjoining county, sent us a case upon which we operated, and which has thus far not been reproduced.

The subject was a negro woman about 25 years of age, who presented a tumour about the size of a small hen's egg attached to the posterior surface of the velum palati by a broad base, about the size of a half-dollar coin. In this instance, instead of slitting up the soft palate, we plunged a hook into the centre of the protuberance and circumscribed it with a circular incision

carried through the soft palate, thus leaving entire the lateral half arches and the uvula. The tumour was then readily drawn through the aperture thus made, for it had no posterior attachments. The wound cicatrized completely in a short time, leaving no deformity.

Augusta, March, 1853.

ARTICLE XIX.

Case of Dropsy, cured by External Diuretics. By H. A. BIGNON, M. D., of Augusta, Ga.

Having noticed in the June No. of your Journal a report of cases of Dropsy, treated by external diuretics, I was induced to give them a trial. The mixture used consisted of equal parts of tr. of digitalis, tr. of squills, and soap, of which one drachm was well rubbed over the abdomen three times a day.

The patient was a negro girl, belonging to Col. M. B——: she was about twelve years of age, and had been laboring under general dropsy for some months before coming under my care. The chest, abdomen and extremities were very much distended, and her respiration very much impeded, so much so that I was compelled to tap her, and succeeded in getting about three gallons of fluid; but as soon as the puncture closed the abdomen commenced distending again, and she became very nearly in the same condition as when I first saw her. I then began the use of the diuretic mixture, and in about two weeks my patient was well, and has continued so up to this time—five months.

PART II.

Eclectic Department.

Letters upon Syphilis. Addressed to the Editor of L'Union Medicale, by P. RICORD. Translated from the French, by D. D. SLADE, M. D.

[Continued from Page 223.]

TENTH LETTER.

My Dear Friend—To-day I shall speak to you upon syphilis. As you can have remarked, I have not lost sight for an instant of my point of departure.

What was it? To seek out the specific causes of those diseases considered venereal; to study in a more rigorous manner their mode of action, in order to arrive at last at a more exact knowledge of their consequences and of their treatment.

In the preceding letters, I have endeavored to show that if blennorrhagia can have a special cause, it was not always easy or even possible to distinguish this special cause from the common causes of the inflammation of mucous surfaces. I have endeavored to establish that this cause was not that which produces syphilis properly so-called; that its consequences were entirely different, and that its treatment, unless empirical, cannot be that which we ought to oppose to syphilis.

I should have been very happy to have merited in all respects the criticism of M. Vidal, who asserts that my efforts have tended only to prove "that two and two make four." If I should apply this to all that still passes in syphilopathy, this proof would not for every one be equally easy to arrive at.

The cause of syphilis not existing in blennorrhagia, where must it be sought for?

Do not require that I should precipitate myself into the depths of history. I have often descended there, and I declare to you, dear friend, that I think it impossible to discover the truth there. The farther one descends, the less light penetrates, and he arrives at a point where the obscurity is complete. So that, arrived at this point, authors only proceed by groping; they wander about without cessation, and lead us astray with them.

Where did syphilis commence? By whom did it commence? I much fear that these questions are forever insoluble. What we can affirm, is that syphilis, such as we know it at the present day, is not developed spontaneously in man; it appears to be always transmitted. And yet, as we have already remarked, we do not meet with it in any other class of animals. I well know that very recently your Journal announced that syphilis

had just been found in Italy in the horse. In order to believe this news, I await some more complete descriptions of the symptoms. It would, nevertheless, be rather singular, that syphilis, which they accuse of having been propagated for the first time in Italy upon the human race, should appear also for the first time in Italy upon the horse.

What strikes every man who studies history without preconceived ideas, is, to find in the ancient authors, and especially in those who were anterior to the epidemic of the fifteenth century, perfect descriptions of all that we know to-day, and which we range among the primary accidents. Could we trace out at the present day a description more exact and more true than that of Celsus? Galen goes even so far as to find some relation between the accidents of the genital organs and those of the throat. William of Sallicet knew that the primary ulcerations of the penis had been contracted from relations committed with filthy women; he established perfectly the relations which exist between ulcerations of the genital organs and buboes, &c.

What has been wanting to observers and historians of the verole, from the earliest times, is the more exact knowledge of the filiation of the symptoms, of the connections and origin of the primary and constitutional accidents. But what was the leprosy of that epoch? Was the leprosy of the Greeks or of the Arabs, which we know to-day, similar to the leprosy of those times? In no respect; for the leprosy was then often contagious, and it was frequently communicated by sexual intercourse. Evidently it was not our present leprosy. The Bible, in spite of all the efforts of commentators, enlightens us but little upon this point. Probably the divine inspirer of the sacred books might have had serious motives in leaving some obscurity upon this point. I have no pretension to retrospective science; the works of Astruc have frightened me too much, and I confess that I am little tempted to undertake so great a work for so small a result. But whoever studies syphilis, however little he may have his mind tormented by the anxiety to know, will ask of himself, what I have done a hundred times, what was this terrible epidemic of the fifteenth century, and where did it come from.

Some cotemporaries have made it come from the stars. I do not know that they retrospectively searched out what passed astronomically at that period, and I am myself unable to do it. But it is certain that syphilis always reigns although Jupiter is to-day much more temperate, and Saturn and Venus no longer deliver themselves up to conjunctions which had such unhappy consequences for the human race. We are, then,

forced to seek our explanation upon the earth, and to take our subject from a less elevated point of view.

This terrible epidemic, this veritable '93 of the verole (1493), which no cotemporary at first thought of making come from the new world, found this origin in the writings and in the active propagandism of Oviedo, from motives into which it is useless to enter, and of which we shall find the application in the religious, political and jesuitical history of the time. We know that it is this fable which has become the theme of the great romance edited by Astruc. Heaven preserve me from discussing this; it is a work that has already been well done by Sanchez. I will allow myself only a slight observation in a pathological point of view.

In order to have brought about an epidemic upon such a grand scale, it must have been necessary that all or nearly all the sailors of Christopher Columbus should have been infected with syphilis. It was necessary that during a very long voyage, which was then not made by steamers, the primary accidents should have remained at the period of progress or of specific *statu quo*, susceptible of furnishing the contagious pus that we shall soon study.

One thing is very remarkable, that the sailors of the fleet, having arrived at Lisbon and at Bayonne, did not first infect the women of those ports: and yet is it probable, that contrary to the habits of sailors of all times, these should have, after a long voyage, exercised continence after arriving in harbor? Well, it is not to the women of Lisbon and Bayonne, that they communicate their disease; they leave for Italy where they go to meet the army of Gonzalve de Cordova in May, 1495, and it is there that they communicate the verole—to whom? We know nothing excepting that it was in Italy in the midst of three armies—Spanish, Italian and French—that a disease, then known since 1493 or 1494, raged with fury, each of the belligerent parties repelling the disgrace of communicating it to the others.

I do not wish to insist longer upon this historical question so confused and obscure, and which I have not the pretension to wish to clear up. I only ask myself if this epidemic of the fifteenth century resembles our venereal diseases of the present day; and I find certainly not. The accidents that we observe to-day resemble infinitely more those that the ancients have described, than the epidemic of the fifteenth century.

Here, my friend, permit me to communicate to you, with the reserve and the discretion which similar things exact, an idea which I believe to be a fruitful one. I submit it as a simple indication to some young and industrious colleague, who shall

have the good fortune of finding himself in that happy period when consistent researches are possible. In studying with care the descriptions of the epidemic of the fifteenth century, I am struck with a fact, which appears to me to be of marked interest. The mode of the transmission of the accidents, their gravity, the predominance of the constitutional infection over the local phenomena, which are wanting, or which passed unperceived, all this appears to me to resemble much more what we recognize to-day as the acute glanders, and the farcy, than the verole. Van Helmont has published an analogous idea, which has not failed to have been considered perfectly ridiculous. He makes the verole come from the farcy, as the consequence of I do not know what ignoble beastly relations. Apart undoubtedly from the shameful source from which he drew his opinion, Van Helmont was perhaps not far from the truth.

Remark, my friend, that a knowledge of the glanders and of the farcy in man is very recent, and yet the liability of man to contract this disease, which has existed from all time in the horse, ought not to be a recent fact. How many men suffering from the glanders and from the farcy have been liable to be, and have been, taken for syphilitical patients!

The manner of the transmission of the epidemic of the fifteenth century ought to strike us. The disease was often communicated by the breath in churches, in confessionals, to such an extent that Cardinal Wolsey, accused of having the syphilis, was brought to judgment for having spoken in the ear of Henry VIII. This mode of propagation is entirely inexplicable for syphilis, which requires an immediate contact. I well know that all the authors of the time do not admit this mode of transmission by the sole contact of the breath. Fallopius ridicules very pleasantly Victor Benoit, who had seen some holy daughters of a convent catch the verole through the thick grates of the parloir. Fallopius believes that there was mixed with this, a little *holy water*. But in all cases could not the epidemic, which certain authors already, and Paracelsus among others, considered as a mixture of the ancient venereal diseases and of the leprosy, be more probably considered as a mixture of the ancient venereal diseases with the glanders and farcy—the glanders, so spontaneous and easily produced upon horses, and especially in time of war, and with the incumbrances which follow in its trail.

Study the symptoms, and you will first see manifested, and as if *d'emblée*, the gravest accidents; which does not happen with respect to the syphilis of the present day. You will see that inoculable pus was produced in all parts of the body, which you do not see in the syphilis now known to us. I do not

know if I am mistaken, but it appears to me that there is in this, a truly remarkable subject for research. I seem to see the first dawning of a truth which has escaped us, even to this hour. We shall owe this truth to the beautiful works of M. Rayer, and of his school, and of M. Renaud (of Alfort) upon this terrible disease with which man is found so sadly endowed; and in which I find such striking resemblances with the epidemic of the fifteenth century. What glorious things there are to be done in this matter!

Are we aware of what the glanders, transmitted from man to man, and removed from the horse, can produce? Do we know what its hereditary influence is? For individuals suffering from the glanders or from farcy can procreate, and we are completely ignorant of what would become of the product of these procreations.

I should be happy to awaken the zeal of some laborer in our science. There is here, it seems to me, an ample harvest of glory to reap. But I confess it, all these ideas are still agitated in my mind, in the vague domain of hypothesis. Your readers I can understand must be desirous to see me enter into the field of reality. I arrive there: adopting the conclusion of Voltaire, I say that syphilis is like the fine arts, of which no one knows the origin nor the inventor. But what I know is, that it is found to-day at a source, alas, too certain, and it is from this source that I shall draw it in my next letter.

ELEVENTH LETTER.

My Dear Friend,—We must now determine the source, where the specific cause, the morbid poison which produces syphilis, is to be found. This poison, we can at the present day call by its name, the *syphilitic virus*.

Well! this virus—I must needs recall the circumstance, inasmuch as endeavors have been made to cause it to be forgotten—was formally contested and denied, when I undertook my first researches in syphilopathy. This was the time when numerous physicians did not dare to give it this name without fear of compromising themselves. It was the time when the learned Jourdan, in an access of singular anger, cried out—"call it as you will, but do not give it the name of virus."

The source of this virus, I have obtained at the point of the lancet, upon which, however, I have not had the pretension of placing all science, as my honorable colleague, M. Cazenave, wittily accuses me.

It is in studying comparatively all the accidents reputed syphilitic, that I have succeeded in demonstrating that one alone

of these accidents would furnish regularly the purulent matter ; capable, in placing it under conditions which we shall determine, of producing, in virtue of a special irritation, an ulcerating inflammation identical to that which has been the source of it, and of reproducing in its turn the same special secretion, the same morbid poison, and this without limit.

The syphilitic lesion, source and origin of the secretion, placed in favorable conditions, produces fatally the phenomena which we have just indicated, and which is the primitive accident to which has been given, and which has preserved the name of *chancre*.

Every time, as I have already had occasion to remark, that we were able to see the surfaces from which we took the morbid secretion, which should serve for experimentation, it is only when there existed a chancre, that positive results could be obtained, and that we were able to reproduce the chancre.

Must I again say that my excellent colleagues, M.M. Puche and Cullerier, at Paris ; M. Baumés and Diday at Lyons ; M. Renault at Toulon, Serre at Montpellier, M. Thiery at Brussels, M. Lafont Gouzy at Toulouse, &c., have arrived in their very numerous experiments, absolutely at the same results as myself.

Every time that the chancre could be produced with a secretion which had not been taken immediately from a primary ulcer, the secretion was furnished by surfaces which could not be inspected. The small number of cases, exceptional in appearance, in which the chancre could be reproduced with a purulent matter taken from a non-ulcerated surface, find their rational and absolute explanation in facts analogous to those of which I have recounted the history. How can it be concluded that the surfaces which cannot be inspected are not the seat of chancre, inasmuch as they furnish absolutely the same secretion as the chancre? Ah ! if it was proved that the primary ulcer, fatal source of the syphilitic virus, could not be seated, excepting upon external surfaces which are always visible ; that the depths of the urethra, and the cavity of the neck of the uterus, could not be the seat of these concealed ulcerations—if this was proved, all would be said ; but does there exist one sole writer upon syphilis who denies the existence of the primitive ulcer upon all these regions, who does not know and who does not believe that all syphilitic ulcerations are not always visible ? How, then, can we deny the possibility of the existence of deep and concealed chancre, when it in itself furnishes the most undeniable proof, that is the secretion ?

It has been said that inoculation cannot serve any purpose in proving the existence of the specific cause of syphilis ; that it was preferable to confine ourselves to the ordinary results of

contagion to arrive at this proof; for with any pus whatever we can produce what I pretended to produce only with the pus of the chancre, while by the mysterious ways of common contagion phenomena are observed, which inoculation does not produce.

It is at least strange that these same arguments are equally employed, both by the maintainers of the syphilitic virus, and by those who deny its existence. In fact what do these physiologists say? That with any pus whatever, that with a cause no matter what, the same result was arrived at—that is to say, the production of every variety of venereal disease. And upon what do they rely to sustain this doctrine? Upon motives which could then appear reasonable; upon all the uncertainties which ordinarily exist under the circumstances in which the venereal diseases are contracted; for the want of examination of women; upon the great number of the accidents determined by the same woman upon several men while this same woman could leave other men entirely indemnified from evil consequences; finally, upon all the fables that we have already signalized and combated, and upon which one is truly astonished, after what the speculum has discovered, to see men of merit as incontestable as M. Cazenave wish still to ground superannuated doctrines.

But I am profoundly astonished that the partizans of the syphilitic virus, those who recognize in syphilis a specific cause and in its virus a specificity of action, sustain, that with any pus whatever effects can be produced analogous to those of the inoculation virulent *par excellence*. Do the partizans of these doctrines think that we could produce vaccinia or the variola by any kind of pus? If it was given to them to experiment upon purulent matters the source and origin of which they were ignorant, what would be their criterion for determining the nature of them, if it was not the effects produced? Is it not in this way that I arrived at distinguishing the syphilitic pus?

But to this objection of *any pus whatever* as proof of the inutility of inoculation, I have another thing to answer.

I have inoculated the same patient, and that a hundred times, with the pus of chancre, of balano-posthitis, with the muco-pus of urethral blennorrhagia, with the muco-pus of blennorrhagic ophthalmia, with the pus furnished by the phlegmonous inflammations of other regions; and while that of the chancre inevitably reproduced the chancre, the other kinds of pus remained without action. What do they want more than this proof, and what can they answer to it?

Another objection, however, has been made. They have said, the inoculation does not prove anything as to the nature

of the cause, from the effects that it can produce upon an individual already submitted to the infection; in other words, in inoculating the patient with the secretion that he himself furnishes, no conclusion can be arrived at, inasmuch as that if infected every wound can and ought to become syphilitic.

Herein is a strange error, the consequences of which might be very grave; a dangerous prejudice, which I am astonished to see again brought forward in our day with the sanction of observers who make pretensions to exactitude and precision. The facts which I have just recalled peremptorily destroy this objection. I well know that facts relating to leech-bites, for example, have been cited, which have taken on later the character of venereal ulcers. But be assured, my friend, these bites like every wound in a syphilitic patient, do not become virulent ulcers, unless they are finally infected by contagion. Apply leeches where there has been no contact with inoculable pus, bleed the syphilitic patients as much as you wish, practise any other operation whatsoever, and never, unless there has been virulent contact, will a virulent transformation be possible. Among the numerous observations, which I have collected in proof of the truth of this assertion, I will recall the following fact of the Hospital du Midi.

At the period when I had women in my wards, a patient affected with a phagedenic chancre of the vulva, with abundant suppuration, was seized with a pain in the tibio-tarsal articulation. Leeches were applied upon the painful spot. Some days after the patient complaining at the seat of the bites, it was easy to recognize that some had undergone a veritable transformation, and that they had become veritable chancres. One could believe for a moment in the influence of the general condition of the patient, and some of the students believed in it. As to myself, I had not the least doubt upon the mechanism of this transformation. In the first place, all the bites were not ulcerated—first proof. Secondly, the patient was seized with similar pains in the articulation upon the opposite side; a new application of leeches was made, but this time, in guarding the bites from every infecting contact, none of them underwent the least syphilitic transformation.

I have made an experiment more conclusive still. It has often happened that I had to experiment with the pus of a chancre upon a patient even then under the influence of a constitutional syphilis determined by a preceding contagion. Some comparative inoculations were made, and then again the matter of the chancre alone gave place to positive results. Thus, whatever may be said, it is impossible to compare a syphilitic patient to a bottle full of virus, which it would allow to escape

through the smallest opening. The image is poetical, but it is not just.

But in order that these results should be inevitably obtained, reason tells us, in the first place, that the *virulent matter* ought to be taken from a chancre at a certain period—that is to say, at the period of progress, or of specific *statu quo*. It is very easy to conceive this, and I am sure of not fatiguing you in trying to make you understand that if you take the pus for inoculation from the surface of an ulcer which is in process of reparation and of cicatrization, you will have a simple and inoffensive pus, which will give you negative results, and that the same accident interrogated at two different epochs will say to you, yes and no. You will conclude, then, with all observers of good faith, that there is here no contradiction in the results of experimentation, nor uncertainty, and that it is no evasion, no subtlety of doctrine, to explain facts opposed to the principles which I sustain, and similar to those of Bru. When Bru did not succeed in inoculating the pus of chancre, one of two things happened; either he made a false diagnosis and directed his attention to other ulcerations, or he took the pus from chancres *at the period of reparation*. There is no way of escaping from this dilemma; for I repeat it, and I am ready to prove it to the incredulous, if there are any still, *the pus of the chancre is inevitably inoculable*.

You will perhaps find, my friend, that I suffer myself to go too far in the pleasure of writing to you; but it is your fault, you never stop me. Profiting, then, by your good will, I will say that if the *virulent matter* composed of a special morbid poison and of a vehicle, is ordinarily formed of a thin, ichorous, sero-sanious pus charged with organic detritus, it does not always present itself with the same characters; it can offer all the known varieties of pus or of muco-pus. It can be acid or alkaline, contain animalcules or not. These different conditions which appear contradictory, and have also served as an argument to those who deny the existence of a virus, belong only to its vehicle, and change nothing of its nature, which remains always the same. There is but one circumstance important to signalize, and which experiments upon inoculation have verified—viz. that the putrid pus is not virulent, that gangrene destroys the virus—*it kills it*.

In order to act, whatever may be the seat of the chancre from which it has been taken, the virulent matter has no need of being recently secreted and warm. Preserved as vaccine is, it acts equally well. Artificial inoculation proved this, contrary to the opinion of Cullerier, which hitherto was in vogue in science.

Inoculation has proved the truth of the different modes of contagion, more or less contested, so far as necessity of the physiological action and of the orgasm of the part which furnished the contagion was believed in, and as it was thought that this ought to be yet warm at the time of infecting. The observations of Fallopius and Hunter of chancres contracted in touching the seats of public privies; those of Fabricus of Hilden of accidents taken in sleeping in sheets in which infected persons had already slept, and of so many others, in fine, have thus become incontestable.

You will still permit me to say a word upon the conditions in which the part which one inoculates ought to be. Whatever it may be, skin or mucous surface, no matter what region, it suffices to have *a simple solution of continuity without the aid of any physiological act, in order that the effect should be inevitably produced*: there is nothing here, as in the case of the variola and vaccine, which resists the primitive accident; there is no privilege of idiosyncrasy; the most perfect equality exists in the presence of a point of a lancet charged with virulent matter.

Thus, then, dear friend, the inoculation made with the pus coming from a primary accident, with the pus of a chancre, in the condition which I have just recalled, has always produced identical results, whether experimentation has had for subject the patient who furnished the pus, or whether the pus has been inoculated from an infected to a healthy individual, as some experimenters have done.

It has however, again been said—it is imprudent, rash and impossible to conclude anything from artificial inoculation; you impose upon nature conditions other than those in which she is placed during the contagion which we can call natural by contradistinction. And condemning this artificial inoculation, they thought they could say of it what is said of physiological experimentation—"Torture demands and pain responds."

Our celebrated physiologist M. Magendie, to whom you addressed your first, and so remarkable *medical letter*, will tell you what he thinks of this indignation of the poets. As for myself, who do not wish to speak with the same authority, I shall say that I do not contest the mysteries of nature, that I know she does many things by processes which she conceals from us. But I maintain, also, that it would be an unworthy weakness to seek to render her still more mysterious, and to thicken the veils which cover her; that it would be shameful to shut our eyes when she wishes to unveil herself.

Let us see, then, if there exists any real difference between the natural and the artificial contagion. I shall tell you what I think of this in my next letter.—[*Boston Med. and Surg. Journal.*

On the Treatment of Syphilis. By Prof. BENNETT, Edinburgh.

[The following remarks are taken from No. 8, of Dr. Bennett's very valuable Clinical Lectures:]

The treatment of syphilis may be said to be of two kinds,—namely, the simple and the mercurial. The profession are rapidly deciding in favour of the first, although some of its members still give mercury in inveterate cases. Many of those we meet with, therefore, have taken the drug, and we have to eradicate the effects of the mineral poison as well as that of the original disease.

The Simple Treatment is divided into internal or medical, and external or surgical. The first consists in the observation of certain hygienic rules, and the employment of general therapeutic means. The diet must be light and mild, meat and all stimulating viands retarding the cure; even with the lightest diet the hunger should never be quite appeased. The regimen must be the more diminished and rigid in proportion to the youth and vigour of the patient. Diluent beverages, decoctions of barley, liquorice, and linseed, alone or mixed with milk, should be taken freely, to the amount, indeed, of several pints a day. Perfect repose must be secured by confinement to bed. Constipation must be obviated by the use of emollient clysters or mild laxatives. The air should be maintained at the same temperature; this is an indispensable precaution in chronic, consecutive, and mercurial affections. Exercise is only useful in the convalescent stage. In chronic syphilis, however, it may often be carried to fatigue with advantage. Tepid baths, repeated three or four times a day, are always attended with advantage. General blood-letting is often required where the primary disease is intense, or the system excited and the patient plethoric, but should not be used indiscriminately.

In the external or surgical treatment, strict attention to cleanliness and the position of the diseased parts should never be lost sight of. Emollient decoctions or fomentations, or dressings of simple cerate, are the best applications, and the dressings should not be too frequently renewed. Leeches are generally necessary. The greatest benefit is derived from the external use of a concentrated solution of opium (in the proportion of about two drachms to one ounce of water;) it soothes excessive irritability in all cases. When the suppuration is moderated and the surface of the ulcer cleansed, stimulating dressings, consisting of solutions of the sulphates of alum and copper, the nitrate of silver, and subacetate of lead, favour cicatrization.

In inveterate cases, more especially those labouring under

tertiary symptoms, the iodide of potassium was introduced by Dr. Wallace, of Dublin, and used by him with considerable success. I have myself given it in numerous cases with benefit, in doses of five grains, three times a day, conjoined with emollient applications to the affected parts.

The Mercurial Treatment consists in keeping up slight salivation, by means of the internal administration of blue pills or some form of mercury, sometimes conjoined with mercurial frictions or fumigations, at least for the space of a month. This physiological action of the drug may be produced by administering any of its preparations continuously in small doses. If combined with opium, they act less on the bowels and more on the system generally.

It is necessary during its action, that the patient do not expose himself to cold. A certain irritability is produced, and the constant soreness of the gums, the metallic taste in the mouth, not to speak of the inconveniences of profuse salivation which occasionally occur, render this species of treatment anything but agreeable to the patient.

Both kinds of treatment have now been extensively tested. In the year 1822, the Royal Council of Health, in Sweden, having been charged by the king to conduct a series of experiments upon the different modes of treating venereal diseases, reports from all the civil and military hospitals were ordered to be drawn up annually. These reports establish the inconveniences of the mercurial system, and the superior advantages of simple treatment. In the various hospitals of Sweden, 40,000 cases have been done under treatment, one half by the simple method, the remaining half by mercury; the proportion of relapses have been in the first instance, seven and a half, in the second, thirteen and two-thirds, in one hundred. Dr. Fricke's experiments in the Hamburgh general hospital were first made public in 1828. In four years, out of 1649 patients of both sexes 582 were treated by a mild mercurial course, and 1067 without mercury, the mean duration of the latter method has been fifty-one days, and that by mercury eighty-five. He found that relapses were more frequent, and secondary syphilis more severe when mercury had been given. When the non-mercurial treatment was followed, they rarely occurred, and were more simple and mild when met with. He tells us that he has treated more than 5000 patients without mercury, and has still to seek cases in which that remedy may be advantageously employed. He has never observed caries, loss of the hair, or pains in the bones, follow his treatment, and in all such cases which have come under his care, much mercury has been given.

In 1833, the French Council of Health published the reports

sent in by the physicians and surgeons attached to regiments and military hospitals in various parts of France. Some of the reports are in favour of a mild mercurial course, others in favor of simple treatment. They all agree by stating the cure by mercury to be one-third longer than by the other treatment. At Strasburg, mercury was only given to very obstinate cases. Between 1831 and 1834, 5271 patients had been thus treated, and the number of relapses and secondary affections calling for the employment of mercury had been very small. No case of caries, and only one or two instances of exostosis, had been observed. Full reliance may be placed on these facts, as regiments remain in garrison at Strasburg for five or six years.

In the various reports now published, more than 80,000 cases have been submitted to experiment, by means of which it has been perfectly established that syphilis is cured in a shorter time, and with less probability of inducing secondary syphilis, by the simple treatment.

These facts are now very generally admitted, and malignant syphilis is gradually disappearing. Twenty years ago, the most frightful secondary and tertiary cases were met with, and the usual treatment was profuse salivation. At present, such cases are rare. Abroad, owing to the wise police regulations, the disease is infinitely more innocent even than it is at present in Scotland; and under the salutary influence of a mild and simple treatment, its virulence is daily abating.

In appreciating the value of this important revolution in practice, we should not forget to eulogize those who had first the boldness to introduce it. The credit of this is mainly due, in England, to Mr. Fergusson, and other British army surgeons, who practised it during the Peninsular campaign ('*Medico-Chir. Trans.*, vol. iv.,) and to Mr. Rose, of the Coldstream Guards (*Ibid.* vol. viii.) In Scotland, the writings and lectures of the late Professor John Thompson, of this university, were mainly instrumental in convincing Scotch practitioners of the evils of mercury in venereal diseases. In England, the Hunterian theory and practice have been deeply rooted, and in Ireland have been supported by the writings of Carmichael and Colles. Mercury in consequence is still very generally employed in those parts of the kingdom. The gigantic experiments made abroad however, ought to convince the most sceptical; if not, let them compare what syphilis is in Scotland with what it was, and especially observe that we never see an instance of the disease such as that now in the ward (Case 1,) unless the patient's system has been contaminated with mercury.—[*Lancet.*

Change of Color in an Adult Negro. By A. HAMMER, M. D.,
of St. Louis.

We are very much indebted to Messrs. Br—— and D——, M. D., in this State, for their kindness in giving us an opportunity of seeing and examining a very interesting case. It is a negro, named Joseph Daniel, born in Woodford county, Kentucky, residing now in Saline county, Missouri, who is a slave of Richard Robertson. He is five feet seven inches high, forty-three years of age, stout, and appears healthy, has never been ill, save transient rheumatic pains in various parts, and is at present suffering from slight pains in the right knee. He is, from the testimony of the highest authorities, born from black parents. His present condition, differing from that of his infancy and boyhood, is the following:

The whole frame, and especially the formation of the skull and features, present all the characteristics of a full-blooded negro. The color of the skin of the whole body is white, like the Caucasian race, that is, entirely dissimilar to that of the Albinos, but as natural as it can be in a white man, with the exception of the face; and a few spots varying from the size of the head of a pin to that of a five cent piece, on both wrists and the sternum, which spots are quite as black as in the negro—form and color of the nails being as natural as in the white man. The face is partly white and partly black, or rather darkish-brown. The lower portion of the face, from the nose downwards and bounded on either side by the anterior border of the masseter muscle, is entirely white, with the exception of a narrow stripe of black on the inferior lip, extending transversely across it on a line with the mouth. On the upper lip there is a similar stripe of black, broader than that just mentioned, extending upwards on the right side to the *ala nasi*. Both *ala nasi* are black. At the union of the nasal bones with the nasal cartilages a black stripe, from three to four lines wide traverses like a saddle the bridge of the nose, communicating on both sides with black surfaces on the cheeks; all the rest of the nose being white. Both cheeks, including the orbital regions, form a continual blackish-brown surface, each one being a triangle, more or less rectangular, and connected with each other by the above-mentioned saddle. The triangular blackish-brown surfaces spoken of occupy a large space on both sides of the face, and may be described in the following manner:—The superior side of each triangle corresponds with a line drawn from the internal angle of the supra-orbital arch, running backwards over the arch and somewhat downwards to the zygomatic process; the inferior side commencing from the

point of termination at the zigoma, passing downwards and forwards to the anterior edge of the masseter muscle at its attachment to the inferior maxilla; the anterior side is described by a line uniting the last point with the point at which it started—viz., the internal angle of the supra-orbital arch. It must not be understood, that these lines are strictly mathematical. They are irregular and somewhat denticulated. The forehead, with the exception of a few irregular spots of various sizes scattered over its surface, is entirely white. Strange to say, there is a white circle of about an inch in width, encircling the whole face, bordering superiorly the hair of the head, and running downwards on either side of the face over the masseter muscles and passing over the inferior part of the face. The ears are covered with minute spots of a blackish-brown color, giving to them a freckled appearance. The whole scalp is white. The hair is short, and as in the genuine negro, curly, being partly black and partly grey—the grey hairs most numerous at the temples—the eyes are those of the negro. The formation of the eyeball and its appendages is normal. All membranes and visible organs healthy, the cornea excepted, upon which in both eyes a complete arcus senilis, broad and corresponding to the whole circumference of the cornea, is visible. The conjunctiva is of a dirty-yellow tint, the pigment unimpaired, the iris of a beautiful brown color, sight perfect.

The hair on the mons veneris are fine, slightly curled, and yellowish white, but not colorless as in the albinos; of the same quality are the hair surrounding the anus and on the extremities. Those parts of the skin, which are white but exposed to the influence of the sun, as the throat, neck, anterior part of the chest, and hands, present a sunburnt appearance, and the epidermis upon them is very much wrinkled. His mental faculties are of a low order.

I have in my possession a Daguerreotype of this man, and have caused another to be taken at the Daguerreotype Gallery of Messrs. Dobyns & Spaulding, corner of Fourth and Olive streets, where any one having the curiosity to inspect it may have an opportunity of doing so.

History of the Case.—We have the most reliable testimony that this negro is descended of entirely black parents. He was black at birth, remained so up to his sixteenth year, when he was bitten in the scalp by a healthy dog, being unaware at the time of the dog's approach. The wounds received were not serious. About two weeks afterwards, however, his hair, which was before black, commenced turning grey. At the same time, the deep black of the general surface of the body

became uniformly lighter, some places becoming entirely white, while the surrounding parts remained of a light brown color. The white places mentioned, gradually increased in size, until the whole body was as white as we have above described. This change of color went on, from his 16th to his 25th year. From this time, up to the present, a period of eighteen years, his condition, as regards color, remains unchanged, with the exception, however, that the black patches on the face were eighteen years ago somewhat lighter than they are now. We are told, that they neither increase nor diminish in size, but in color, their intensity is influenced by the seasons. At twenty-five years of age, when the change in the color of his general surface was perfected, he married a full-blooded negress of a genuine black color. He has a son now seventeen years of age, well formed and vigorous, and as black as his mother, and not a single indication of approaching change like that of his father up to this time exists.

Remarks.—This case of a change in the color of the skin of the negro stands not alone. Several cases are cited, by Blumenbach, Bates, Gualtier, Le Cat, Rayer, Fisher, Rush, [vide *Cyclopedia of Anatomy and Physiology*, by Robert B. Todd, Part 1st, second Edition, London, page 86, in a note :] by Byrd, Jefferson and Morgan, [vide *Lectures on Physiology, Zoology*, A. S. O. : by W. Lawrence, Salem, page 265, in a note]

No one of these cases, quoted by the authors mentioned corresponds entirely with our own, some being instances of complete albinism in the negro, other's instances of a change in color, by no means so extensive, or involving such an amount of surface as this case. Among the cases of partial change, mentioned by Le Cat, one was, in his opinion, the consequence of a severe burn or scald.

The question now arises, how has this change been brought about, or in what does it consist? It is a question which naturally occurs to the mind of every one, but is by no means susceptible of a satisfactory solution.

We will, nevertheless, venture upon a few remarks, leading the attention of our readers to some of the more prominent features of the case, which seem to throw, at least, some light on this dark question. The case differs, as we have before said, from that of the negro-albino. Albinism is generally attributed to a decay and an arrest of development of the pigmentary cells—to a qualitative change of the blood.

With this explanation, we are usually content, though it is very unsatisfactory, in as much, as it does not explain the *cause* of this change.

The usual explanation of albinism is not at all applicable to

our case, for the pigmentum nigrum is seen on some parts of the surface. The blood cannot, consequently, be changed. There is merely an irregular distribution of the pigmentum, or rather the absorption of its elements from the capillary vessels is limited to a few select points.

We presume nobody will be so senseless as to assert, that the blood-vessels of any particular region of the body have a predilection for the elements of the pigmentum nigrum, and that they are not equally distributed throughout the whole current of the circulation. The irregular distribution of the pigment is good evidence to the exclusive humoral-pathologist of the part, which the tissues, or so-called solids, play in nutrition and the metamorphosis is of tissue. It is needless to add, that both, fluids and solids, are equally active, and that an error in either can give rise to a pathological state. Our suggestion is, then, that the entire act of metamorphosis, of tissue in the cutis, [*rete Malpighi*,] is changed in this case in such a way, that the tissues (solids) have lost the power of attracting from the capillaries the elements of the pigment, and that this power of attraction is preserved, unimpaired, only in a few places. If we draw too definite a line between fluids and solids, as the exclusive humoralists and exclusive solidarists usually do: we by no means attach to this distinction an antithesis, for we recognize, in the human organism, no separation of fluid from solid, but view them as the unity of organization, each having its special attributes, and between them a mutual dependence. We have been forced to make this remark, because of the difference existing between the schools.

Now, if an abnormal physiological act takes place, or a pathological condition occurs, and the blood (fluid) is evidently unchanged, we necessarily look to the solids, for the cause of the change.

Though we know very little of the part which the so-called solids, viz., bone, cartilage, fibrous tissue, &c., play in the processes of life—there is one tissue, viz., the nervous, of which a great deal is known, and much established. Our attention, then, in speaking of the solids, must be principally directed to this tissue—as it may be considered the representative of the solids. We would say, in one word, that we attribute a pathological condition, under such circumstances, to a *perversion of innervation*.

We must now recall to our readers, the fact, that this negro was bitten by a dog; that he is of a low order of mind, and that his change of color commenced immediately after this accident. Farther, we must note the fact, that, in one of Mr. Le Cat's cases, the change of color was immediately preceded

by a burn or scald, and that Le Cat considers the accident the cause of the change. We must farther remark, that in the Annals of Medicine, cases are related, in which the hair changed from black to white, in a single night, from excessive passions.

We allude to these instances, not in the hope of building up an infallible theory, but as matters worthy of some consideration. Would it appear unreasonable or absurd, to suppose, that a continued perversion of innervation is the result of excessive mental emotions? We are inclined to think not. Similar perversions of innervation, only lower in degree and less continuous, as far as the result is concerned, are of every day's occurrence, and nobody seems to be much astonished at them, while frequently occurring. This idea, which we have advanced, is but an hypothesis, and we do not contend that it explains the whole process. It remains for us to show the nature of the supposed perversion of innervation; but the present state of medical science does not enable us to do so.—[*St. Louis Med. and Surg. Journal.*]

Fracture of the Processus Dentatus—fatal only 5 months after.

[Professor Parker relates, in the New York Journal of Medicine, the following very interesting case of Fracture of the Processus Dentatus, in which the patient continued to follow an active occupation for five months, and then suddenly died.]

“On the 12th of August, 1852, while driving a ‘fast horse’ at the top of his speed on the plank road near Bushwick, L. I., he was thrown violently from his carriage by the wheel striking against the toll-gate. He alighted upon his head and face, about fifteen feet from the carriage. Upon rising to his feet he declared himself uninjured, but soon after complained of feeling faint: after drinking a glass of brandy, he felt better, got into his carriage with a friend, and drove home to Rivington-street, in this city, a distance of more than two miles. There was so little apparent danger in his case that no physician was called that night. Early on the morning of the following day, Dr. B. was called to visit him. He found his patient reclining in his chair, in a restless state, and learned that he had suffered considerable pain in the back part of his head and neck during the night. He was entirely incapacitated to rotate the head, which led to the suspicion of some injury to the articulations of the upper cervical vertebræ; but so great a degree of swelling existed about the neck, as to prevent an

efficient examination. There was no paralysis of any portion of the body, his pulse was about 90, and his general system but little disturbed. Warm fomentations were applied to the neck, and a mild cathartic administered. On the following day there was no particular change in his symptoms, but as there existed considerable nervous irritability, tinct. hyoseyami was prescribed as an anodyne, and fomentations of hops applied locally. On the third day leeches were applied to the neck, and after this the swelling so much subsided, that on the fifth day an irregularity was discovered to exist in the region of the axis and atlas, which had many of the features of a partial luxation of these vertebræ.

"At this time he began to walk about the room, having previously remained quiet on account of the pain he suffered on moving. He persisted in helping himself, and almost constantly supported his head with one hand applied to the occiput. He often remarked, if he could be relieved of the pain in his head and neck he should feel well. He began to relish his food, and the swelling nearly disappeared, at the end of a week, leaving a protuberance just below the base of the occiput, to the left of the central line of the spinal column, with a corresponding indentation. Notwithstanding strict orders to remain quietly at home, on the ninth day after the accident he rode out and in a day or two after returned as actively as ever to his former occupation of distributing milk throughout the city to his old customers. During the following four months no material change took place in his symptoms, although he constantly complained of pain in his head. For this period he did not omit a single day his round of duties as a milkman, which occupied him constantly and actively from five o'clock in the morning to nearly noon. On the 1st of November Prof. Watts examined him, and inclined to the opinion that there was a luxation of the upper cervical vertebræ.

"About the 1st of January, 1853, the pains, from which he had been almost constantly a sufferer, became more severe, and he was heard to complain that he could not live in his present condition: he remarked also that he had heard a snapping in his neck. After going his daily round on the 11th of January, he complained of feeling cold, and afterwards of a numbness in his limbs. In the evening he had a chill and complained of pain in his bowels. He passed a restless night and arose on the following morning about six o'clock; he was obliged to have assistance in dressing himself, and experienced a numbness of his left, and afterwards of his right side. He attempted to walk but could not without help, and it was observed that he dragged his feet. He sat down in a chair and almost instantly ex-

pired at eight o'clock A. M. on the 12th of January, precisely five months from the receipt of the injury.

"The autopsy was made thirty hours after death, by Dr. C. E. Isaacs, in presence of several medical gentlemen.

"Muscular development uncommonly fine; an unusual prominence observed in the region of the axis and atlas. On making an incision from the occiput along the spines of the cervical vertebræ, the parts were found to be very vascular. These vertebræ were removed *en masse*, and a careful examination instituted. The transverse, the odontoid (ligamenta moderatoria,) as also all the ligaments of this region, excepting the occipito-axoideum, were in a state of perfect integrity; this latter was partially destroyed. A considerable amount of coagulated blood was found effused between the fractured surfaces, some of it apparently recent, but much of it was thought to have occurred at the time of the accident, and afterwards to have prevented the union of the bones. The spinal cord exhibited no appearances of any lesion. The odontoid process was found completely fractured off, and its lower extremity inclining backwards toward the cord. Death finally took place, doubtless, from the displacement of the process, during some unfortunate movement of the head, by which pressure was made upon the cord. The destruction of the occipitoaxoid ligament, which would otherwise have protected the contents of the spinal cavity must have favored this result."

Backward Dislocation of the Astragalus.

[Prof. Alden March, of Albany, relates the following very interesting case, in "The Western Lancet." We give it a place in our Journal because of its exceedingly rare occurrence:]

"Isaac Quackenbush, of Herkimer, N. Y., aged about forty years, on the 24th of January 1853, while engaged in removing snow from the roof of his house, and elevated upon a ladder about ten feet whose base rested on the head of a hog'shead, was suddenly precipitated to the ground; and in his descent it was supposed his left foot struck with great force on the edge of the stoop, or step about eighteen inches from the ground.

"The first impression after the fall that Mr. Q. experienced, was that he had broken his leg.

"The fore part of the leg pointed a little downwards, having an unnatural fullness of the space below the internal malleolar

process, and not only a fullness, but even a projection between the *tendo achilles* and the back part of the tibia, so much so, as to cause the tendon to be prominent and tense. The knee joint was sub-flexed, and the patient experienced more or less of a *numb pain* in the affected part, and running up the limb; and at times he complained of a burning heat in the bottom of the foot, and at other times as though it were in a freezing mixture; and for most of two days it gave him great pain to extend the limb. The great toe was partially flexed, though susceptible of being extended by the hand without giving much pain.

"This symptom, and that of the limb being more free from pain and more comfortable, when partially flexed, can be explained, when we consider that the tendon of the *flexor longus policis pedis*, was carried out of its natural course upon the backward projecting astragalus, like a fiddle string elevated upon the bridge; and that the displaced bone acted in somewhat the same manner on the *tendo achilles*, when the limb was extended at the knee joint.

"Dr. Doolittle, of Herkimer, N. Y., a very intelligent physician, and of a great deal of experience, being the son of an aged physician of extensive practice, saw the patient twenty or thirty minutes after the occurrence of the accident, when the character of it was readily detected, and an attempt made to reduce it. But he soon discovered, by its position, and unyielded character, that all his efforts, and with all the means within his reach, would be unavailing.

"The patient, accompanied by a young physician, was placed in a railroad car, and brought to Albany, on the evening of the accident with a view to procure my aid in reducing the dislocation. I left town the early part of the same evening, before the arrival of the patient, to be absent for a few days.

"The services of my colleagues, Doctors James H. Armsby and James McNaughton, gentlemen in every respect competent by their anatomical knowledge and their experience and skill in the practice of surgery, were solicited; and with all the exertion in their power, both manual and mechanical, and continued for a long time, failed to effect a reduction.

"The patient returned to Herkimer where he was again placed under the care of Doctor Doolittle, who leeches the limb and put him on a thorough antiphlogistic course of treatment, which very much reduced the tumefaction and inflammation in the course of eight or ten days.

"The *State Medical Society* commenced its annual session in this city on Tuesday the first inst., at which were assembled something like thirty five or forty of the most respectable Physicians and Surgeons of the State.

"It was deemed quite advisable that the patient should have the benefit of the skill and wisdom of such an assemblage of medical men. Accordingly he was requested to visit Albany again, and although he did not arrive until after the adjournment of the Society, consequently the case was not examined by its members, yet I had an opportunity to examine it most thoroughly on the evening of Thursday, ten days after the accident.

"At this time, as I have already remarked, the inflammation and tumefaction had nearly subsided; and on examination I found the foot admitted of a moderate degree of flexion and extension, which was evidently between the *os calcis* and the astragalus, instead of between the *tibia* and astragalus.

"The limb did not appear to be much deformed; and my deliberate conviction was, that it would not be worth while to make any further attempt to reduce the displaced bone—and my prognosis, that the limb ultimately would become quite useful."

Contributions calculated to enlighten us in relation to the action of remedial agents and to the best method of administering them are always acceptable. We therefore cheerfully give place to the following communication of Dr. John T. Plummer, contained in the "Western Lancet," on

Liquor Ammoniacæ Acetatis.

"The following remarks may be of service, as items in the general stock of experience. They have been induced by the diversity of results, reported by different authors. Introduced more than a hundred years ago, its medicinal properties do not yet appear to be definitely settled, especially with regard to its action on the uterine functions; and the doses to be administered. Cloquet of Paris, Mazuyer of Strasburg, and Patin of France, report favorably of its use in dysmenorrhœa. The last named physician appears to have given 40 to 70 drops three or four times a day; and by these doses giddiness was produced. Christison (Dispensatory) says 'the dose should be half an ounce for an adult every two or three hours;' and makes no reference to its pathogenetic effects; but adds, 'many physicians err in prescribing it in small doses.' Patin advises against the incautious use of it, as it diminishes the quantity of the menstrual discharge. 'In large doses,' says Christison, 'it probably possesses the narcotic-irritant properties of other neutral ammoniacal salts.' In cases of profuse catamenia and uterine hemorrhages, Patin obtained the most remarkable results.

"For twenty years, I have employed this remedy with fre-

quent success in *amenorrhœa*, instead of 'profuse catamenia;' and in *dysmenorrhœa*. I have prepared it extemporaneously, with strongest cider vinegar; and with acetic acid of sp. gr. 1011. The first I have generally used in *amenorrhœa*, in half ounce doses every two hours, at the commencement of the menstrual efforts, indicated by headache, pains in the loins, &c.; and in *dysmenorrhœa*, in like manner. The preparation with acetic acid, I have used in the same affections, at intervals of 2 to 4 hours. From the use of the vinegar combination, I have never witnessed the slightest ill effects, except, perhaps, sometimes that degree of nausea which most medicines are apt to occasion. From the acetic acid preparation, I have observed variable effects in the doses named.

"A married female, about 30 years old, suffered from a multitude of nervous symptoms, in consequence of a prolapsed uterus, occasioned by too early rising after confinement with her first, and as yet only child. To restore menstruation, she had taken, from various physicians, hellebore, savin, and numerous other articles, with but little effect. (The prolapsus appears not to have been suspected by her medical attendants.) After replacing the uterus, I found the catamenial period painful, but without flow. I gave $\frac{3}{4}$ ss doses of the aqueous acetate of ammonia, of the strength above named, and the menses appeared without any pathogenetic action from the medicine. At the next period, the same means were used, after the patient had been allowed to pass several days beyond the expected time; and a more copious catamenia followed, than she had ever witnessed.

"To another patient about 20, married, but sterile, yet possessing general good health, I gave $\frac{3}{4}$ ss of the acetate, with the effect of vomiting, severe headache, and delirium. In a few hours the headache and delirium had ceased. Next day $\frac{3}{4}$ j. of the acetate were given with the like result, but in less degree.

"This article appears to have been proposed as a means of 'dispelling intoxication.' In the last named case, it evidently induced symptoms, closely imitating intoxication, if it was not intoxication itself.

"My experience fully accords with that of others, in respect to the febrifuge properties of this remedial agent. I have never been able to discover that it, in the slightest degree, excites the sanguiferous system. So far from it, it has in my hands, appeared to be a cooling sedative. I have found it to be especially serviceable, in the early and eruptive stages of measles, sometimes combined with camphorated water, but mostly alone.

"I do not wonder, however, that it has by some, been con-

sidered as a stimulant of the arterial function. Even as originally prepared by Minderer, it appears to have differed from our modern solution, in being more disagreeable in consequence of the accidental formation of an ammoniacal soap. But as I have seen it formed extemporaneously by some, claiming to be physicians and apothecaries, it would serve well, in case of emergency, to fill a smelling bottle.

"I was called in consultation, in the case of a child laboring under dysenteric fever; and prescribed, in part, the solution of acet. of ammonia. In a few hours afterward, I was called, in the absence of the family physician, on account of the occurrence of vomiting and increase of fever. Suspecting the cause, I examined the preparation, and found it so strongly ammoniacal, that, to satisfy my curiosity, I took it home and added to one part of the original preparation, *six parts* of acid. acet. dilut., before the alkali became neutralized. At another house, in a case of infantile pneumonitis, I discovered a like preparation to be the all-sufficient cause of a violent exacerbation of all the symptoms. I need not multiply examples; the cases given, will serve to show, not only an inexcusable recklessness in the preparation of the medicine; but also, the premises from which some are willing to draw their conclusions of the properties of remedial agents. To assure me that it was a neutral compound that he had been administering, the physician stated that he added the carbonate of ammonia until effervescence ceased. He may possibly have *heard* of litmus and termeric; but according to his own avowal, the cessation of effervescence was enough for him, without the aid of these vegetable tell-tales.

"Of late years, I prepared this solution exclusively with dilute acetic acid and the alkali. On account of the unsightly, turbid, brown liquid produced by the combination of ordinary vinegar with the ammonia; and the great variation in the strength of vinegar; dilute acetic acid of officinal specific gravity should always be used. When properly prepared, it requires much patience in attaining the point of neutralization, even with the aid of test papers. It may be readily kept for months, in a cool place, in well stopped vessels."

On the Exhibition of large doses of Quinine as an abortive remedy in Typhoid Continued Fever. By OTIS F. MANSON, M. D., of North Carolina.

"Principiis obsta; sero medicina paratur
Cum mala per longas convalere moras."

For the past nine years the writer of this paper has pursued a system of treatment unmentioned or condemned by the standard authorities in practical medicine, and, until very recently,

unknown on this continent, save in the extreme Southern States of our Union.

At the risk of being unread, or of being attacked by overwhelming numbers—the great calamities of authorship—we pen this article: we do so, we fervently believe, not from any *cacoëthes scribenda*, or desire for ephemeral notoriety, but from a conscientious desire to contribute to the cause of medical truth and medical progress. When we first came to this state, (1841), a fever, belonging to the periodical class, and exhibiting novel and peculiar phenomena, was then making its advent in the vicinity of our location. The usual treatment of the most skillful resident physicians most signally failed in arresting its progress, that treatment consisting in the employment of the usual *rolé* laid down in the books, viz: bleeding, general and topical, the mercurial impression, diaphoretics, opium, cathartics, and the various external revulsive and derivative irritants. General bleeding was soon abandoned by all, as it was quickly discovered to be hazardous in the extreme. Local bleeding only to a moderate extent was safely borne, the mercurial impression in the large majority of grave cases could not be timely effected; and as in these cases intermissions or decided remissions very rarely occurred, guided as we then were by the precepts of the schools, quinine was but rarely given, and then only in small doses and with a trembling hand. This fever presented symptoms of abnormal sanguineous determination to the cephalic, thoracic and abdominal organs, either separately or combined, with the periodical occurrence of cold stages, in which the symptoms of organic engorgement and diminished circulation in the extreme parts were made more manifest: in a few words, the disease was a remittent fever, with marked hyperæmia of some or all of the organs essential to life.

In an essay published in 1846, we gave the symptoms and a brief outline of our treatment in this fever, the main feature of which consisted in the liberal employment of quinine without any hindrance to its use from the presence of local congestion, fever or inflammation. Not only was this treatment adopted in this but in every form of idiopathic fever, whether remittent or continued, which came under our care early in the attack, with the most satisfactory results. Since the year 1843, our general rule has been to endeavor to arrest these fevers during the first twenty-four hours the patient is seen—this rule subject to restrictions hereinafter noticed.

When we entered upon this practice we were opposed by nearly every member of our professional acquaintance, and by a host of others. Pictures of consequent insanity, apoplexy, phrenitis, paralysis and loss of the senses were held up to the

people to warn them against the innovation. In the midst of this clamor we visited Richmond for the purpose of publishing our views. We there met our ancient tutors and confrères: from all we received a kind and patient hearing, but with encouragement from none. Some heard us in silence: many attempted to dissuade us from persisting in what they considered "most hazardous practice." These obstacles only stimulated our ardor; and we may have been intemperate in our zeal, but not, we hope, to such a degree as to justify the opinion of our friends, who hinted that we possessed the necessary qualifications to entitle us to an abode in those desirable asylums of Williamsburg and Staunton. But now (we hope we may be forgiven the taunt, the provocation was severe) they talk very learnedly of the abortive and sedative action of quinine, and have forgotten the name of him from the lips of whom they first heard it proclaimed. They have "stolen our thunder," and we shall not forget them.

Nothing daunted, however, by all these obstacles, we pursued our way. Every case of idiopathic fever (chiefly remittent and typhoid) in which the patient was seen near the accession of the disease, was immediately subjected to this treatment with the most signal success. The people, to whom the appeal was made—competent to judge of results, if not of means—pronounced in our favor beyond our most sanguine expectations. In their train many of our opponents followed; but a few—we are glad, for the honor of the profession, they were but few—stoutly maintained "that the practice *was nothing new to them—they had been pursuing it all along:*" and one oblivious old fogey, not to be outdone, dated his employment of the alkaloid at a period before its discovery.

We mention these facts, in order that the date of our first employment of quinine in continued fever may be proven: the practice is original with us, so far as we have any knowledge. Others may have pursued it before; and if so, we will cheerfully yield the merit of priority to those who can establish their claims thereto.

Now we desire to be so plainly understood that misconception must be willful, viz: that we lay not a shadow of claim to the introduction of the use of quinine as a sedative febrifuge. It was used in this mode before we commenced our professional career. But to this we do lay claim: that we were the first to promulgate its applicability to the endemic fevers of Virginia and North Carolina, and the first anywhere, as far as our information extends, who successfully employed it as an abortive remedy in continued fever—concerning which we now proceed more particularly to speak.

For many years past, the usual varieties of periodical fever have been gradually yielding place to that mysterious, subtle, hitherto intractable, and often fatal form of continued fever, now almost universally termed typhoid. There is no need here of a description of the physiognomy or pathology of this malady. The distinguished Louis, the able and indefatigable Chomel, together with a host of others, have nothing left in these respects for the pen or scalpel to delineate, but we here lay our humble contribution before the Master Mind of him who is yet to indite the *Unwritten Chapter of its Treatment*.

The practice pursued by us in this disease is in the highest degree rational, founded as it is upon the nature of the disease, so far as it is known, and on the effects of remedies proven by repeated and numberless experiments.

Called to a patient in the first stage of the disease, our efforts are first directed to combat the symptoms of morbid action, which, for convenience, we will term the palliative treatment; and secondly, to arrest, by specific means, the farther progress of the malady, the abortive or curative treatment.

First: *Palliative Treatment*.—As, in other forms of fever, the symptoms presented will differ accordingly as different vital organs are implicated, the general rules of treatment will necessarily be adopted. Our favorite plan, however, is to commence with the administration of an emetic of ipecacuanha, from which we have seen great benefit derived, not only from the removal of the contents of the stomach, for we have seen headach, lumbar pains, and, in a few cases, delirium, disappear under its influence. If the headache be intense or annoying, cups are applied to the mastoidal regions, whence blood flows profusely from clean scarifications; if delirium is present and the pulse will bear it, blood is taken from the arm to the extent of from 10 to 16 ounces, and ice freely applied. At 8 or 9 o'clock at night a cathartic of calomel and rhubarb, in quantities of 8 or 10 grains of each, is administered. If diarrhœa is present, a full dose of opium, of $1\frac{1}{2}$ or 2 grs., is combined with the cathartic. Should there, however, be any fear of the rhubarb not being tolerated, the calomel alone is given, in a dose of 15 to 20 grs.

In the interval between the administration of the purgative and 2 o'clock next morning every means are used to relieve the threatened organs. Cups are continued to the occiput or epigastrium, as may be indicated; cold applications to the head assiduously applied, and all proper means used to prepare the patient for the

Curative or Abortive Treatment. At 2 o'clock in the morning from 20 to 30 grs. of sulphate of quinine in pills, or diffused

in an ounce of cold water, are given at once to the patient, the dose being regulated according to the intensity of the fever, *the quantity being increased pari passu with the degree of vascular excitement.* Four hours are now suffered to elapse; the patient is allowed to drink moderately of diluents, of which, at this moment, we prefer sweetened milk and water pleasantly warm, or the effervescing draught. The condition of the patient at the end of four hours, viz: at 6 o'clock, in a vast majority of cases, will be sensibly improved; his head symptoms will be partially or entirely removed; his pulse will be diminished in frequency and force, and have acquired a more natural degree of fullness and softness; his skin will have become relaxed and perspirable, and in many instances a copious sweat breaks out, and, as the patient has often expressed himself to us, "He feels like a new man; his head is easy for the first time since he has been sick!" At 6 o'clock, (unless there be marked symptoms of quininism—deafness, roaring, buzzing, hissing, ringing of the ears—present,) if the pulse has not declined very nearly to the normal standard, it is our rule to repeat the quinine in doses of 10 to 20 grs. During the night the cold applications and local bleeding are continued, if indicated, but it is very rarely we have to repeat the cupping after the quinine is given. Ice in most cases will suffice.

The quinine is now suspended, if fever still continues. Calomel is administered in 1 or 2 gr. doses every one or two hours, until the bowels are sufficiently moved. If diarrhœa is present, the hyd. cum creta and Dover's powders are preferable. If fever still continues, at 2 o'clock next morning the quinine is again given, the dose to be lessened accordingly as the fever has declined, and the alterative continued. Attention is now directed to the symptoms present—for which no specific rules can be given, for here the general principles of treatment are brought into requisition—the patient is carefully conducted towards convalescence, and the wants of nature supplied. As the fever disappears the diet must be increased, and wine and tonics, of which we prefer gentian, will be useful, and often indispensable.

If, however, the fever persists, as it sometimes will, but always in a mild and modified form, the oil of turpentine, a remedy long used, but more particularly called to the attention of the profession by Prof. Wood, now comes admirably into use. From 20 to 30 drops in a wineglassful of sweet milk, or in emulsion, may be given every four or five hours. This, however, will sometimes give rise to unpleasant and even dangerous symptoms, viz: a peculiar intoxication, gastrodynia, hæmaturia and dysuria, when of course it will be discontinued,

After this treatment is pursued, it is but seldom that we give any aperient medicine: officious meddling in this respect, we have no doubt, has had a principal share in the mortality of typhoid fever. Mucilaginous enemata are daily used, and a simple remedy, that will often succeed when repeated enemata fail, is a suppository of stewed molasses.

Observations on the foregoing Treatment.

The practitioner must not be deterred from the use of the mercurial cathartic in the first instance, though diarrhœa be present; for if the opium does not control its action, the quinine, often administered, certainly will, and in such cases we have never seen hypercatharsis induced—QUININE POSSESSING THE PROPERTY OF PREVENTING AND REMOVING LOCAL DETERMINATION, WHETHER INDUCED BY THE ACTION OF MEDICINAL AGENTS, OR OCCURRING AS A PHENOMENON OF DISEASE!!

Neither should nausea or vomiting be considered as indications adverse to the employment of this remedy: on the contrary, if the stomach will bear its mechanical presence for a short time, it will allay these troublesome symptoms when all other means fail. IT PRODUCES THESE EFFECTS BY ITS PROPERTY OF ALLAYING IRRITABILITY AND SPASMODIC ACTION.

Nor should delirium be considered a contra-indication. After using the means before directed, with all the activity and energy the case demands and the patient will safely bear, the delirium will still continue in some instances. In such cases we have repeatedly seen every symptom of cerebral implication removed as if by magic, under the influence of this incomparable sedative. It is our rule to try the usual means *first*; we believe them to be necessary and efficient adjuvants to the salutary influence of the abortive: they prepare the way and assist the remedy in producing its complete and peculiar influence: the exhibition of quinine does not interfere in any degree with the demands of the rational indications, *but it is given for the purpose of assisting their action, and rendering their effects complete and permanent.*

The usual treatment of the best authors of the present day is merely *expectant* and *palliative*; their whole endeavor is to *ameliorate* the condition of vital organs, and to remove the *secondary phenomena*: on the other hand, *quinine strikes triumphantly at the roots of the malady and removes the cause.*

But the reason may be demanded of us, "Why we do not use this remedy in the latter as well as the primary stage?" We reply—Because our experience is against its use. Believing, as we do, that the symptoms of the prodromic and invasive stages are altogether referable to an altered and perverted ac-

tion of the nervous system, and knowing that quinine possesses the property of counteracting the morbid action then present we unhesitatingly and boldly prescribe it; but soon another class of symptoms present themselves—symptoms referable to and indicative of organic change—those lesions so minutely and accurately portrayed by the most celebrated pathologists of the age, and which we believe that quinine has no power to remove but may often aggravate. These cases must be treated according to the rational indications. Active interference, our experience has proven, is no longer admissible; with watchful eye and careful hand we must discern and assist the operations of nature, endeavoring to mitigate and remove the symptoms of distress, discomfort and danger, and guarding closely the approaches through which the destroyer may enter at any moment.—[*Stethoscope*.

Collodion in the Treatment of Buboes. By JNO. HERBERT CLAIBORNE, M. D., of Petersburg, Va.

The obstinacy that buboes sometimes assume, pertinaciously refusing either to suppurate or to be discussed, often proves exceedingly annoying to the patient, if it does not indeed occasionally bring reproach upon the efficacy of medical treatment. Few physicians, I imagine, who have had the treatment of many of these troublesome affections, have not seen the *patience* of their *patients* grow weary under their unavailing efforts to scatter a chronic bubo in the groin; and fortunate may they consider themselves if they have not been compelled to listen to imprecations upon their art for its worthlessness and inefficacy. Since I have begun the use of collodion in such cases, I am glad to remember that I have been almost entirely exempt from listening to tirades from the subjects of this malady at least.

I use the collodion solely for the purpose of discussing or scattering the bubo, and in my hands it has proved incomparably superior to any other discutient. It operates by pressure, I believe, thereby lessening the capillary congestion of the parts, and stimulating the absorbents to remove whatever products may have been thrown out by inflammation about or in the affected glands.

I have used it in the treatment of buboes of the groin only. When a patient with a bubo presents himself, if there be not much acute local inflammation, no matter formed, and no constitutional reaction, I immediately shave off the capilli from and around the tumor, and with a camel's hair pencil apply

the collodion upon the place, allowing one layer to dry before the application of another, until I form a scab or membrane of such thickness as I think will exert the proper compression. Sometimes the application has to be renewed in the course of twenty-four or forty-eight hours. If put on too thickly, it will give a great deal of pain; and I have seen it contract so tightly upon the tumor, that the epidermis would be split in fissures around the borders of the application. This will serve to give some idea of the force of the compression which it will exert. After the application of the collodion, it is better that the patient remain quiet for a day or two, take an aperient, and restrict his diet, though I have succeeded in discussing a chronic bubo when not an hour has been lost from his usual avocations.

If there be much inflammation about the tumor, I usually endeavor to subdue this by general and local depletion, saturnine applications, rest, &c., before applying the collodion; and if the bubo be of venereal origin, institute, of course, the specific treatment for the existing disease.

I have succeeded several times in discussing the bubo in this manner, even when fluctuation discovered the presence of matter. If it be of syphilitic origin, however, I doubt very much the propriety of this. The dissipation of the tumor, in this instance, can only occur by a reabsorption of the virus of the pox into the blood, whereas the sole object of the treatment of syphilis is to eliminate this poison. In such cases, therefore, I prefer puncturing the bubo at the earliest indication of matter, and encouraging suppuration by poultices.

I have several cases in my note-book which strikingly exemplify the efficacy of collodion in the discussion of buboës. I suppose it might be usefully employed in chronic enlargements of glands in other portions of the body. I do not know that I can lay claim to the honor of originating this application of collodion, if there be honor in it; but I do not remember to have seen it spoken of by any medical author. If any one shall rise up and claim it, I am ready to yield unto him "the things which are his." The principle of scattering buboës by the application of pressure was first remarked by me in following one of the surgical officers in the wards of the Pennsylvania hospital some few years ago. In that institution they were in the habit of making pressure by fastening on plates of sheet lead. In the inconvenience of applying these, and in some extremely obstinate and annoying cases, which greatly perplexed me, I thought of substituting the collodion. The success which attended it exceeded my expectations.

Perhaps some of my professional brethren have used it—I would like to know with what success.—[*Ibid.*

The Alum Springs of Virginia.

Having for some time heard of and entertained a favorable opinion of the Alum Springs of Virginia in the treatment of certain affections, we are happy to have it in our power to lay before our readers some account of them. We derive the following facts from an article by Dr. Thomas Pollard, published in the "Stethoscope."

The *Rockbridge Alum Springs* are situated seventeen miles from Lexington. The water of Spring No. 2, was analyzed by Dr. Hays, of Boston, one gallon of which was found to contain—

"Sulphate of potash,	-	-	-	1.765
" " lime,	-	-	-	3.263
" " magnesia,	-	-	-	1.763
Protoxide iron,	-	-	-	4.863
Alumina,	-	-	-	17.905
Crenate of ammonia,	-	-	-	0.700
Chloride sodium,	-	-	-	1.008
Silicic acid,	-	-	-	2.840
Free sulphuric acid,	-	-	-	15.224
Free carbonic acid,	-	-	-	7.536

				56.867
Pure water	-	-	-	58315.133

58372.000

"In a conversation with the late proprietors* of this property, they informed me that they did not consider the analysis as made from a fair sample of the water; that the water was probably weak at the time, and No. 2, in their estimation, not so good as the other springs, and that they believed them to contain iodine. Dr. P. H. Christian, one of the proprietors at the time referred to, informed me that Prof. Rogers had made an analysis of the water, which had never been published, and that he understood that Prof. R. found it to contain iodine.

"*Therapeutic Application.*—The diseases in which this water has most reputation, are dyspepsia, scrofula, chronic diarrhœa and skin diseases. The time has gone by for skepticism in relation to the medicinal virtues of mineral waters. I can only give a general outline of the cases to which I shall allude, as some of them did not come directly under my observation

* It had at this time been purchased by Mr. John W. Frazier of the Bath Alum, though he had not obtained possession.

and others I have no permission to make a formal report of. That some undoubted cases of scrofula have been relieved by the water of which we are speaking, does not admit of a doubt. Several such cases I have seen, and of others heard from reliable authority. Dr. Christian, who has had considerable opportunity of observing its effects, remarked to me that he believed every case of scrofula, where the constitution was not utterly broken down, might, if the water were long enough continued, be cured. We can scarcely hope it, however, to be a specific in this disease. Two cases which at the springs came under my notice, where after two months' use in one case and one month in the other, no perceptible benefit had been perceived except most decided improvement in the general health. Probably the foundation of cure was thus laid, and another summer at the springs, with use of the water in the intermediate time, may effect the desired object. I allude to these cases as being the only unfavorable ones I saw or heard of among those suffering from scrofula. One case of most marked improvement I may allude to as being no doubt familiar to some of the faculty of this city: Mrs. S., of Rocketts, suffering from a large scrofulous sore on the side of the neck. She had used the water four or five weeks, and from her representation the amendment had been rapid. While I saw her, which was during the space of two weeks, the sore was every day diminishing in size and assuming a more healthy appearance, and there seemed every prospect of perfect cure. Closely allied to the disease of which we are speaking, were two cases reported to me as having been cured by the persons who were the subjects of them, and as far as an opinion may be formed from two cases, of course imperfectly described, the use of this water in incipient phthisis would seem very encouraging. One case, a servant who waited on the hotel, had been sent by his master to use the water for hæmoptysis, of which he had had several attacks, and from which his general health had suffered much. The hæmoptysis was attended with pain in the side, and the lung of that side had been pronounced diseased by his attending physician. When I saw him his general health was good, and as far as I could judge, he seemed a well man. The other, a white man, resident in the neighborhood, told me he had had several attacks of hæmoptysis, with constant pain in the side, while residing in Georgia. Superadded to this was a running scrofulous sore about the neck. The sore had healed; no hæmoptysis had occurred for many months, and to all appearances he was well.

"In *dyspepsia* this water seems to act with excellent effect. Almost every case of this disease which has come under my

observation, in which the water has been used both at the springs and after transportation, has been much benefitted. In chronic diarrhœa, and in cases of dyspepsia attended with irritability of the bowels, its action is usually very advantageous. In such cases the quantity used, however, should not be large, and when it excites any irritation, as it sometimes does, should, for a day or two, be suspended.

"In *skin diseases* its virtues are said to be very valuable. I had no opportunity of judging of its effects in this class of diseases, except in one case of chronic erysipelas of the leg, which was rapidly improving.

"In one case of *chronic sore throat*, which came under my notice, the effect of the water was most salutary. The subject of it, a gentleman from Philadelphia, had suffered from ulceration about the fauces and tonsils for several months. Caustics of various kinds had been used by a medical gentleman of eminence, and no doubt with skill and judgment. He had visited the White Sulphur Springs without benefit, and came to the Rockbridge Alum still suffering a good deal, particularly in swallowing. After spending five days here, at which time I saw him, the improvement had been most marked—difficulty and pain in deglutition had ceased; the ulcers, of which there had been several, were healing, and in two weeks his throat, to use his expression, 'felt well.' At this time the ulcers had all healed, with the exception of a long fissured one, which was cicatrizing.

"*Modus operandi.*—From the existence of sulph. alumina in this water, we might expect that constipation would be produced by its use. Such, however, is not the fact. The bowels seem regulated. If constipation exists, it is removed. If chronic diarrhœa, it is generally cured. This may appear contradictory, but is not incompatible with the known action of other remedies. In some cases, when long continued, the water seems to irritate the bowels—the effect probably of the iron. The presence of the sulph. alumina furnishes a valuable adjuvant in preventing, in general, this irritant effect of the iron, of which we have frequently to complain in the use of our medicinal preparations of this metal.

"*The quantity to be used.*—This, of course, will depend upon the strength, which, as I have mentioned, varies considerably. When the water is at its strongest, a tumblerful will frequently produce emesis, owing, I suppose, to the existence of the iron and alumina. As a general rule, six glassesful during the day is enough for most individuals: this should not be taken when the stomach is full. Too much of this water is frequently drunk, overloading and oppressing the stomach, and this remark we believe will apply generally to the use of mineral waters."

The Bath Alum Springs are seventeen miles from the Rock-spring—and, on Analysis by the same chemist, yield from one gallon :

“Free sulphuric acid,	-	-	-	7.877
Carbonic acid,	-	-	-	3.846
Sulphate of potash,	-	-	-	.258
Magnesia,	-	-	-	1.282
Lime.	-	-	-	2.539
Protoxide of iron,	-	-	-	21.776
Alumina,	-	-	-	12.293
Crenate of ammonia,	-	-	-	1.776
Silicate of soda,	-	-	-	3.150

				54.798
Pure water,	-	-	-	58317.202

58372.000

“Some of the springs are stronger in iron than No. 3, and no doubt constitute the strongest chalybeate waters in the State. The water of No. 3 is most generally used. The iron and alumina are no doubt in the form of the sulphates, both in this water and in that of the Rockbridge Alum springs. It is so given by Prof. Rogers, as reported in Dr. Burke’s book, p. 295.

“*Therapeutic application.*—The diseases in which this water is used are very similar to those in which the Rockbridge Alum springs are applied. Containing more iron, they are more strongly tonic, and are more adapted to some forms of dyspepsia, chlorosis and affections generally requiring an active tonic impression. Dr. Burke, in his work on the Virginia Springs, quotes a letter from Dr. Strother of the Warm springs, speaking highly of their virtues, and recommending them as valuable in chronic diarrhœa, chronic thrush, dyspepsia, nervous debility and some forms of neuralgia, uterine disorders, particularly menorrhagia and fluor albus. The water of these springs has not had a fair trial, a prejudice existing against it from the fact that the public are apprised, from their strong metallic taste, that they contain *copperas*, which, not knowing that it is one of our best tonics, (sulph. iron,) they mistakenly believe to be poisonous.

“This water being very strong, a tumblerful (a quantity which many persons ignorantly use) produces emesis. This confirms the impression of the ‘poisonous copperas,’ and they cannot be persuaded to make further trial of it. The consequence is, the water has been used to a limited extent. My own impression of its value is, that when these springs become

better known, and the water more properly used, it will rank as one of the most valuable of our mineral waters. Though these springs have been in use only three or four years, I have been informed, through reliable sources, of the cure of three cases of scrofula. Two cases of chronic diarrhœa which came under my notice, were much benefited. Another case improved but slightly, but I thought it was because the gentleman used the water in too large quantities.

“Quantity to be used.”—Four tumblersful are generally enough to be taken during the day. In some cases, and particularly in chronic diarrhœa, one wineglassful, repeated five or six times a day, is as much as should be used.”

On Reflex Phenomena. By M. CL. BERNARD.

Reflex Phenomena are of two kinds: the one having for object the accomplishment of the functions of organic life, the other those of animal life. M. Bernard has by his experiments endeavored to show that both of these phenomena are originally identical, and that the great sympathetic nerve plays a prominent part in their production. He first examined the reflex phenomena of organic life.

Two kinds of nerves are requisite for the production of these phenomena: the first transmits the impression to the nervous centres, the second to the viscera. With one order of these nervous filaments is always connected a ganglion of the great sympathetic; example: the lingual nerve transmits the impression of taste to the nervous centres, a special nerve then conveys a corresponding excitation to the submaxillary gland; on one of these nerves is situated a ganglion of the sympathetic, the submaxillary ganglion.—Another example: the optic nerve and the *motores oculorum*, the first transmitting the impression, the second the reflex excitation, are separated by the ophthalmic ganglion. The pneumogastric nerve may be considered in the same light in its relations with the liver, lungs, and spinal marrow; the last is here a conductor of reflex excitation, and the ganglia of the solar and cardiac plexuses play the same part as that which has been assigned in the preceding examples to those of the submaxillary and ophthalmic regions.

Excite the lingual nerve, and the secretion of saliva will be increased; cut the submaxillary ganglion, the nerve which connects it with the nervous centre, and the excitation of the lingual nerve will no longer produce this phenomenon. The experiments of Herbert Mayo prove that in the movements of the pupil, the results are identical as regards the optic nerve and the *motores oculorum*.

The pneumogastric nerve is the seat of analogous phenomena; as example: this nerve transmits to the nervous centre the impression made upon the lungs by atmospheric air, their habitual excitant; this impression, by means of the spinal marrow and sympathetic, immediately causes the production of sugar in the liver, which secretion, the result of reflex action, corresponds to the excitation of which the lungs are the seat, just as the secretion of saliva is the result of an irritation of the lingual nerve. Excite the lingual nerve and the secretion of saliva will be exaggerated; excite the lungs by means of chloroform or chlorine and the secretion of sugar in the liver will be increased. From these considerations, M. Bernard is led to attribute the exaggerated production of sugar in the liver, caused by irritation of the pneumogastric nerve, to the reflex phenomena of organic life.

After having called the attention of the society to the important function which is performed by the great sympathetic in the production of visceral reflex phenomena, M. Bernard asks if the reflex phenomena of the limbs have not, even in this last point of view, a great analogy with the preceding; it seems at the first glance that such is not the case, for in the experiments of Marshal Hall, the ganglia do not appear to have any relation with the production of reflex movements. The following experiment, however, imagined by M. Bernard, seems to show that the intervertebral ganglia cannot be wounded without a corresponding cessation of reflex movement in the limbs. By a singular disposition the roots of the nerves of the anterior members in the frog, are seen, on dividing successively the muscles of the shoulder, to the outside of the medullary canal; the posterior provided with their proper ganglia. If these be examined attentively it will be observed that *all* the nervous filaments of the posterior root, do not traverse the ganglia, and that a certain number of them may be easily isolated by the point of a needle. From this anatomical arrangement it results that the ganglia may be destroyed and still a portion of the posterior root remain intact. Now if the section of the ganglia in the living frog be completed, reflex action ceases in the corresponding member; the animal no longer withdraws its foot on its being irritated; that sensibility, however, remains perfect, is proven by the fact that the animal manifests signs of pain by movements of the *whole* body. This experiment shows then, that the intervertebral ganglia are necessary to the production of reflex movements in the limbs. Lead by analogy, M. Bernard concludes, in assimilating the intervertebral ganglia with those of the great sympathetic, that they are both necessary in the production of the two order of reflex phenomena admitted by him.—[*Gazette Medicale. N. O. Med. Register.*

On Hemorrhage. On a new Styptic. By M. PAGLIARI.

This styptic forms a subject of a long communication to the Académie des Sciences by C. Sedillot. Its composition is as follows: eight ounces of balsam of benzoin, one pound of sulphate of alumina and potass, and ten pounds of common water are boiled for six hours in a glazed earthen vessel, care being taken to add fresh quantities of *boiling* water, so as to supply the loss in evaporation, and to stir continually. At the end of this time the supernatant liquid is separated from the undissolved benzoin, which has lost its odor and inflammability, and filtered and preserved in glass bottles. The liquid thus obtained is limpid, and of the color of champagne; its taste slightly styptic, and its odor pleasant and aromatic, and when evaporated it leaves a transparent deposit on the sides of the vessel.

The styptic properties of this preparation seem very remarkable. A single drop immediately coagulates a cupping-glassful of blood, and a larger quantity (equal proportions) converts the blood into a firm and resisting solid. Applied to a wound the hemorrhage ceases almost immediately, in consequence, as it would seem, of the formation of such clots upon all the orifices of the bleeding vessels. The application also produces no irritation and inconvenience, nor does it interfere in any way with the process of cicatrisation.

The cases given are well authenticated, and the results such as to leave no doubt as to the valuable styptic properties of the preparation. There are cases of obstinate primary and secondary hemorrhages, after surgical operations; one from a severe cut in the finger; some from the extraction of a tooth. In these cases a piece of lint was soaked in the styptic and bound upon the wound.—[*Ib.*]

The Laws regulating the Bodily Temperature and the Frequency of the Pulse. By R. LICHTENFELS and R. FROHLICH.

The authors have made a most careful series of experiments on themselves. Each experimenter is twenty-two years of age; the pulse of one of them is normally 71 per minute, that of the other 88; the normal temperature of each is 98.434. During the course of the experiments, they rose shortly before 7 A. M., took coffee between 7 and 8, had dinner at 2, and evening-coffee between 7 and 8.

1. *Daily rate of pulse and temperature.*—The influence of the period of the day, *per se*, was very trifling, but both pulse and temperature were greatly affected by food. Before the morning-coffee, the pulse was lowest; by the end of the first

hour after coffee it rose, on an average of many observations, nearly 8 beats per minute; it was slightly less rapid at the end of the next hour; at the end of the third hour it was only 3.3 beats; and at the end of the fourth, 2.77 beats over the original number. The pulse did not sink to the number noted before coffee, till six hours had elapsed. The mid-day meal raised the pulse again, and this occurred apparently sooner after protein than after starchy food, but to a less extent. After the evening-coffee, the pulse, which had fallen, again rose, but to a less extent, and its declension occurred more rapidly.

The temperature of the body was affected in a similar way by food, but the augmentation occurred later than the rising of the pulse; so that the temperature was often at its maximum when the pulse had fallen considerably towards the point from which it had risen. The average amount of increase is about $\frac{1}{3}$ of Fah. The greatest average range of the thermometer in the course of the day (between 7 A. M. and 10 P. M.) was rather less than a degree of Fah.

2. *Influence of customary liquid.*—The experiments were performed in the afternoon; each lasted 100 minutes, and the greatest tranquility of body was preserved. After *beer*, the pulse sank 6 or 7 beats in from 10 to 15 minutes; in 30 minutes it regained its former frequency; much before this time, the subjective feelings of slight incipient intoxication were felt. In about two hours, the pulse was heightened nearly double as much as it had been depressed. The temperature, after the use of beer, fell about one-third of a degree of Fah. After *wine*, the pulse at first fell in the same way, and then rose greatly; the temperature fell about half a degree of Fah. The same occurred with alcohol, but afterwards the temperature rose about a quarter or half a degree of Fah. Cold water lessened, at first, the number of the pulse, and lowered the temperature. In 15 minutes both returned to their former amount. Coffee, as already said, raised the pulse, but more in the morning than in the evening.

3. *Influence of fasting.*—Fasting for from 20 to 21 hours lowered both pulse and temperature. At the end, the pulse was from 12 to 16 per minute; the temperature as much as 1.8° Fah., under the normal. The curious observation (made also by Davy and Gierse) was noted, that at the period of customary meal-times both pulse and temperature slightly rose.

4. *Influence of muscular movements.*—Various experiments were tried with different kinds of movements. 1. A ten pound weight was allowed to hang from the arm for five minutes, the body being tranquil; the pulse first fell in frequency, then rose; its greatest frequency was after the termination of the experi-

ment. When the weight was on the left arm, the rise was nearly double that which occurred when it was on the right arm. 2. A weight of one pound was held out horizontally; the pulse rose and fell remarkably several times. 3. A weight of two pounds was rapidly swung round and round with one one arm, while the other was placed on a table, that the pulse might be counted. This exercise produced the greatest effect on the pulse, raising it sometimes from 30 to 50 beats. 4. Long-continued moderate exercise, carried on to fatigue, raised the pulse greatly for some considerable time, but never produced the enormous rise noted in the previous kind (3) of muscular exertion.

5. *Influence of narcotic poisons.*—*Belladonna* and *atropine* at first diminished the frequency of the pulse (16 to 20 beats), but after a variable time (50 to 117 minutes) the pulse again rose (12 to 30 beats). The smaller doses produced greater primary sinking than the large, but required much longer time to do so; on the contrary, the larger doses produced much greater secondary rising; that is to say, the maximum sinking point is inversely, and the maximum rising point is directly, proportioned to the amount of the drug. It might be said that small doses depress, larger excite, the pulse. The temperature was diminished in all cases. *Opium*, especially in small doses, caused rising of the pulse, but afterwards there was great sinking, and the temperature diminished. The *Cannabis Indica* produced many periods of rising and falling; the temperature rose for about four hours, and to as great a degree as 7° or 1° Fah. *Chloroform* and *ether*, if not pushed to too deep a narcotism, raised both temperature and pulse.—[*British and For. Medico-Chirurgical Review*.

Infusion of the leaves of the Ash-tree (Fraxinus polygamie) in the treatment of Gout and Rheumatism. By Drs. POUGET and PEYRAUD.

The most useful acquisitions in materia medica are due generally to chance, or, rather, to a protective Providence over human health; *e. g.*, the antiperiodic properties of cinchona.

In 1842, Dr. Peyraud experienced his first attack of gout, for which he employed the usual remedies. The attack lasted twenty-five days. During the three following years the disease increased both in frequency and violence, and the patient went through the several forms of treatment without finding relief. He heard, by accident, that the infusion of the ash-leaves had long been considered a specific in the department of Dordogne, and that the peasantry of the district used it to "chase away

their pains." Dr. Peyraud found it upon trial so useful that he had but one attack of the gout from 1845 to 1849.

M. B., a merchant of Bordeaux, was seized with an attack of gout, for the first time, in the right foot. He was ordered infusion of ash-leaves; upon the second day he was able to come down stairs and to leave his house. Ever since, he has had recourse to the same remedy with good effect upon experiencing premonitory symptoms, such as headache, loss of appetite, etc. The same success followed its administration in cases of acute articular rheumatism. A house-painter, aged 23, had, upon several occasions, suffered from acute articular rheumatism, accompanied by endocarditis, which prevented his following his occupation for four months or more. December 25, 1849, he had pain in the articulations of the feet, knees, and wrists, with swelling, accompanied by difficulty of respiration and inability to lie down. December 30, he was so weak that it was thought better, before bleeding him, to try the infusion of ash-leaves. In about twelve hours, and after the third dose, the patient was able to breathe easier, and there were fewer periodical pains. On the fourth day, the articular pains had diminished in intensity, the respiration was freer, and the appetite had returned. January 22d.—He was able to resume work. The authors now administer it in the form of a fine powder in preference to the infusion. Other cases are related in the Essay.—[*London Med. Times*.

On Adulteration of Sulphate of Quinine. By Dr. MOLL.

The excessive price of the true cinchona, the calisaya of Bolivia, has led to the substitution of many inferior kinds, chiefly remarkable for their containing large proportions of *quinidine*. In consequence of their lower price they have obtained admission to the quinine manufactories in large quantities, and much of the sulphate now produced is depreciated by the addition of *quinidine*. This substance differs from the sulphate of quinine by its greater specific gravity and less flocculent crystallization, and it is much more soluble than it is in water and alcohol. The addition of both *cinchonine* and *quinidine* may be detected by means of ether; for while *cinchonine* is almost insoluble in this substance, *quinidine* is so in a far less degree than is quinine, inasmuch as sixty drops of ether and twenty of ammonia will dissolve ten grains of quinine and only one grain of *quinidine*. On the addition of these quantities of sulphuric ether and liq. ammonia, to ten grains of quinine, with ten drops of dilute sulphuric acid, and fifteen of water, all will

remain dissolved, unless cinchonine, or more than ten per cent. of quinidine be present, the mechanical impurities only appearing at the surface. If 10 per cent. of quinidine be present in the ethereal solution, it will soon crystallize on the surface of the ether. Traces of this substance can be yet more certainly discovered if ether saturated with quinidine be employed, when all that exists in the suspected salt will remain insoluble. If the powder contain cinchonine, or more than 10 per cent. quinidine, it will remain undissolved at the line of demarkation of the two fluids. If it be quinidine, it is soluble in additional ether, which cinchonine is not.

To establish the purity of quinine, we must also assure ourselves of the absence of *inorganic* substances, by calcination in platina, or by a solution of the salt in alcohol. Sulphate and carbonate of lime, magnesia, &c., remain undissolved, while boracic acid, though soluble, betrays itself by its blue flame on conflagration. The absence of *organic* substances, as salicine, sugar, starch, stearic acid, is known by the colorless solution which takes place in concentrated sulphuric acid. The presence of *ammoniacal salts* is revealed by the odor which ensues on the addition of caustic alkali.—[*Brit. and For. Med. Chir. Rev.*, from *Rev. Medico-Chirurg.*

On the Reproduction of Nervous Substance, and on the Structure and Functions of the Spinal Ganglia. By Dr. A. WALLER.

Dr. Waller, after having made many experiments on different animals, principally warm blooded ones, of an early age, and frogs, considers himself entitled to the conclusion: "That the old fibres of a divided nerve never gain anew their original structure and function, and that the reproduction of nervous substance does not take place merely in the cicatrix itself, but also downwards into the terminating ramifications. The old fibres gradually waste, and after a month or later, new fibres are formed, which are pale and transparent, possess no double contour, present a very unequal diameter, being on the one place very thin, on the other, varicose, like the fibres of the spinal marrow. In the peripheral part of the glosso-pharyngeal nerve of a frog, three months after the section, their size was only about one-sixth to one-third of the original fibres; they resembled, therefore, much more the ramifications of the nerve in very young frogs. In the central part of the cut nerve the fibres remain unaltered. Concentrated acetic acid dissolves the membrane of the newly-formed fibres, leaving fusiform nuclei; the membranes of the original fibres are com-

pletely dissolved, no nuclei being left. The reproduction of fibres, and the return of function, proceed in the same proportion. Of great importance are Dr. Waller's experiments for the understanding of the *structure and function of the ganglia*. While, as he has previously shown, all motor nerves, separated from their cerebro-spinal centre, become entirely changed in their microscopic appearance, the peripheral part of the sensitive spinal nerves, the root of which is cut through between the spinal cord and the spinal ganglion, remains unaltered as long as the connexion with the ganglion is maintained. Ten or twelve days after having divided one or both of the roots of the second cervical nerve, he was enabled to make the following observations: 1. That part of the sensitive nerve which is situated between the place of division and the ganglion, is disorganized in the same manner as any dissected nerve in its peripheral end. 2. Tracing the disorganized fibres into the interior of the ganglion, they are seen mixed with normal fibres; the disorganized ones appear to pass into ganglionic globules, which are likewise altered, seeming to be deprived of their contents, and to consist merely of a thin, indistinct membrane. 3. The normal fibres appear to end by very thin filaments passing into the normal ganglionic globules. 4. All the fibres originating within the ganglion are in their normal state. 5. The motor fibres are completely disorganized in the whole of the peripheral part of the nerve [no motion is produced by galvanism, or any other stimulus]. 6. After having divided only the posterior root, all the fibres *below*, or on the *other side*, of the ganglion were normal. 7. After having divided the nerve *below* the ganglion, or after having cut out the ganglion, all the fibres in the peripheral part were disorganized. It is evident, from this, that the spinal ganglion acts as a nervous centre for the sensitive fibres, but not for the motor ones. Dr. W. promises to give soon more detailed observations, as well on the same subject as on the function of the nervus vagus and sympatheticus.—[*Muller's Archives. Brit. and For. Medico-Chir. Rev.*

On the Influence of the Sympathetic Nerve on the Animal Temperature. By Dr. T. BUDGE.

To the communication of Bernard's observation, that by dividing the sympathetic nerve between the first and second ganglion cervicale, the temperature of the corresponding side of the head soon rises several degrees, and remains increased for some days [*Compt. Rendus, Mars, 1852*], Dr. Budge adds the remark, that he had observed the same fact already in De-

ember, 1851. In another experiment, Dr. Budge destroyed the lumbar part of the spinal marrow, after which he observed a considerable decrease of temperature in the whole of the posterior part of the body.—[*Froriep's Tagesber. Ibid.*

Treatment of Spasmodic Diseases.

Dr. ALEXANDER WOOD, in an interesting paper (*Monthly Journal of Medical Science*, February, 1853,) points out the irrational and contradictory treatment usually pursued in spasmodic diseases. In the severer convulsive diseases, as tetanus and hydrophobia, he remarks, wine, brandy, and opium—stimulants—are conjoined with the cold affusion or cold bath—sedatives. Opium in small doses, which as a stimulant and soothing remedy might prove useful, is counteracted by tobacco a sedative, or by irritating and depressing purgatives. We are told to administer the most violent purgatives in these diseases, and are encouraged to persevere in their employment to an enormous extent, by the hope of eventually obtaining from the bowels an assortment of strange and heterogeneous matters.

“In these diseases,” he adds, “where the violent muscular effort demands a large supply of blood, and where the wants of that supply increases the irritability of the system, we are taught to bleed, beginning with from thirty to forty ounces, and repeating it if need be. What end can bloodletting possibly subserve, unless in those exceptional cases, to be afterwards specially considered, where urea exists in the fluid? There is no morbid matter in the circulating fluid which bleeding can remove! There is no inflammation which it can subdue! On the contrary, that terrible muscular action, like a consuming fire, is draining away the nutriment of the system even faster than your relentless lancet, and when the supply is exhausted the flickering taper will expire.

“In the case of an unhappy infant, ill-thriven, ill-fed, who has imbibed irritability of constitution with the milk of a gay, luxurious, self-indulgent, and nervous mother, and who has had that irritability confirmed by a long course of ill-regulated management; why will you, when the irritation of a tooth has proved the cause of convulsions, not rest satisfied with removing the immediate pressure, and then set about improving the general system, in which the real danger lies, but apply cold to a head where no symptoms of congestion or inflammation is to be found, harrow the gums with daily scarifications, and administer a powerful purgative, glorying in the expulsion of depraved secretions, which only indicate the impaired state of

nutrition, and the necessity for a very opposite kind of treatment?"

Dr. Wood very earnestly solicits attention to the following therapeutical considerations, based on the physiology and pathology of spasmodic affections:—

1. The importance of great attention to a proper supply of nutriment and of air in all circumstances where either hereditary tendency or other circumstances are likely to develop convulsive diseases. Trismus nascentium is epidemic in the West Indies, from the absurd way in which infants are there treated. The same disease was banished from the Dublin Lying-in Hospital by proper attention to ventilation and cleanliness.

Laryngismus stridulus is never so satisfactorily treated as by change of air. The mortality of tetanus in our naval stations in the West Indies has been very much reduced, mainly according to Dr. Dickson, by improved hygiene.—(*Med. Chir. Trans.* vol. vii. p. 765.)

2. Let us divest our minds of the idea that it is necessary to treat the fit in any of these diseases. It is only a part of a train of morbid phenomena, and though the part most striking to the bystander, ought not to make the same impression on the intelligent physician. No one now thinks of treating the fit either in epilepsy or in chorea, and why should we think it necessary in tetanus or hydrophobia?

3. Is it not worthy of consideration whether the obstinate constipation in tetanus and lead poisoning may not be a spasm of the muscular coat of the intestines, analogous to that of the voluntary muscles, and like it, not to be overcome by brute force (*i. e.*, purgatives?) Certain it is, that in lead colic the finger, introduced into the rectum, is held by the sphincter as by a vice, and pressed tightly by the gut, and that this pressure returns at each paroxysm.* Certain it is also, that the retraction and hardness of the abdomen, associated by Merat† with that internal constriction, is found also to exist in tetanus, though I am not aware that any one has ever explored the internal state of the bowel in that disease. It may be necessary to remove the morbid secretions in the bowels, though every intelligent physician will have to balance the amount of irritation produced by their presence with that caused by the drastic purgatives necessary for their removal. The spasm which retains them is the effect of the disease. It is to treat symptoms, to attack it alone; and when we succeed in overcoming the disease, the bowels will spontaneously relieve themselves. Tralles‡

* Tranquerel—*Traité des Maladies de Plomb, etc.* p. 210.

† *Traité de la Colique Métallique.*

‡ *Opii. Usus et Abuses*, sect. 2. p. 260.

found opium succeed in a case of ileus where purgatives had failed. I have seen the same. Lentilius* has confirmed this; and in a severe form of colic, Bonn† became convinced by experience that it was the most powerful remedy.

4. The most efficient and the most frequent agents in the production of these diseases are sedatives. Bloodletting is a most powerful sedative, and if carried to an extent in a healthy person produces convulsions. Is bloodletting, then, a suitable remedy in these affections? In chorea, it was formerly practised, and is now abandoned, from the injurious effects which it produced. In delirium tremens and hysteria, its use has been also given up. In epilepsy, it is rarely used by intelligent physicians, except to meet the requirements of secondary affections; and if we still retain its employment in the more severe convulsive diseases, it is probably only because their rapid course and frequently fatal issue makes it very difficult for us to ascertain the effect of any treatment.

5. In one class of convulsive diseases bloodletting seems, in the present state of our knowledge, to be indispensable. Where urea exists in the blood and produces convulsions, it must either be expelled or counteracted. We scarcely know how to accomplish the latter indication, and therefore are driven back on the former; but even while seeking by bloodletting to get rid of the urea which is mixed with that fluid, we must never forget that it "both acts on the nervous system as a narcotic poison and impoverishes the blood, inducing degeneration of the tissues;"‡ and that therefore, while we take blood to remove the poison, we must do our utmost to replace the nutriment which we are unavoidably compelled to abstract along with it.

6. Our views on this subject would become more definite and precise, could we avoid imagining that spasm implies augmented strength. It is not easy for us, when seeing the violent agitation of the system which prevails, to divest our minds of the idea of great power being developed, but the same remarks apply to mania, in which disease, thanks to the exertions of Dr. W. A. F. Browne,|| general depletion, tartar emetic, brisk cathartics, and ice to the shaved scalp, are no longer in such vogue as they once were.

If such are the objections to the routine practice, what course ought to be pursued? It is easier to point out error than to demonstrate truth. But I venture to suggest—

* Eph. Mat. Cur. dec. 3, ann. 1, p. 131.

† De Officio Medici.

‡ William's Principles of Medicine, p. 150.

|| Monthly Journal, 1841, p. 75.

7. That sedatives should be cautiously used. Chloroform and cold affusion have each proved fatal in delirium tremens.

8. That every effort should be made to put into the system as much nourishment as it is capable of beneficially employing.

9. But, if there is any faith to be placed in antipathic treatment, it is to stimulants we must most trust.

10. It is worth observing, that most of those chemical agents which produce convulsions, acting, as has been already said, in large doses as sedatives, do in small doses act as stimulants.

11. We find also, that where the minor spasms, as cramp, have been excited by irritation of the peripheral distribution of one class of nerves, as those of the mucous lining of the bowels, they are often relieved by irritation of the peripheral distribution of another class of nerves, as by friction on the skin.

12. The cerebral functions, more especially volition and sensation, being much in abeyance, any stimuli, whether mental, or physical, by which they can be excited, should be freely given. It was on this principle that Boerhaave prevented the recurrence of epileptic attacks, by directing a red-hot iron to be applied to any who might be seized.*

13. The extreme sensitiveness to all irritations which exists whenever the spinal predominates over the cerebral system, suggests the propriety of enforcing the most absolute quiet, and preventing the access of all bodily and mental stimuli. In tetanus and hydrophobia, the creaking of a shoe, the slamming of a door, the sight or even the thought of water, or the gentlest zephyr playing on the surface of the body, excites a fit. Hence Armstrong tells us, that in tetanus those patients recover best who get little active treatment, but are nursed, as it were through the fit.†

14. And if sources of irritation ought to be prevented, those actually existing ought to be removed, but never by irritants which are more powerful than themselves. This surely gives sufficient latitude to the most devoted admirers of the gum lancet, the bolus, or the bag and pipe, while it suggests to him a little caution in these somewhat coarse though most popular remedies.

15. And lastly, if I have not succeeded in pointing out any one remedy which stimulates the cerebral without exciting the spinal system, it is because few, if any such are known. It is a subject of investigation well worth attention. And if no particular plan of treatment has been announced, which can be unhesitatingly recommended, I am perfectly satisfied if I have created any doubt as to the course which at present is too

* Falconer on the Influence of the Passions, p. 100.

† Lectures, edited by T. G. Thomson, p. 572.

unhesitatingly followed. I think, at least, something has been done to show how much more constitutional these affections are than is usually supposed, and thus to open up new plans for prevention, if not for cure. And if in acute mania, in puerperal insanity, in delirium tremens, in chorea, in laryngismus, and may I add in epilepsy also, empiricism or the influence of authority has induced us to abandon antiphlogistic treatment, and to adopt stimulant and soothing remedies, I do not despair of a sounder pathology soon prevailing in regard to this whole class of afflictions, and of the discovery yet being made, that they are much more amenable to treatment than has hitherto been supposed.—[*American Journal of Med. Sciences.*]

Pathology of Rheumatism and Gout. By Prof. J. H. BENNETT.

The present theory with regard to these affections is, that they are both connected with an increase of lithic acid in the blood. In rheumatism, this is dependent on excess of the secondary, and in gout, on excess of the primary, digestion. In rheumatism, however, there is considerable excretion of lactic acid by the skin (Todd,) whilst in gout there is an excess of soda, which, uniting with the lithic acid, produces a compound of lithate of soda, that may be detected as such in the blood (Garrod,) while sometimes it exudes into the cellular tissue of the skin, constituting tophaceous deposits. In both diseases, there is an undue balance between the excess of lithic acid and the power of excretion—in rheumatism by the skin, and in gout by the kidney. This pathology serves to explain the similitudes and differences existing between the two affections. In both there is a certain constitutional state dependent on deranged digestion, during which exciting causes occasion local effects. These exciting causes in rheumatism are bad diet, hard work, exposure to cold and wet, and its subjects generally are the poor and laboring population. In gout, the causes are good diet, indolence, repletion, or indigestion, and its subjects are for the most part the rich and sedentary. The local manifestations in both are acute wandering pains, with pain and swelling—in rheumatism of the large, and in gout of the small joints, constituting the acute attack in the one, and the so-called regular attack in the other. These are combined with a tendency to various complications of the internal viscera, which are more or less dangerous to life.

The general indications of treatment are, in both diseases (1.) so to regulate the nutritive functions as to insure a due balance between the amount of matters entering the blood as the result

of digestion, primary or secondary, and the amount of matters discharged from the economy by the excretory organs. (2) To conduct the acute attack to a favorable termination, carefully watching the internal viscera, and being prepared to act with vigour should these become affected. Hence the treatment of these diseases resolves itself into what may be called curative and preventive—the first having reference to the acute attack, the second to the means most likely to hinder its return; the one must be carried out by remedies which act upon the blood and excretory organs, the other by the management of diet and exercise.—[*Monthly Jour. Med. Science.*

Treatment of Acute Rheumatism by Nitrate of Potash. By
Prof. J. H. BENNETT.

Although the general pathology above mentioned [see preceding article,] which considers rheumatism as a blood-disease may be considered on the whole as correct, we are not yet enabled to explain by it the symptoms of an acute attack of the disease, where, in addition to the constitutional disorder, we have local pain, occasional heat, redness and swelling, with febrile symptoms. Most practical men have attributed these phenomena to a superinduced inflammation, although it has not been shown that exudation occurs, or that it is followed by the usual results of that condition. Besides, its erratic character is opposed to what we know of the process of true inflammation, and calling it an unhealthy inflammation in no way clears up the mystery. The real pathology of acute rheumatism, therefore, has yet to be determined, and as a preliminary step, a careful histological examination of the affected tissues is absolutely necessary. So far as I am aware, this has never yet been attempted, if we except some observations by Hasse on the Structure of the bones in rheumatism (see the *Monthly Journal* for June, 1847.)

Our treatment of this disease, therefore, is purely empirical sometimes directed against the pain, at others against the supposed inflammation; now attempting to combat the pathological condition of the blood, then striving to remedy its effects by acting on the excretions; and not unfrequently giving specifics, in the hope that any change in the constitution, however produced, may be beneficial. In no disorder, probably, has such a crowd of opposite remedies and plans of treatment been extolled, and yet none of them can be depended on, so that it has been imagined that six weeks' rest is the most useful prescription (Warren.) The latest author on rheumatism endeavours to explain this by observing, that this need not be won-

dered at by "those who consider the true nature of the disorder and the variety of circumstances under which the physician may be called upon to minister to his patient's relief. The bleeding, which in the young, plethoric, and robust, may be necessary to allay excessive vascular action and cause free secretion, may in the weakly induce irritability of the heart, and a consequent attack of cardiac inflammation. The opium, which in one person may prove of the greatest service in promoting free perspiration, and in allaying the general irritability of the system, may in another check the biliary and other secretions, and thus prevent the elimination of the rheumatic poison. The continued use of calomel, and the constant purging, which may be beneficial to one patient by removing large quantities of unhealthy secretions, may unnecessarily exhaust the strength of another, and tend very greatly to impede recovery. And so in regard to every remedy which has been proposed. What is useful at one time proves useless, or positively injurious, at another; and the conclusion is forced upon us, that what is wanted 'is far less the discovery of untried methods of treating disease, than of discriminative canons for the proper use of those we possess :—far less the discovery of any new medicines than the adaptation of our present remedies to the exigencies of each case.'" (Fuller on *Rheumatism*, p. 73.) These judicious observations may serve to explain the cause of our failure; but until we obtain more exact information regarding the *special* pathology of rheumatism, it is in vain to hope for a rational treatment.

For my own part, I generally treat rheumatism on what is called "general principles;" these are, to alleviate severe pain by anodynes, diminished excessive vascular action by moderate bleeding and saline antiphlogistics, and encourage every attempt at critical discharges by diaphoretics, diuretics, purgatives, etc. Occasionally I have tried the effects of special remedies in this disease, and watched a series of cases, all of which were treated in the same manner. Thus I have tried aconite, and believe that alone it is of little service; colchicum, also, I have given frequently, and am of opinion that in pure rheumatism it is of no advantage, although in gout it is invaluable. This session (1851-2) you have witnessed another trial of this kind with the nitrate of potash, a remedy formerly recommended by Dr. Brocklesby, and which has been given with good effect by M. Gendrin, in the wards of La Pitié in Paris, as recorded by Dr. Henry Bennett (*Lancet* 1844, vol. i. p. 374.) It has more lately been pressed on our attention by Dr. Basham (*Medico-Chir. Trans.* vol. xxxii.,) who tells us that from one to three ounces of the salt, if freely diluted in water, may be taken by the

patient in the course of twenty-four hours, without any injurious results, but with the effect of relieving in a marked manner the swelling, heat, and pain in the joints.—[*Ib.*]

Miscellany.

We have received from a much esteemed correspondent a communication having for its object the criticism of an article to be found in the original department of one of our late numbers. The writer takes exception to the treatment of the case, and gives his reasons for thus differing from the reporter. While we can perceive some advantages to be derived from such criticisms, we also feel assured that the evils would very far counterbalance them. There are few, very few, men who can discuss a question, even purely scientific, without forgetting the proprieties without which any controversy may degenerate into personalities. It is therefore that we have invariably refused to admit into our columns any thing having the least tendency to such a result.

It may be well, on the present occasion, to reiterate, that in publishing original communications, with the name of the author, we cannot hold ourselves responsible for any theories, opinions, or modes of practice they may advocate. We have frequently published articles containing views, theoretical and practical, utterly at variance with what we would deem correct—yet the name of the writer being given, he can alone be responsible for the impression made upon the reader. If we were to publish none but such as we could endorse in every particular, we apprehend that we would soon incur the odium of being unnecessarily fastidious.

The true policy is to let every writer stand upon his own merits. If he publishes nonsense, he will soon cease to be read and to do mischief: if, on the contrary, his contributions are found to indicate thought, judgment, and a well stored mind, he will be duly appreciated and read with advantage. An Editor, it is true, has the privilege of rejecting such communications as may be deemed unworthy—and we do not hesitate to exercise the right whenever necessary—yet we always feel inclined to make periodicals as democratic as possible by lending their pages to whoever wishes to be heard—provided he knows how to write.

Books Received.—We have before us the “Proceedings of the Medical Association of the State of Alabama, at its sixth annual meeting held in the city of Selma in December, 1852.” This is a neat

volume of about 170 pages, filled with interesting matter, and highly creditable to the profession in our sister State. We hope to be able to refer to it more at length in our next.

We have also received the "Tenth Annual Report of the Managers of the State Lunatic Asylum of the State of New York." The "Report of the Eastern Lunatic Asylum in the city of Williamsburg, Virginia, for 1852-3." "The Philosophy of Medical Science, considered with especial reference to Dr. Elisha Bartlett's Essay on the Philosophy of Medical Science. A Boylstown Prize Essay, 1849. By E. Leigh, M. D. Boston, 1853. Addresses to the Graduates of their respective Schools, by Prof. J. W. Richardson, of Nashville; by Prof. N. S. Davis, of Chicago; by Prof. M. M. Pallen, of St. Louis; and by Prof. H. V. Wooten, of Memphis—all of which evince a high order of talent.

More Medical Journals.—We have received "The Virginia Medical and Surgical Journal," edited by George A. Otis, M. D. and Howel L. Thomas, M. D., published monthly at Richmond—price \$5. Also, "The American Psychological Journal, devoted chiefly to the elucidation of Mental Pathology, and the Medical Jurisprudence of Insanity," edited by Edward Mead, M. D., and published at Cincinnati—32 pages Bi-monthly at \$1. We cheerfully add them to our list of exchanges, and wish them a successful career. Our Virginia friends manifest a degree of industry and good taste, as well as of ability, which must secure to them an extensive patronage.

Dr. Frank A. Ramsay has, it appears, abandoned the East Tennessee Record of Medicine and Surgery, and joined the editorial corps of the "Southern Journal of the Medical and Physical Sciences," published at Nashville.

The Country Doctor.—Some of our contemporaries pride themselves upon never quoting anything from "the secular press;" but we have always preferred to adopt the wiser course suggested by the motto of the Southern Medical Journal—"je prends le bien où je le trouve"—and in so doing do not feel that any one's professional dignity has been injured, or any one's professional robe sullied by contact with the common herd. Acting on these principles, we give below an article from the Knickerbocker, entitled "The Country Doctor, a faithful autobiography: by Glauber Saultz, M. D." Perhaps all our readers can narrate facts which surpass these; we are sure almost all can equal them; but then it is easier to laugh at another's misfortunes than our own, and "Doctor Saultz's" feelings will not be hurt if every one laughs as hard as it will answer for a dignified M. D.;

neither will he make a charge for his visit.—[*New Hampshire Journal of Medicine.*

I had stumped about the country for a dozen years or so, in the same equipage, having wonderful success in curing "cases," but half the time cheated out of the credit of it by catnip tea. I took a notion to cast up my books to see how rich I was, and what could be made of outstanding accounts. It cost a great many evenings of hard work to arrive at the knowledge that, all debts being paid, I was not worth a "brass farthing"—not a red cent. Notwithstanding all the lucrative cases of typhus which I had managed, I remained poor. I believe that people in the city pay their fees with alacrity, because the charges are exorbitant. When a bill for a hundred dollars, for looking two or three times at a sick child, is presented to one who lives in a well furnished house in the upper part of the town, the very largeness of the demand is a delicate compliment upon his ability to pay. The man of the house sits down at a handsome secretary, and draws out a clean check for the full amount, saying "Doctor, you are very moderate: now that Jacky is out of the woods, come in, in a sociable way."

As soon as the messenger is gone, the *pater familias* exclaims, "What an outrageous bill! It is an expensive luxury to be sick." However, it has its advantages to be attended by a fashionable doctor, as it has to worship in a fashionable church. On one occasion I was called in midsummer to attend a sick man on the sea-shore. After several days, his family physician, the renowned Doctor Jallaps, arrived from the city, and the patient was soon after on his legs, no thanks to me, and ready for the surf.

"How much are you going to charge him?" said Doctor Jallaps.

"Twenty-five dollars," said I.

"Poh!" said he, "make it a hundred. He expects it."

"If he expects it," said I, "it would give me great pain to disappoint his expectations;" whereupon I acted advisedly, and received an honored check for a round C. on the Phœnix Bank.

On another occasion, when attending one of my own patients in the same vicinity, while crossing the "big bridge" when the tide was up, I came near being drowned. My sulky was soon afloat, but the horse, being a good swimmer, reached the opposite bank. Now, besides risking my own life, I fairly dragged the patient from the very gates of death. I got him out of a bilious remittent, drove the jaundice out of his skin, and when I came to ask him for ten dollars, he black-guarded me like a chicken-stealer, and would never employ me again. The fact is, that people in the country abhor taxes, and a doctor is the worst of publicans. To be sick they think is a dead loss, which they unchristianly grumble at; but to have to pay for being cured irritates them beyond measure. Oh! how meek they are when they lie prostrate in a burning fever—when their teeth chatter, and the whole house jars with their shaking ague! Oh! how welcome the latch is lifted up to admit you when life seems to hang upon a hair! But get

them on their legs, and the first thing which they forget will be that they were ever on their backs. If many of them do pay you, it is under protest, procrastinating the settlement to a time when the account might be outlawed, clipping down the fair proportions of a just bill, and giving you the most ragged representative of money.

I say that when I came to overhaul my accounts, I was not worth anything, and therefore arrived at the conclusion that it was high time to marry a wife who would take care of my money. I did so, and found my condition better, but for some years had a hard time of it. My children were extremely pettish and peevish, and what with nocturnal calls, I had not a night's rest for five years. If anything ailed them, they were sure to cry the night long; but if they were well, they woke up long before the crowing of the cock, climbing over me at the very moment when I had composed my head for a short morning nap. But paternal philosophy can well be reconciled to the sweet music of "crying babes," some thousands of which have been imported into New York during the present year. But the number of people taken sick in the day time, who send for the doctor at night, produced a compound fracture of my time, which seldom gave me a comatose state. It is the sweetest of all consolations to lay a weary head upon the pillow, with the thought that rest awaits you until the dawning light. Whatever carking cares have vexed you, that is a long season of immunity which stretches through the dark hours of the night. Then do the strained muscles lapse into the most easy attitudes in the yielding couch, and the taxed intellect is still, and you bolt the door upon ingratitude and strife.

But to lie down without security from disturbance is enough to frighten away sleep. Such is the lot of a country doctor. I could relate innumerable instances of the utter disregard with which he is routed from his bed, without occasion, at all hours. Here is one in point.

I arrived late one winter evening at my own door, after a hard day's toil. With what a feeling of relaxation did I divest my feet of heavy boots, set them smoking at the fire, and then regale them in easy slippers! Then, wrapping about me a soft padded gown, with what luxury did I fall back in my arm-chair, peruse the daily paper, and sip a cup of tea! "Now," said I, "the labors of the day are over. A storm is brewing out of doors. I hope that no body will come here to-night. If they do, I won't go. Let them go after Bogardus. I won't immolate myself for any body. It is unreasonable." With that I pulled down my ledger and made a note of the day's visits, one half of which were to poor houses, negro huts, and Irish shanties. As to this class, they loved me like a brother, and their confidence in me was unbounded. They sent for me if their bones ached, or if their corns hurt them, and I went with all speed, though I sometimes had occasion to scold them. Before retiring for the night, I opened the outer door, as was my custom, to see the state of the weather. It was a tremendous night. The moon shone palely, but the wind blew a hurricane. It rained, it hailed, it snowed, it blowed. I thought again

of the poor mariners on the coast, and with a silent prayer for them, and all houseless, unprotected ones, I closed the door, and went to bed. I had just recovered from the shivering sensation of cold sheets, and become conscious of grateful warmth, while that delightful drowsiness which borders upon sound sleep stole over me, when there came a knocking, impatiently repeated, enough to wake the dead. "Bless me!" I groaned out, crawling out of bed, and lifting the sash, "what do you want?"

"Doctor, want you to come right straight away off to Bank's. His child's dead."

"Then why do you come?"

"He's p'isoned. 'They gin him laud'num for paregoricky."

"How much have they given him?"

"Dono. A good deal. Think he won't get over it."

"When did they give it to him?"

"This arternoon."

"Why didn't you come sooner? How do you think I am to go two miles on such a night? Have you brought a wagon?"

"No."

"Then I won't go. Tell them to ——;" and having prescribed hastily out of the window, I closed the sash, and went back to bed. But the howling wind and rattling sleet against the panes had not that soothing effect which they have to one who lies snug and warm and irresponsible in his couch. "What," said I, "if that child should die through my neglect! Will it absolve me from criminality because the parents are poor? I will go: I must." With that I leaped out again, kindled a match, and went down into my office. Not choosing to wake my man Flummery, or to disturb my old horse, who was craunching his oats, and housed for the night, I took my stick, and set out to walk. The snow water went through my shoes like a sieve; my neck and bosom were instantly covered with sleet. Nevertheless, I had some humorous thoughts while breasting the storm, and composed a Latin distich by the way. I had just got the last foot of the pentameter correct, when my own foot struck against something which looked like a black log. On scrutiny, by the light of the moon, I found it to be my old patient, Timmy Timmons, apparently sound asleep, with his beloved rum-jug by his side. In vain I shook him, to make him aware of his situation, and see if the spirit had left his body. I shook the rum-jug, but there was no spirit there, not a drop. "Timmy," said I, "wake up." No answer. I then kicked him, but he bore it as if he had been used to kicks. "He is dead," said I, and passed on to the next house. There, while opening the gate, I was fiercely attacked by a stout bull-dog; and while keeping him off, and fighting my way up to the house, the master came out in his shirt-tail with a loaded gun. "Don't you know me?" said I, as he examined the priming; "it is the doctor."

"Souls alive!" responded he; "I thought it was a thief! I'm glad you spoke when you did. In a minute more I should have popped you over, Doc'. Sorry to do that. My son John's got the fever-aig. Here, Bull, Bull, Bull, Bull!—g'home, sir!"

"Timmy Timmons," said I, "is lying out in the lane, drunk or dead, I don't know which; dead drunk, at any rate. He must be looked after."

"Wait till I put on my breeches. What a wonderful night! Won't you come in and git warm?"

"No: get on your breeches, and make haste."

"Guy! when I first heered you, I thought it was Lawrence comin' to break house. He's a desput fellow. So I gets up and looks out o' the window, and then I went into the corner to find my gun, and if I didn't——"

"Come, come; do you want——"

"To get the rheumatiz? No, I don't. Hold on, Doctor; be down in one minute."

We returned to the congealed Timmons. My coadjutor took up the jug, shook it, and said, "Not a drop." He then smelt it.

"It is rum," said ., "the cause of all this misery."

"No, Doctor, not *all* rum; there's been a little *molasses* into the jug, by the smell of it."

"Lift him up," said I. He did so, and carried his burthen home, where I brought Timmy to life.

"I now trudged on upon my original errand, hoping to save another life more valuable than that of Timmons. Arrived at the house, I perceived it shut up as if hermetically sealed. Not a light was to be seen. I knocked furiously, and at last a nightcap appeared from the chamber window, and a woman's voice squeaked out, "Who's there?"

"The doctor, to be sure," said I; "you sent for him. What the dogs is the matter?"

"Oh, its *no* matter, Doctor. Ephraim's better. We got a little *skeered*, kind of. Gin him laud'num, and he slept kind o' sound, but he's woke up now."

"How much laudanum did he swallow?"

"Only two drops," said she. "T as'n't hurt him none. Wunnerful bad storm to night!"

I buttoned my coat up to my throat, turned upon my heel, and tried to whistle.

"Doctor, Doctor!"

"What do you want?"

"You won't charge nothin' for this visit, will you?"

Now, as I travelled back on foot, the moon became obscured, the driving sleet blinded the eyes, I heard the Atlantic breakers booming and beating upon the coast; and with head down, like a bulrush, I arrived at my own door, wet and disconsolate, saying to myself—
"THAT LITTLE PLANT CALLED PATIENCE DOES NOT GROW IN EVERY GARDEN!"

Orfila, the great Toxicologist, is no more.—The Parisian papers are full of the honors paid to his memory. His funeral was attended by upwards of fifty thousand persons, and eulogies were pronounced at his grave by seven distinguished representatives of the Scientific bodies of which he had been a member.

SOUTHERN MEDICAL AND SURGICAL JOURNAL.

Vol. 9.]

NEW SERIES.—JUNE, 1853.

[No. 6.]

PART FIRST.

Original Communications.

ARTICLE XX.

Cases of Malignant Spotted Fever;—with Remarks. By
ARTHUR W. PRESTON, M. D., of Americus, Ga.

The following cases of malignant spotted fever are sent for publication,—it may not be amiss to state that, in their delineation and treatment, the utmost frankness is observed :

During the past twelve months, all the endemic diseases have increased in number and intensity. The first months presenting serous diarrhœas, cholera infantum and cholera morbus, with occasional cases of angina pharyngitis. From June to the middle of September, intermittents and remittents, together with melanosis and carbuncle. From the middle of September until November, malignant double tertians, some few algid, others combined with semi reaction—in which inflammations of the asthenic type prevailed—which if not promptly treated passed within a few hours into profound and irremediable congestion. Carbuncles have not diminished from their first appearance up to the present time. From November to the present, the following diseases have been in existence: Intermittents, chronic and relapsing; measles, very abundant; congestive pneumonia; congestive bronchitis; typhoid pneumonia and spotted fever—the latter disease is the one which I shall now speak of.

In Copland's Medical Dictionary, page 1176, will be found

an article upon Spotted Fever of New England, which bears a strict analogy to the cases we have recently met—in which the primary lesion must have been upon the ganglionic nerves, and the second upon the vascular system. So great was the rapidity of the attack in four cases, although sent for immediately, we had not time to reach them before they were dead or dying. As these diseases sprung upon us without premonition, we were entirely unprepared to determine what would be the general outlines, should more present themselves. It is an easy matter to point out defects when too late to remedy the evil. That no more shall follow, I beg permission to review the nature and treatment before I submit to your criticism, after which, any light you may deem proper to throw upon them will be thankfully received.

CASE I. March 28th, 1853. Russel Jenkins, aged 22—sanguine nervous temperament, five feet seven inches high, weight 130 lbs., occupation a farmer, habits perfectly temperate and industrious, circumstances good—on the 27th, in the afternoon, complained of general indisposition: took a dose of salts, which operated five times; after which, continued to grow worse to the time I saw him on the following morning at 8 A. M., at which time the countenance was altered from a florid to an exsanguious appearance; the eyes were peculiarly brilliant and jactitating; lips pale; tongue broad, flat, thin, and very pale, but firm in fibre; teeth cold, dry and dead; very restless, tossing from one side of his bed to the other; acute lancinating pain in the head; soreness of the muscles and joints; stiffness and soreness on the back of the neck; some sore throat; pulse 80, full and compressible; great anxiety and constant thirst; occasional delirium; hysterical symptoms. Had been vaccinated four weeks, previous to which his health had been very good; but since, had been from time to time troubled with pains in his joints, and thought he had rheumatism. Two days previous to confinement he helped to ditch, and was above his ankles in water the greater part of that time, during which he thought he had fevers.

Diagnosis.—I was slow in forming an opinion as to the precise character of his disease. The more I looked at it, the

more I was puzzled, and shall not fear censure when I confess the following: At one time, I thought it a retrocession of rheumatism upon his brain—at another, of the secondary vaccine fever. Again, that some malignant exanthema had concentrated all its powers upon the cerebro-spinal axis. The progress of the case determined the justness of the latter conclusion.

With appearances so malignant, and uncertainty as to the correctness of my diagnosis, I trusted to general principles for my guide. I therefore summed the following symptoms:—1st, the general exsanguiousness; 2d, the cold, dry and dead teeth; 3d, restlessness, &c., and resolved to support the vital powers as I best could, until further indications. Instead of his being benefited by the free purgation on the previous day, he rapidly grew worse—a weighty argument against venesection. He was ten miles from town; I therefore did not ask for consultation, but gave the following: Hyd. sub. m. gr. 3, ipecac. gr. 2, morph. sulph. gr. $\frac{1}{4}$, every eight, and sp. æth. nit. every four hours. A blanket was divided into three parts, immersed in hot water, and well sprinkled with turpentine—then applied over the thorax, abdomen, feet, legs and knees. The first dose of nitre was immediately rejected, together with a pint of green fluid, mixed with mucus. 10 A. M. More quiet; blood returning to the surface; headache not so severe. 12 M. Headache better; tongue red.

29th, 8, A. M. Had slept but little during the night. Family thought he would die before morning. Is now watchful, restless, and constantly occupied about his farm; motions automatic, and passes rapidly from one idea to another. 4 P. M. Was visited by Drs. Barlow and Vagt, in company with myself. The conjunctiva was beautifully injected, and the eyes peculiarly brilliant; there is much more calmness, and answers our questions rationally; headache is greatly diminished—the pills have procured three bilious operations. Treatment, as before. After our leaving him, continued to grow worse, and passed a very restless night, slept none, and was constantly fighting his brothers, one of whom he bit on the arm. During the night, he got out of his bed and climbed the logs up to the joists before they could control him.

30th, 10 A. M. All the symptoms had now become greatly aggravated; constant jactitation; tries to get out of bed so that he may work. *Typhomania*.—On the arms and body there are several ecchymoses, from the 12th to the $\frac{1}{4}$ of an inch in diameter, their edges jagged and somewhat raised with a few vesicles. On close examination, a few rose-colored spots about the size of a duck-shot, smooth and circular, and others twice or three times as large, resembling a blood-blister. They were filled with a sero-sanguineous fluid. Blisters to extremities, cupping on the nape of the neck to one half-pint, anodynes and diaphoretics, with wine, constituted the remainder of the treatment. He grew rapidly worse until he died, on the evening of the 31st.

REMARKS.—During the last two days of his illness, his delirium and restlessness constantly increased, until he completely exhausted his strength; and when he no longer possessed strength to articulate, he muttered incessantly and gradually passed into a moan, which continued to his last moments. The blotches, after death, were conspicuous, and in all might have numbered two hundred or more.

CASE II. Marion Young, age 23, five feet six, weight 120 lbs., temperament sanguine, circumstances good, lived five miles from Case 1st; previous health bad, was a dyspeptic and invalid; occupation a farmer—the brother-in-law of case 1st, and waited on him two nights before his death.

April 1st, 10 P. M. Complained of headache, and vomited bilious matter; general soreness of muscles and limbs. Did not think it necessary to obtain medical aid until early the following morning.

April 2d. Died at 11 A. M., without treatment.

No post-mortem allowed. Deceased was covered with purple blotches and ecchymoses over the whole body, and scarcely was there a square inch free from them—their general diameter was the $\frac{1}{2}$ of an inch, some as large as a ten cent piece. From each nostril a large amount of bloody froth issued—there must have been a pint or more.

CASE III. Marion Jenkins, age 32, a farmer, and brother of

case 1st, and was bitten on the left arm by case 1st, lived four miles from case 1st.

April 2d. Was slightly indisposed the preceding night, and greatly alarmed—thought his brother might have been poisoned, and looked to be taken ill himself; but when so taken, was not desirous to obtain medical aid. At 3 P. M. the following symptoms were noticed:—Headache; soreness of the limbs and muscles; body moist with sweat, and the hair around the temples perfectly wet with it; is disposed to sleep, but when awake his expression indicates alarm, and a close examination with a view to the reading our thoughts as to the nature of his disease; at times tries to throw around him an air of carelessness, and says, Pshaw!—he knows he is not like the others, and is not going to be very sick. Drs. Barlow, Fish and myself, agree upon the following for the night:—*Infus. senna*, \mathfrak{z} iv., *Magnes. sulph.* \mathfrak{z} ss., to be taken immediately. Liberal doses of morphine after the operation of the above, to quiet his fever and support his strength. 5 P. M. Sweating profuse, over the head, face and chest—large colliquative drops; begins to talk at random about his wagons and cart wheels, immediately after the operation of the senna and salts, which took place within five minutes after they were given, which were bilious and watery. Every appearance of rapid congestion was setting in with great force—*Tr. opii.* xxx. gtt., and in ten minutes xxx. more. The fever, which was before violent, and pulse 120 to 130, full, but compressible, sunk to 90. Sweating diminished, together with the delirium, as it were by a charm, and prospects brighten. I should have stated that we discovered rose-colored spots upon his chest, arms and thighs, in number twenty, and one line in diameter: they were well marked, smooth and circular. On the left elbow there was one sero-sanguineous blotch larger than a pea, and its surface distinctly elevated. 9 P. M. Symptoms good, skin a little moist, and pulse 80 to 90. *Tr. opii.* gtt. xxx., with a draught as follows:—*Infus. senna*, \mathfrak{z} j.; *senega*, \mathfrak{z} ss.; *magnes. sulph.*, \mathfrak{z} j.; *zingib.* \mathfrak{z} ij.—to be repeated every four hours.

April 3d, 1 A. M. Continues better; says he feels well, and wants to sleep. As there was from the first a disposition to profound sleep, I ordered him to be awaked when he slept

too hard. 5 A. M. No change. 9 A. M. Has discovered the blotches upon himself, and says he knew he had the same disease, but did not mind, as he knew he should get well. The blotches were now numerous—in all, perhaps, two hundred, or more. From a fine rose color they had passed to a muddy brown, and some of them looked as though he had been severely pinched—were much darker, and towards the depending margin about $\frac{1}{8}$ was sero-sanguinous, with a cuticle resembling a blister: in fact, there were rose-colored, purple and dark, promiscuously over the whole body at the same time. One particular circumstance deserves mention: A large consolidated plug of mucus, mixed with blood, was blown from his right nostril—it was 2 inches long, $\frac{1}{2}$ wide, and nearly the same in thickness. 1 P. M. Camphor 5 grains, and tr. opii. gtt. xv. every four hours. There has been from the beginning great soreness of muscles and stiffness of joints; on the right forearm there is acute tenderness—the moment it is touched, be it never so tenderly, he cries out; from the metacarpus to the phalanges of the fore-finger there is a deep inflamed blush; the left thigh, leg and ankle are similarly affected. There is, moreover, swelling of the glands in his groins.

April 4th, 10 A. M. There has been a febrile exacerbation the preceding nights, which abated after a mild sweat. There is now considerable soreness of the whole body, with deep red blushes on the affected limbs. The blotches have ceased to increase—they are all dark brown or purplish. Pulv. ipecac. 3 gr., camphora. gr. 4, with infusion of senega every six hours; at night, 2 grs. hyd. sub. m. were added to each of the above powders.

5th, 7 A. M. The calomel has produced three dark consistent operations. He now becomes dejected, and is very solicitous about his recovery. His wife has importuned to give my opinion, and though several times I have declared almost positively he will recover, still she is frantic with the loss of hope. M. J. sees her distress and becomes more cast down. I endeavor to calm him, whilst he says, "I am worse, and shall die." "No, your prospects are bright, and you ought not to suffer yourself thus to be cast down, when your present weakness is produced by the operations, from which you will soon

rally." No, I shall die, and that before night; you will see it, and I know I shall." To be taken immediately—tr. opii. gtt. xv.; sp. am. aromat. gtt. xx. It was rejected in a few minutes. Again we gave am. aromat. gtt. xx., which produced a burning sensation, and was therefore discontinued, and instead thereof a wine-glassful of poor port wine (the best we had). 9 A. M. Is much calmer, and feels better. As he has not been left alone, without the presence of a medical man, since his illness, is afraid to be left, as he apprehends some sudden difficulty which may sweep him off: is, therefore, urgent in entreaties for Dr. Fish and myself to remain until Drs. Barlow and Vagt return. 4 P. M. Called with Drs. Barlow, sen., and junior. Is very much improved, and no longer retains the morning's presentiment. Dover's powders grs. v. with infusion of senega every eight hours.

6th, 10 A. M. The blotches are disappearing, and continues to improve. Pulv. camphoræ, ipecac et opii. every twelve hours.

7th, 12 M. Has had fever the preceding night; blotches have nearly disappeared. Took ol. ricini. ʒss. in the morning—it griped severely. Ordered tr. opii. gtt. xv. et infusion senega every twelve hours. The inflamed places are nearly gone, and he lies with more comfort. Soreness up to this time has been too troublesome to admit his lying long in one position.

9th. Blotches gone; speaks of going home next morning, which was allowed, owing to peculiar circumstances.

REMARKS.—In case 1st, the urine was very abundant until the last two days: it was slightly albuminous towards the latter part—almost entirely suppressed, it did not exceed one tablespoonful, and was highly colored.

In case 3d, the urine was abundant and slightly albuminous: on the fifth morning was scanty, and when boiled was brownish and muddy. Calomel was administered at two different times—first by Dr. Barlow, and next by myself. Although it might have done good, I believe it interfered with the critical sweats, and, therefore, prolonged the febrile exacerbations.

CASE IV. William Gaskins, age 30, five feet ten; of a san-

guine nervous temperament; possessed a feeble constitution; was crippled by extensive diffuse cellular inflammation, produced by a wound in the knee.

April 2d. Had nursed case 1st, and stayed up all night with case 3d—was extremely devoted in attention to the latter. At 9 A. M. the following day, I was told Wm. Gaskins was afraid he had been sitting up with a case of malignant small-pox, and *if so, he was certain he should take it and die*. He soon after presented himself to Dr. B. and myself, upon which the following dialogue ensued:—"William, you are not afraid, are you?" "Yes, to tell the truth, I am;" at the time trembling and very pale. "What are you afraid of?" "The small-pox—they say it is the small-pox which killed R. J. and M. S., and I believe it is." Finding whatever we said to the contrary unavailing, we persisted no longer, but gave him tr. opii. gtt. xv., and a dose of senna, senega and salts.

April 3d. Was visited by Dr. Barlow and myself. 6 P. M. Eyes very red and muddy; face deeply flushed; but one rose-colored spot upon the gastric region; pulse 120, full and compressible; complains of severe pain in the head; gentle moisture over the body. A pint of bilious matter is vomited, and no relief follows. Thirst urgent and unassuageable. Ordered senna, salts and ginger, every two hours, until free purgation ensued, and thereafter morph. sulph. gr. $\frac{1}{3}$, with sp. æth. nit. every six hours.

4th, 12 M. Was found by Dr. Barlow almost pulseless; features deeply altered, and extremities icy cold. There were deep purple blotches over the whole, with intervening spaces about half an inch in diameter. Wine and other stimulants were freely given, until Dr. Fish and myself saw him at 11 P. M. He was then pulseless, rational, and the extremities flexed; the fingers drawn in permanently upon the palms of the hands; conjunctiva dry and puckered. Ordered sp. am. aromat. gtt. xl. in warm wine every fifteen minutes; sinapisms to spine, &c.

CASE V. April 3d. A stout negro man, belonging to Rev. Jasper Hayne, age 22, possessing previously fine health, lived five miles from the above cases; had visited M. J. and staid

about the premises an hour—after which, went home, and was seen walking about the yard on Sunday morning. The same night, on Mrs. Hayne's return, was found lying upon his back groaning, but speechless and insensible. At 8 P. M. he died without treatment of any kind.

Autopsy, eighteen hours after death.—Owing to very pressing circumstances we only examined the thorax and abdomen. Upon lifting the sternum, the pleura was more than ordinarily dry; the pericardium was filled with serum; the right ventricle was filled with dark clotted blood; the left contained a fluid but dark blood. On the anterior outer wall of the heart there were a few ecchymoses about the size of a duck-shot. The lungs were infiltrated with a sero-sanguineous fluid, and upon squeezing them a bloody froth exuded. The surface was mottled and very much ecchymosed. The cavity contained a pint and a half, or more, of serum.

Abdomen.—The abdominal viscera presented a dry, muddy and brittle aspect; the omentum was shrivelled, and the mesentery as if it had been parched. Upon the arch of the colon there was an ecchymosis the size of a ten cent piece—its color was mahogany. The viscera were otherwise healthy.

CASE VI. Mrs. Jenkins, age 62, the mother of case 1 and 3, had waited on both. The two died, and the third was taken ill at the house. Her health was previously good.

April 9th, 9 A. M. Vomiting; pain in the head, and general soreness of the whole body. 12 M. Was seen, and declared she should die: therefore, wanted nothing further than to make her die easy. At 6 P. M. she was a corpse. There were no blotches upon her body; countenance natural after death.

CASE VII. Olivia, an orphan girl, age 14; previous health good; lived at E. G. Brown's, three-quarters of a mile from Mrs. Jenkins; had no communication with the above cases, save Wm. Gaskins, on whom she waited during his illness.

April 9th, 10 A. M. Was taken with vomiting of bilious matter, and severe pain in her head. Saw her in the following condition at 7 P. M.—Eyes very red and muddy; lips had a fuliginous coating; was tormented with thirst, which no amount

of water could assuage; pulse 130, and rather compressible; rose-colored spots upon the arms, chest and thighs—also ecchymoses as large as a five and ten cent piece; at times delirious. This symptom appeared in all immediately after vomiting or purging. Cold water to head, and waited two hours to determine the propriety of bleeding. I had been advised to bleed, and contrary to my prepossession did so to the extent of 14 ounces—after which she soon complained of cold feet. I gave her brandy directly after the blood was drawn. There was no syncope, nor a tendency thereto. To avoid evil results, I adopted all the necessary precautions, and gave, in addition to the above, tr. opii. camphorata, ʒij.: applied sinapisms along the whole spine, and warm bricks to feet. Within one hour and twenty minutes of venesection she was a corpse.

Autopsy fifteen hours after death.—We were permitted, or rather implored, to examine but the brain. The pia mater and arachnoid were deeply injected over the whole surface; the veins were filled with black blood, and so numerous were the anastomoses of the arteries of the pia mater and arachnoid that the surface of the brain appeared of a rusty mahogany color; the membranes were shrivelled and dry; the ventricles contained no fluid whatever; the substance of the brain was very soft, and putrefaction far advanced. The integuments on the whole posterior surface of the body were of a thick, dark brown color.

CASE VIII. James Brown, son of E. G. Brown, age 15; previous health good: had visited the sick, and was taken ill in the morning of the 9th, at the same time as Olivia. 11 A. M. Complained of soreness of muscles and joints, headache and vomiting. 7 P. M. Pulse 100, full and compressible; rose-colored spots over the whole body. Gave the following every three hours: Sp. am. aromat. gtt. xv.; ext. belladonna gr. $\frac{1}{3}$; pulv. camphoræ gr. iii. Sinapisms along the spine. There being a muddy redness of the eyes, and some thirst, ordered abstinence from drink, for reasons hereafter to be stated.

11th, 10 A. M. Very much improved; blotches more abundant and dark colored; there is great soreness of limbs. Pulv.

ipecac. co. gr. v. ; sp. am. aromat. gtt. xv. et ext. belladonna, every eight hours.

12th, 11 A. M. Still improving, with less fever. Gave pulv. camphoræ gr. iii. every eight hours; mucilage with ol. ricini. ʒj. ; and sp. tereb. gtt. xv. Within a few minutes two large serous operations. He then complained of headache, and asked what should be done for it. Ordered, cold cloths to forehead, and sinapisms to abdomen, with tr. opii. c. 3ss. to be repeated until the operations ceased. On the next day I visited him in company with Dr. Bruce, and found him profoundly comatose: deglutition abolished; face swollen and suffused with a dark red; had had several spasms during the day, and the left side was drawn towards the right; the body is covered with dark brown blotches from the size of a small shot to a five cent piece. He died at 9 A. M. on the 15th. Throughout his illness, the soreness increased, together with redness and swelling of the joints.

CASE IX. Melissa Brown, (sister of the above,) age 5 years, was taken on the 9th of April, at midnight, and within one half hour from apparent health, her pulse was vacillating and compressible; no headache. There were rose-colored blotches over her arms and thighs. Aromatic sp. of ammonia, together with camphor and belladonna, were given. The blotches continued to increase in number, and grew darker, until scarcely an intervening space was left. Quin. sulph. and wine were given, and a decoction of red oak bark applied over her body. Beneath the integuments of the neck and armpits there was a dark suffusion of blood; the countenance was cadaverous and shrunk; the pulse fluttering, and the slightest pressure would stop it—it required a nice touch to distinguish the systole from the diastole. All these symptoms continued to increase up to the morning of the 13th. I then with confidence addressed myself with a determination to rely on tr. opii. alone, and that in as large quantities as she could bear, believing that had the others been treated with full and free doses more would have been saved. Ordered, tr. opii. gtt. viij., and in four hours, tr. opii. gtt. x. Immediately after the first dose all the unpleasant symptoms disappeared, and she continued to

steadily improve, with the addition of quin. sulph. gr. ii., and wine $\frac{3}{4}$ ss. *ter die*.

CASE X. Mrs Brown, age 36, complained of being chilly on the evening of the 13th. There were small petechiæ upon her arms—they were very dark. She firmly believed she had the disease, and would die. Gave her morph. sulph. gr. $\frac{1}{2}$; wine and quin. sulph. the next morning. Chilly and very much alarmed. There were several dark colored spots upon her legs—those upon her arms had not increased; some headache. This lady had lost a mother, brother, brother-in-law, an orphan girl, and a son, by the same disease. She verily believed she should follow them. Grief—despondency were the circumstances against her, together with two weeks watching and constant nursing. A lady of remarkable kindness, and whose solicitude knew no bounds. I therefore knew that I had an exhausted nervous system to contend with. Consequently, gave tr. opii. gtt. lx. every twelve hours; quin. sulph. gr. iii. and wine every three hours. There ensued no further difficulty.

CASE XI. and XII. Were treated by stimulants and quin. some days before rose-colored spots appeared. They were not confined to their beds. I, myself, had undoubtedly rose-colored spots over my arms, abdomen and thighs. As a prophylactic I trusted to opium, stimulants and quinine, and have been able to attend to my professional duties without the slightest inconvenience.

The character of this disease was so new to us, that we were entirely unprepared to anticipate its rapid progress—its mode of attack varying in almost every case. We were likewise ignorant of what the next twelve or twenty-four hours would unfold. Trusting to general principles was at best but a slim guide. We could not determine *à priori*; therefore some extenuation will be allowed—*à inferiori*. We can look through the past, and see our errors. I can now see that bleeding would have been pernicious—it must have been so. Even cupping, when the patients would not bear the slightest purgative without a manifest tendency downwards. Stimulants, alone, could

not be relied on; sinapisms down the spine were beneficial; and, above all, opium in full and free doses.—Wherefore? Whatever may be the poison which operated with so much power and certainty, its primary effects must have been through the medium of the blood, directly upon the organic nervous system. The three great nervous divisions—the Motory, Sensory and Respiratory—were the last to experience its effects; but those concerned in the office of reproduction and depuration were the first to fail. The tissues and capillaries lost their normal rhythm. The desire for fluids was intolerable, and the more it was indulged, the greater was the thirst. Vomiting occurred in all the fatal cases, resulting, no doubt, from cerebral disturbance. The period of reaction, instead of being followed by a benign sweat, was terminated by congestion. The congestion was evidently of two kinds—one sanguineous, and the other serous. I know not whether this distinction has been made by writers: I shall, however, venture upon its adoption, and account for it accordingly. When the organic nervous system has lost its tone partially, semi reaction follows. The capillary system being already weakened, is unable to bear the tension required for the increased volume of blood: it is nevertheless injected, and during its passage the tissues transmit the smaller globules of serum through their walls. A removal still farther, and the organic nerves cease to supply elasticity, and we have, together with serum, the red globules or coloring matter of the blood; from whence issue infiltration, especially in the lungs—a mechanism of exquisite capillary formation. The inflammation was asthenic, and that in proportion to the gravity of the disease. Whenever a septic influence operates upon an individual, the phenomena will be proportionate to its power and susceptibility.

Permit me, whilst with timidity I call your attention to a class of diseases, little understood, which sweeps off, annually, thousands of our citizens. I do this with a view to elicit from the profession that information which every Southern practitioner ought to have, and no doubt desires.

For many years, during my first practice, I heard of Bilious Pneumonia and Typhoid Pneumonia committing great havoc in certain districts of this State. I must confess I was at a loss

to understand how these diseases could kill in the short space of one or two days. These terms, I find, are used for any violent disease, either of the brain or lungs. All means generally proved abortive; and physicians, skilful in the treatment of other diseases, were, and are (the majority) totally unprepared to form a platform on which they might rely for a guide. It is the prevailing opinion, that each season has its simple type of disease, and that these types generally follow in regular order, is granted to some extent. There is a constitutional cycle, continually changing our atmosphere, producing thereby a dysentery, amenable to purgatives one year, to tonics another, and to astringents, alteratives, &c. This is the pregnant source of Doctors disagreeing. Medicine is a science—a pure science; but vastly more complex than all others. When we look through the lists of our venerable fathers' times, ought we to throw all their learning aside, and lay their experience upon the shelf. Through their records we learn the ever-varying faith in remedies. For a season, one drug has proved unvarying in its remedial qualities—it has borne the title of specific and bothered to gainsay. Anon, it falls into other hands, and times, and is found useless—aye, inert.

We are startled by the conviction that coincidences have had so much to do in the matter. In the science of Quantity, men do not vary so widely from the truth. From one to a thousand start in the demonstration of a mathematical problem, and arrive at the same conclusion. But place these same men upon the demonstration of a problem in Quality—their conclusions are not alike. Wherefore? The bases are all different: Climate—the physical qualities of soil—moral and hygienic influences are equally opposite. In the annual and periodic evolutions of atmospheric and aqueous conditions, no two seasons are precisely alike. At one time we have an easy road to travel, when referring to authors for our guide in the treatment of disease—we are chained by the lucid and exact detail. But again we are foiled when trusting to the same source. To illustrate: The exanthemas, especially those commonly known by the name of measles, scarlatina, &c., have had their specifics; and well might they, when yielding, from the simplicity of their type, to almost any remedy, right or wrong. The

very simplest of all combinations were then prevailing—hence the benign inflammatory action.

We are told in the books, that inflammation is manifested by certain signs and terminations; that an incised wound heals by the first intention, &c. True, so far as they go. But there is one department which has not been properly investigated—viz., the constitutional combinations, which may convert this simple inflammation into the more complex varieties. A cause sufficient to produce inflammation of the viscera in London, Stockholm, &c., may give rise to the phenomena recognised in those places; but when superadded to this, we have an individual exposed to the above, with a powerful concentration of malaria, in a malarious district, and his system saturated with the latter, it would be marvellous did we recognize the same symptoms as in the former.

To add force to my deductions, I will here call your mind to the fact of erysipelas prevailing as an epidemic in the wards of hospitals. Let us now take an individual who has suffered the simplest wound in a pure atmosphere, and there allow the healing process to be partially completed—carry him from thence to the infected ward, and the same wound is converted into one of a much more serious character: it is erysipelatous. Again, under analogous circumstances, let us carry a case of pneumonia into wards where malignant typhus prevails, will it not become typhoid pneumonia. By a parity of reasoning, I will now refer you to those seasons when bilious and typhoid pneumonia, together with asthenic inflammations, prevail. We are too apt to consider that, upon the general disappearance of intermittents and remittents, after frost, the malaria which produced these diseases is also abolished therewith. A knowledge of facts will convince us to the contrary, especially when we are credibly informed by the Swedish physicians that intermittents prevail so high as the latitude of 62° North. Now it must be evident that our thermometer will stand as high, and higher, in many of our winter days, as in the Northern parts of Sweden. In our locality, a physician of ordinary skill, and the plainest routine, succeeds in curing every case of pneumonia. All that he has to do is to give his patients a little soup and antimony, and the lucky dogs get well. Step a few

miles farther, and your learned Doctor—your *theorist*, is mortified by his signal failures. Can it be possible that, as a general rule, antimony, given under precisely similar circumstances in the same disease, should fail in the hands of the learned in twenty cases—whereas it certainly succeeds in the hands of the ignorant practitioner in a like number? Certainly it would be the greatest absurdity to deem it true. I will, in my mind, offer you a more rational ground for the discrepancies which are so constantly witnessed. When the variations in temperature alternate with great changes from low to high, and that in a country where intermittents, &c., are endemic, we shall witness corresponding changes in the type of disease. When an individual whose system is saturated with a malaria capable of producing a benign quotidian, tertian, or quartan, or a malignant quotidian, &c., and he is moreover laboring under the exciting causes of pneumonia, phrenitis, &c., we shall not witness phenomena such as result from the above named diseases separately; instead thereof, will be found the types blended together—to wit: a congestion, or cerebro-spinal meningitis, coupled with pneumonia in its simple form. Again, there may be pneumonia associated with gastritis, or gastro-enteritis, together with hepatic derangement, giving rise to the common phraseology—complicated pneumonia, congestive pneumonia, congestive bronchitis. As a proof of this fact, we have only to treat any of the above forms for simple pneumonia, or the like, and we very soon find we have to change our course or give up the ship. Let us now remove the complication, by setting aside the one which most readily yields—to wit: the intermittent, with quinine, opium, &c., and if thereafter is found a pneumonia, it will yield to the ordinary remedies when judiciously administered; or if you please to let it alone, it will in many instances get well of itself; but this is trusting too much to nature, and giving the chance to the umpire of matter. Again, whenever the malarial influence is so strong as to prostrate the organic nervous system, and associated therewith inflammation of any organ, there will be asthenic inflammation instead of the sthenic.

Let us now, in another season, have diseases of a typhoid type associated with a malarial influence—they will be respectively still more complex and vastly more rapid.

Shall it be Dysentery, thus characterized, prevailing as an endemic, or epidemic? How sorely puzzling will be its forms!—how devastating its influence!!

Have we not seen the Algid typhus in the negro, with his cold icy body; and when asked how he is, have we not heard, and despaired, from the laconic answer—"better!" But outwardly we behold marasmus, sure and steady as clock work. Thirst, there is none; appetite none; instincts are almost abolished, together with the moral and physical impulses. Prostrate he lies upon his back, more careless than incapable of motion—intellect torpid, but when roused, clear. There is profound apathy, with alternate transitions to the quotidian, tertian or quartan types. Thus do we see the crises of the 7th, 14th, 21st and 28th days; and by marking the periods (odd or even days) when the patients are better or worse, we may with more certainty base our prognosis.

It has been our lot, within the last three months, to be visited by that peculiar form of inflammation which has swept off thirty or more of our citizens, within the space of from three to forty-eight hours, and that within a radius of seven miles. By some, the disease was called Typhoid Pneumonia—by others, Bilious Pneumonia. But, in fact, it has been in some an asthenic inflammation of the bronchi—and in others, of the lungs, brain and pharynx. The sanguine, robust, and plethoric, who were susceptible of malarial influences, were mainly its victims. When the disease was of a concentrated form, or fully developed, purgatives, if not inoperative, were sure to aggravate. Again, a class of apparently the same disease yielded immediately to the influence of senna, manna and salts. It was astonishing to see how rapidly the integuments, beneath the eye and upon one side of the face, would become suffused with dark coagulated blood. One case I shall briefly notice.

Rogers, a boy, 9 years of age, previously healthy, possessing a fine sanguine temperament—was taken in February, at midnight, with slight indisposition. In the morning, 8 o'clock, there was still but little ailment—was disposed to be drowsy; face flushed, not much; pulse 90, compressible. Gave senna, manna and ginger tea, together with quin. sulph. and hyd. sub. m. When seen next morning all the unpleasant symptoms had

vanished; beneath the right eye there was a bruised appearance the size of a dollar. The conjunctiva at first was injected, but had disappeared.

During the last two or three years there has been typhoid fever associated with intermittents, and for the last five months the mortality in our section has vastly increased. Carbuncles and melanosis have been no strangers.

Now, whatever has been the influence which has produced so many malignant features, of this one effect we are certain: that we have, in many instances, to deal with an intense asthenic inflammation, which speedily terminates in effusion or in sanguineous infiltration. During the last two months of the last fall, almost all of our intermittents were double tertians, and the greater proportion of these were malignants—instead of postponed, they were anticipated double tertian. In some, there was a marked semi-reaction, very difficult to determine—in others, a combination of typhoid and congestive symptoms. Again, the pure algid and congestive varieties. Now, we have had nearly all of the above forms in the present spotted fever.

It would afford me pleasure to continue this subject. Least I weary your patience, I will briefly allude to the character of inflammation.

There is no doubt that we have been altogether remiss in our investigations with regard to this disturbing force. Disturbing, I call it—for on the one part, nature may be too energetic in restoring an equilibrium; on the other, she may be altogether deficient. In the former, we have the simple benign plastic qualities of an over excited sanguiferous system, in which the potent lancet is truly, and beyond all doubt, the *omnis omne*. But, in the latter the vis vitæ no longer yields the plastic lymph. Let us now marshal our forces, as in the above, and what do we behold: an account soon settled—a patient in articulo mortis.

ARTICLE XXI.

Statistics of Mortality in Augusta, Georgia, during the years 1848, '49, '50, '51 and 52. Arranged by H. ROSSIGNOL, M.D., of Augusta.

The following Tables have been made from the books of the City Sexton, and may be *relied* upon as to *numbers*; but as relates to *ages* and the immediate *causes* of death, I will not vouch for their correctness, as I noticed some errors in these respects in looking over the books.

Similar tables having been published in this Journal in the 1st volume (old series), p. 650—the 3d vol. (old series), p. 648, and the 4th vol. (new series), p. 658, it will be seen that these, when added to them, will complete the vital Statistics of the City of Augusta for a period of thirty-five years, and that they contain valuable information, notwithstanding the inaccuracies alluded to.

TABLE No. 1.

Census of Augusta, taken by order of the City Council, October, 1852.

WHITES.		WHITES.		FREE NEGROES.	SLAVES.		TOTAL.	GRAND TOTAL.
Males.	Females.	Males between 6 and 16	Females between 6 and 15		Males.	Females.		
2779	2177	770	766	243	2330	2388	11,753	
Transient or temporary residents—white.							400	
Free Negroes and Slaves, not returned.							400	
Business and Suburb population.							1,519	14,072

TABLE No. 2.—*Deaths in each year.*

In preparing this table, the still-born are included, as it was deemed unnecessary to make a separate table for them.

YEAR.	WHITES.	BLACKS.	TOTAL.
1848	104	120	224
1849	105	109	214
1850	168	147	315
1851	184	133	322
1852	230	147	377

TABLE NO. 3.

Deaths in each month—the Still-born included.

Year.	Month.	Whites.	Blacks.	M. Total.	A. Total.	Year.	Month.	Whites.	Blacks.	M. Total.	A. Total.
1848.	Jan.	8	10	18	224	1851.	Jan.	10	13	23	322
	Feb.	4	9	13			Feb.	8	9	17	
	March,	7	7	14			March,	16	11	27	
	April,	9	10	19			April,	9	8	17	
	May,	9	6	15			May,	19	10	29	
	June,	13	9	22			June,	15	13	28	
	July,	11	7	18			July,	23	16	39	
	August,	5	19	24			August,	27	14	41	
	Sept.	3	6	9			Sept.	19	9	28	
	October,	14	16	30			October,	10	13	23	
	Nov.	9	13	22			Nov.	14	12	26	
	Dec.	12	8	20				Dec.	14	10	
1849.	Jan.	8	9	17	214	1852.	Jan.	11	5	16	377
	Feb.	3	9	12			Feb.	10	16	26	
	March,	6	5	11			March,	16	12	28	
	April,	8	11	19			April,	17	10	27	
	May,	7	15	22			May,	28	17	45	
	June,	10	16	26			June,	24	19	43	
	July,	17	11	28			July,	28	19	47	
	August,	10	10	20			August,	20	12	32	
	Sept.	12	7	19			Sept.	10	5	15	
	October,	10	3	13			October,	24	9	33	
	Nov.	8	9	17			Nov.	23	15	38	
	Dec.	6	4	10				Dec.	19	8	
1850.	Jan.	12	7	19	315						
	Feb.	12	13	25							
	March,	10	12	22							
	April,	14	13	27							
	May,	15	12	27							
	June,	19	20	39							
	July,	17	9	26							
	August,	10	10	20							
	Sept.	6	15	21							
	October,	20	14	34							
	Nov.	15	12	27							
	Dec.	18	10	28							

TABLE NO. 4.

Number of deaths in each month, during a period of five years, with averages deduced from the column of whites, from that of blacks, and from the total.

	Whites.	Blacks.	Total.	White average.	Black average.	Total average.
January,	49	44	93	9.80	8.80	18.60
February,	37	56	93	7.40	6.98	18.60
March,	55	47	102	11.00	8.14	20.40
April,	57	52	109	11.40	10.40	21.80
May,	78	60	138	14.16	12.00	26.16
June,	81	77	158	16.20	15.40	31.60
July,	96	62	158	19.20	12.40	31.60
August,	72	65	137	14.40	13.00	26.14
September,	50	42	92	10.00	8.40	18.04
October,	78	55	133	14.16	11.00	30.06
November,	69	61	130	13.80	12.20	26.00
December,	69	40	109	13.80	8.00	21.80

TABLE NO. 5.

Average number of deaths per annum, during five years among the whites and blacks, at different ages.

WHITES.		BLACKS.	
Still-born,	10.75	Still-born,	50.50
Under 5 years,	66.04	Under 5 years,	55.08
From 5 to 10 years,	81.02	From 5 to 10 years,	50.60
“ 10 to 20 “	10.60	“ 10 to 20 “	80.40
“ 20 to 30 “	18.08	“ 20 to 30 “	11.06
“ 30 to 40 “	13.02	“ 30 to 40 “	61.00
“ 40 to 50 “	13.06	“ 40 to 50 “	80.20
“ 50 to 60 “	60.12	“ 50 to 60 “	40.80
“ 60 to 70 “	41.02	“ 60 to 70 “	10.20
“ 70 to 80 “	30.40	“ 70 to 80 “	10.00
Over 80 years,	1.00	Over 80 years,	60.20

It will be observed, in the above table, that a much larger number of blacks are put down as living beyond 80 years, than whites. This can easily be accounted for by the well known fact, that negroes, generally, are ignorant of their age, and are particularly fond of living to a “good old age,” which they affect by adding on as many years as suits their taste.

TABLE No. 6.

Ages of those who died in the years 1848, 1849, 1850, 1851, 1852.

Whites.	Still born.	Under 5 years.	From 5 to 10.	10 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	Over 80.	Total.
1848	6	49	4	4	9	8	10	6	5	2	1	104
1849	unknown.	39	6	8	15	9	11	7	5	3	2	105
1850	6	77	16	11	12	12	12	6	9	7		168
1851	16	76	8	10	24	17	16	8	7	2		184
1852	15	91	12	20	34	20	19	10	4	3	2	230
	43	332	46	53	94	66	68	37	26	17	5	791
Blacks.												
1848	8	44	8	8	9	6	3	8	9	12	5	120
1849	unknown.	41	6	4	10	4	13	4	12	10	5	109
1850	3	59	5	10	14	13	12	4	12	10	5	147
1851	5	67	3	10	12	4	7	6	8	10	6	138
1852	6	68	6	10	13	8	6	2	10	8	10	147
	22	279	28	42	58	35	41	24	51	50	31	661

TABLE NO. 7.

Causes of Death, as recorded by the City Sexton.

Very little, if any reliance, can be placed on this table, as the Sexton rarely derived his information from those who were capable of giving it. In making this table it was thought best to give the names of the diseases as found recorded, without any attempt at classification, as that would have been an endless undertaking.

	WHITES.					BLACKS.				
	1848.	1849.	1850.	1851.	1852.	1848.	1849.	1850.	1851.	1852.
Sundries,	2	7	4	14	2	9	7	9	5	8
Casualties,	4	5	4	5	5	3	3	8	6	2
Mortification,				1	1			1		1
Old Age,	3	2		1	1	6	3	6	8	9
Rheumatism,		3		1	1	1			1	2
Diarrhœa,	7	3	2	3	2	1		2		
Brain Fever,		1	1	2			4	1	1	1
Croup,	3	1	3			2	1			2
Dysentery,			5	26	46		1	1	22	25
Cholera infantum,	4	21	14	5	5	6	14	9	4	
Affections of Lungs,	1	1	1	1	1		2			
Fever,	4	4			4	2	4	5	2	1
Still-born,	6		4	15	14	8		3	4	4
Small-pox,			1	6					3	
Pneumonia,	9	16	15	14	22	11	17	18	15	14
Affections of Liver,			1		1	1	1			
Tetanus,		1					1		8	
Intemperance,	1	1	2	1	3		2			1
Fits, Spasms & Cramps,	9	3	20	9	14	9	12	17	17	13
Pertussis,				3				1		
Inflamed Bowels,	7	3	9	4	9	3	1	2	2	7
Erysipelas,			1	1	2			2		
Cholic,	1	1	2		2	3		2		1
Palsy,	1		1	2		1	1	1	1	
Worms,		3	1	2	1	2	3	6		2
Dropsy,	4	2	3	3	3	5	11	6	6	9
Measles,	6	2			13	1				2
Typhoid Fever,	3	6	2	15	9	1	6	1	6	6
Child-bed,	1		2	2	4			5	1	1

[Continued on next page.]

TABLE No. 7, (concluded.)

	WHITES.					BLACKS.				
	1848.	1849.	1850.	1851.	1852.	1848.	1849.	1850.	1851.	1852.
Apoplexy,	2	1	2	6			2	2	1	
Dropsy of Brain,	2				1				1	
Inflammation of Brain,	1		2	3	5	2		5	2	1
Scarlet Fever,		1	34	8	6			10		3
Consumption,	6	8	8	13	15	12	5	3	5	8
Ulcerated Sore Throat,	8		1			13		1		
Jaundice,	1					1				
Congestive Fever,			5		8			3		3
Congestion of Brain,			7	3	1			2		3
Dropsy of Chest,				1				2	2	1
Hemorrhage,			2			1		2		
Cancer,		2	2		1	4	1	2		1
Syphilis,			1							
Debility,	2		2	1	3	1		2	2	3
Hernia,	1									
Paralysis,		1				1	1	1	2	1
Metritis,		1					3			
Disease of Heart,		2			6			2	4	4
Dyspepsia,			1		1			1		
Suicide,				2	1					
Invagination of Bowels,					1					
Stricture,					1					
Diseases unknown,	5	3	4	10	15	10	3	3	7	8
Diseases stated,	99	102	164	174	215	110	106	144	131	139
Whole No. of Deaths,	104	105	168	184	230	120	109	147	138	147

TABLE No. 8.

Relative proportion of the Sexes of those who died.

In 1848, died (Whites)	Males,	59	Females,	45
" 1849, " "	"	56	"	49
" 1850, " "	"	94	"	74
" 1851, " "	"	103	"	81
" 1852, " "	"	119	"	111
In 1848, died (Blacks)	Males,	54	Females,	66
" 1849, " "	"	58	"	51
" 1850, " "	"	78	"	69
" 1851, " "	"	74	"	64
" 1852, " "	"	69	"	78

Eclectic Department.

Letters upon Syphilis. Addressed to the Editor of L'Union Medicale, by P. RICORD. Translated from the French, by D. D. SLADE, M. D.

[Continued from Page 278.]

TWELFTH LETTER.

My Dear Friend—Does there exist any real difference between the natural and the artificial contagion? This is the subject of our conference.

The observation and rigorous analysis of facts demonstrate to those who do not suffer themselves to be led away either by prejudice, or by preconceived ideas, that the contagion of syphilis, under whatever circumstance it may operate, is reduced in the final analysis to a process of inoculation more or less analogous to the process by the lancet. The lancet, in fact, inoculates the accident (the chancre) which by the confession of all is the most fatally contagious. It is by this accident, by the chancre, according to observations well made and *collected in the proper time*, that syphilis commences.

Laying aside artificial inoculation, the chancre is seen to develop itself everywhere upon the surface of the body without choice of seat, and upon all the external or internal integument, which is accessible, and by consequence, without there being need either for the parts which are infected, or for those which furnish the infecting matter, of special functions or of any particular physiological condition. Other conditions are necessary for contagion.

Examine with care all the parts which are affected, you will find that it is those which present the most favorable conditions for mechanical lesions, for scratches, for lacerations, and for solutions of continuity of every kind; you will find, also, that it is there where voluminous and numerous follicles exist into which the virulent matter can introduce itself, that the accident is by preference developed.

Is it not true that in the male it is more particularly the border of the prepuce, especially when there is a phymosis more or less pronounced, the neighborhood of the frænum, the adherent points of the semi-mucous surface of the gland and of the prepuce, points which not having the suppleness of other regions are more easily torn, that by preference become infected by the contagion; in the female the fourchette, the points of insertion of the nymphæ, the carunculæ myrtiformes, are the parts which most easily take on the contagion. In the other regions, is it not true that it is when excoriations exist that the

contagion is established? Thus, an excoriation upon the finger is often the door where syphilis can enter. But the presence of an excoriation is absolutely indispensable. If it was otherwise, should I ever go out of the hospital without having a chancre at the end of each of my ten fingers? The chancre often appears upon the lips, but the lips are almost always cracked; pleasure excites the smiles, and smiling extends and dilates the lips. The nipples of nurses are often the seat of chancre, but these parts are ordinarily cracked and torn. The chancre seats itself everywhere where there has been a cicatrice, but there also there is a loss of suppleness, and consequently cracks and lacerations are easy.

In all this, my friend, you see nothing which is, as they say, physiological, which exacts special vital conditions, a particular state of organism and the exercise of any function whatsoever. All this, for you, as for myself, is reduced to a traumatic and and mechanical phenomenon.

Practice, that criterion of all doctrines, justifies, alas, too often my doctrine. Nothing is more common than to see the physiological act of generation rest indemnified from every unhappy consequence, while other acts which have nothing in them physiological, draw after them painful results. The genital organs, the seat so special of syphilitic affections, do not always take the infection from genital organs. It is not always the genital act properly called, which becomes the infecting cause. Coitus does not become an infecting act unless certain material circumstances come into play. Among the innumerable examples which I could cite for the support of my opinion, I ask permission to cite to you two, which have struck me more, inasmuch as they presented themselves to me suddenly upon the same day. There is no physician who does not know that there are some singular days, when curious facts arrive as if in series.

A gentleman brought me one day his mistress, whom he had infected, and in a manner which much astonished him. He had upon the penis a primary ulcer at the period of specific progress. He had had normal intercourse with his mistress, and in the same night intercourse more to be blamed, *à prepos-tera venere*. The lawful intercourse had been much more frequent than the other. The woman presented absolutely nothing suspicious upon the genital organs, but she had a chancre on the anus. What did this mean? That the physiological and natural passages had yielded without laceration, and had escaped contagion, while the abnormal passages, more resisting, were torn and became infected.

Here is another couple. Here, again, is a contest between a

physiological act, and a prelude which does not belong to the human species, a prelude which is not at least placed among the genital functions of man. A gentleman surprised at seeing a suspicious bud pushing forth upon one of his lips (bud without a flower, as Jean Lemaire would have called it,) without any disease of the genital organs, comes to ask me to examine the woman with whom he had had intercourse. I found upon this woman a chancre at the specific period, situated in the neighborhood of the meatus urinarius. This gentleman had had rather frequent sexual intercourse with this woman during the same night, during which he had gone astray so far as to sadly expose his lips. It is necessary to add that this gentleman was very subject to chapped lips, and that all this passed in winter.

These facts, which I could multiply, prove that the physiological conditions of the genital act go for nothing in the contagion of syphilis. Thus the doctrine of physiologism finishes upon this point by falling to the ground. Be well convinced, that in spite of the most intimate contact, of the most complete fusion, and of the orgasm the most voluptuous, with an entire skin and an irreproachable mucous surface, one can escape safe and sound from the most exposed intercourse. Be assured on the contrary, that a portion of skin torn, a mucous surface chafed, will render the slightest intercourse dangerous, and we physicians have a thousand precautions to take in this respect, and certainly our examinations are strict. We know, however, that the medical corps has furnished victims to the martyrology of syphilis, and that it was in the beneficent exercise of our art that the unfortunate Hourmann, and Delavacherie of Liège, found a death tediously frightful.

After what I have just told you, what can you think of the pretended physiological inoculation of my colleague M. Vidal, as regards blennorrhagia? You know when and how this latter is really inoculated by the lancet. It is then, and only then, when it proceeds from a chancre, and it is the rarest case, as M. Vidal agrees with me. But in other conditions in which blennorrhagia is produced, is there, physiologically and pathologically speaking, anything which resembles the contagion of chancre? Do we even always know, as I have said, till tried, if the blennorrhagia is always due to a veritable contagion? And yet this condition of contagion has been considered as a proof of virulence, as a sort of physiological inoculation, which the lancet cannot produce. Here M. Baumès—it would seem that the successive contagions of blennorrhagia were his means of diagnosis, without telling us, nevertheless, how many times blennorrhagia ought to be produced in order to be

virulent. Thus one takes a blennorrhagia, he gives it to another, where commences the virulence? M. Baumès does not say. Suppose that a woman is suspected of having contracted a discharge from a suspicious man—if we should wish to assure ourselves upon the nature of the discharge of this woman, it would be necessary to hold an inquest, to run after the different sources of the blennorrhagia of the man, and to pursue it, going back even to the gonorrhœal flux of the Bible. Yes, but we should not have made one step in this inquiry, without finding ourselves in the presence of that most common difficulty, of two individuals having had commerce with the same woman, the one will have contracted a blennorrhagia, and the other not. For one, we should conclude upon the benignity of the blennorrhagia, and for the other upon its virulence. All this is not serious.

Facts and observation, then, do not indicate any difference, my friend, between the inoculation called physiological and the artificial. Let us now invoke analogy.

In every malady incontestably contagious we find that the traumatic conditions dominate, and that under ordinary circumstances art can repeat what nature does. Thus the vaccine inoculated does not differ from ordinary vaccine. The variola inoculated does not differ from the spontaneous variola. Thus with the glanders, the farcy, hydrophobia, malignant pustule, and hospital gangrene. This argument from analogy appears to me of incontestable value. Why should the syphilitic virus alone escape from the common rule?

But the chance, it has been said, is not the only contagious syphilitic accident. There are some secondary syphilitic accidents for which the lancet has not yet discovered the *contagium*. Science, in fact, contains a great number of observations which appear conclusive for a very large number of physicians, and which leave some doubt in the minds of many others. The numerous tubercles, or condylomata, are considered by a very large number of writers upon syphilis as contagious, and by consequence can be transmitted.

When I have studied this accident by means of inoculation, considering well all the circumstances which could prevent error, the experiments have always been negative. However, other observers have obtained contrary results. I can only answer for this exception by stating the result of my own experience.

I inoculated with the pus of numerous tubercles coming from the neighborhood of the vulva of a young girl of Versailles, who entertained habitual intercourse with the garrison of the place, and I obtained a positive result. Much astonished, I examined

with more care the surfaces from which I had taken the pus, and it was then easy for me to recognize that among the numerous tubercles, there existed a chancre still at the period of specific progress. Then, some new inoculations being made comparatively with the pus taken upon this ulceration, and with the matter of the mucous tubercles at a distance, the pus of the chancre gave the characteristic pustule, and the muco-purulent secretion of the mucous tubercles remained without result. This experiment appeared to me decisive.

In the observations which have been cited of mucous tubercles which have communicated syphilitic accidents—the period which has passed between the time of observing the patient and the infecting coitus has not been taken into account. It is always three weeks, a month, two months or even more after the contagion, that the patients present themselves to the physician, so that not only the real form of the commencement is wanting, but still it is impossible to determine the true nature of the accident which has been the source of the contagion. Some individuals forget, and others do not know, that by a succession of changes easy to observe, where one takes the pains, the primitive accident (the chancre) passes *in situ* from the state of an *organ* of virulence to the conditions of a secondary accident, not furnishing any more specific pus. Where are the observations of persons seen with mucous tubercles, who have transmitted the disease to another person which could be observed the second or third day after the infecting coitus, and in whom the disease has commenced as we see it commence after the contagion from a chancre? Does the disease in this case commence with the chancre, or with the mucous tubercle? There is not one single incontestable fact which can answer this question. The facts, however, do not fail as regards mucous tubercles. As to myself, I possess very numerous observations of well-characterized mucous tubercles upon men and women, which prove that the patients thus affected could indulge in frequent sexual intercourse without communicating anything. Among all these facts here is one, my friend, which will remain deeply impressed in the minds of my readers, as it has in my own.

A gentleman whom I had attended for a chancre two years before, was about to marry. Before his marriage he came to see me again, in order to submit himself to a rigorous examination. I found him in the best state of health; he could be married without any scruples. However, this gentleman, a very strict man, exacted of me another examination the very evening of his marriage. I still found him perfectly exempt from every accident, and I delivered to him my bill of health

as clean as possible. One month after, he sent for me. My dear doctor, he said, my wife has some large pimples upon her which troubles her very much. See what it can be. Before passing into the chamber of the wife, I proceeded to a new examination of the husband. I found him in as healthy a state as the day of his nuptials.

But it was not the same with his wife. I found some confluent and well-developed mucous tubercles, so as to assure me that the point of departure of the accidents was anterior to the marriage.

Convinced that the husband had nothing to do with this sad affair, and that he could not communicate a disease he did not have, I said to the wife in a firm and decided tone—Madam, you are diseased, and it is not your husband that has rendered you so. If I become your confidant, I become also your accomplice; in the contrary case, I shall remain the physician of your husband. I was not long in obtaining a painful confession, which gave me the key to this unhappy enigma.

I recount to you this fact because it contains this which is interesting, viz., that since marriage the husband had not passed two days without having repeated intercourse with his wife, and notwithstanding, he had absolutely no disease.

I have not finished with the mucous tubercles; permit me to return to them in my next letter.

THIRTEENTH LETTER.

My Dear Friend,—I return to the *mucous tubercle*. As you know, this accident, with many writers upon syphilis, is contagious. Among the proofs invoked for the support of this opinion, we must note that which considers as a result of contagion the successive development of these *mucous tubercles* upon those portions of the skin which are contiguous to those where the accident first developed itself. Thus, patients are seen who have these mucous tubercles upon the sides of the scrotum. Do they develop themselves upon the inner portion of the thighs—contagion! cry out the partisans of this opinion. If upon one side of the anus these tubercles gain the opposite side—contagion! they again cry, and so on. Those of my brethren who profess this doctrine, and there are among them some very distinguished ones, forget one little circumstance; to hold in consideration the cause which has produced the first tubercles; that is to say, the state of the constitutional infection under which the patient labors, a condition which can cause a second and a third tubercle to put forth, for they do not all appear at the same time. The consideration of the seat of

preference of these tubercles cannot in any way aid the doctrine of contagion; in fact, if there is a contiguity in the parts of the skin where these tubercles appear, we must also remark that the acrid secretions are more active: that the skin, in these places, has a tendency to the mucous transformation, as in the neighborhood of the genital organs, of the anus, &c. How can we explain, moreover, by contagion, the development of these mucous tubercles from one arm-pit to the other?

I shall remain, then, always convinced, until proof to the contrary arrives, that when some have thought to have seen *mucous tubercles* contagious, while they have admitted that they could be primary accidents, they have erred in diagnosis. I do not think it useless to call to mind that *the chancre, at the period of reparation*, often assumes, in granulating, the aspect of mucous tubercles, that it can undergo sometimes a veritable metamorphosis, and become *in situ* a secondary accident, the physiognomy and the nature of which are those of mucous tubercles. If we have not been witnesses of its commencement, if we neglect to invoke the testimony of the neighboring glands, the remains of the margin of the ulcer and the characters of its base could have been so modified, that the differential diagnosis would be very difficult to make, especially for inattentive eyes and for fingers little skilled. Add to this certain particular seats where the primary accidents are not usually observed, and where also the transformation of the chancre is more easy, and more rapid, as upon the lips, upon the tongue, upon the nipples, and you will see how easy it is to be deceived.

All those veroles, transmitted by kisses more or less lascivious by the utensils of the table, by pipes, razors, masks, &c., have no other origin. And how many times have not these circumstances been *honest* pretexts for dissimulating other contracts! The mask, moreover, has been from all time, and in our day still a very convenient article for dissimulating a compromising diagnosis.

Even in certain religious practices, my friend, they have sought proofs of secondary contagion; thus they have arranged in this category the syphilitic accidents transmitted to infants by the process of the Hebrew circumcision. But these accidents find their natural explanation in the presence of the primary accidents in the mouth of the circumcisers. Let me here be permitted to say that I am one of those who have most contributed to cause the ancient and dangerous practice of the suction to be rejected by the Israelite Consistoire of Paris.

Many physicians will not absolutely take into consideration the facility with which the chancre passes into the secondary state; they occupy themselves only with its seat; and when

they see a chancre in the mouth, they are induced to consider it, from this fact alone, as a secondary accident. Herein lies a grave error of observation ; this gives me occasion to say that the primary ulcers become much more frequent in the mouth than in the anus. I meet with these last much less frequently, both in the hospital and in the city, than formerly. It appears to me that certain shameful practices diminish in frequency, and that there is progress in this respect in the public morality. However it may be, from the fact alone that a chancre is seated in the mouth, don't conclude that it is a secondary ulcer. Do not forget the famous genito-labial nerve invented by Voltaire, a spiritual pleasantry which must be sometimes considered as serious. I know a very distinguished brother physician, who has always remained convinced without other proof, that an ulcer of the cheek had been communicated to him by a *secondary kiss*.

If I have told you that I have often seen persons affected with different varieties of mucous tubercles upon the genital organs, who transmitted nothing in their sexual intercourse, I ought to tell you, also, that I have seen an equally large number with numerous tubercles upon the lips, upon the tongue, and upon the throat, who lived in family and who practised all the buccal contracts permitted, without ever transmitting anything. I know a gentleman of the neighborhood of Paris who had during six months, numerous tubercles upon the tongue and upon the lips, who has had with his mistress all possible intercourse, very negligent about his treatment, and who, convinced that the accidents which he had could not be contagious, has continued his intercourse without ever communicating anything.

It is, moreover, as regards the transmissibility of these secondary accidents from the nurse to the child, and *vice versa*, that this question becomes important. The fact of this transmissibility is generally admitted. Hunter has, however, denied it, and many serious observers partake of the opinion of Hunter. This question is so important that you should permit me to give it some development. It concerns public hygiene. It is often a question of legal medicine ; fraud, infidelity, cupidity, can be brought into action ; it is important, then, to guard against all the causes of error, and not to accept with readiness the stories of individuals who could have more or less interest in deceiving us.

If one consults the archives of science, if one searches for the basis upon which the opinion of the contagion of secondary accidents of syphilis from the nurse to the child, and reciprocally, rests, he is astonished at the little value of facts, and how

many grave men there are who are content with little. M. Bouchert, for example, in a memoir recently published (*Gazette Médicale*, 20 Avril, 1850.) has collected all the facts which have appeared to him the most positive. Well! read this book, interesting in other respects, and you will be convinced, like myself, that the greater part of these facts are not admissible, that the observations which appear the most probable are wanting in essential details, and are so incomplete that M. Bouchert is himself forced to confess it, to such a degree that he finishes by allowing that his conviction upon this point is more moral than scientific.

Here is what I myself have observed in this matter.

I have seen nurses and infants infected, who have been mutually accused of this infection; most generally I have succeeded in finding the point of departure, regular and inevitable, going back to a primary accident in one or the other. Sometimes I have met with merely simple coincidences. In those cases where it has not been possible for me to go back to the primary cause, I arrive too late; the children were not presented to me till five and six months or more after their being put to nurse. I have had, during several years, a ward of nurses at the *Hospital du Midi*. In this ward, I had often women affected with simple leucorrhœa, to whom I gave to nurse children sent to me from the *Maternité*, infected with secondary accidents, and never under my observation were these women infected.

On the other hand, nurses affected with very manifest secondary accidents have given the breast to infants sent to me as infected with syphilis, these latter having in reality nothing but simple eczematous, impetiginous eruptions, or species of *porri-go*, and never under my observation were these infants infected. My learned and industrious friend, Dr. Nonat, charged during a long time with the care of the nurses dependent upon the administration of hospitals, has arrived at the same results, and does not believe in the contagion of secondary accidents from nurses to children, and *vice versâ*.

In my private practice I have seen a great number of facts of this kind. Here is one of the most remarkable, which I observed together with my friend Dr. Chailly-Honoré. The subject of it was an infant born with hereditary syphilis, and in whom six weeks after birth, various accidents made their appearance, such as mucous tubercles of the ano-genital regions, humid scaly papulæ upon the trunk and upon the limbs, deep ulcerations upon the lower lip. This infant was given to a nurse upon the spot at the moment of its birth. We were able, both M. Chailly and myself, to observe the child as well as the nurse, during the eighteen months that the nursing continued.

The ulceration of the lip persisted during more than three months. This was scarcely cured, when in spite of a careful, methodical and continued treatment, a new ulceration manifested itself upon the velum palati, and resisted also during several months. Well, this nurse remained free from all infection; she enjoyed and enjoys still the most perfect health.

Surely, this is a fact well worthy of attention. I have just observed an analogous one, with my friend M. Bassereau. A child who, with other symptoms of hereditary syphilis, had ulcerations upon the lips, was nursed with entire impunity by its nurse.

You see my friend, how important it is, in the appreciation of similar facts, to hold in consideration all the conditions in which the nurse and child could be, if one did not wish to deceive or to be deceived.

The nurse, at the moment when she takes an infant, might be under the influence of a syphilitic diathesis which nothing yet indicates. I ought to say that in general when one takes a nurse, she is not submitted to a complete and absolute examination. I add that even when this is done, we could still be deceived, for the diathesis could exist when every trace of primitive or secondary accident had disappeared, especially in such a case as chancre upon the neck of the uterus. I ought still to add that the health of the foster-father is not always, alas! a sufficient guarantee. I have known for a long time how I should consider the pastoral maxims upon the pure manners of the country.

The child might be born with hereditary syphilis; child and nurse have nothing as yet apparent; but in some weeks or months we shall see secondary symptoms manifest themselves. These might appear in the infant before, during or after a similar manifestation in the nurse. So that the first in whom the manifestation shall take place, will accuse the other, if they do not both accuse each other at the same time, which frequently occurs. They are both wrong. There is a simultaneousness, a coincidence, and with attention and patience we shall succeed in discovering the truth.

It happens sometimes that nurses contract syphilis during nursing, and the contagion can have its influence upon them through different regions. Most frequently it is by the genital organs. This fact is not uncommon for nurses who come frequently to Paris. Under these conditions the nurses infect their infants by the aid of their fingers contaminated by the virus. They infect even their husbands, and in these cases the cause of the evil is always referred to the *Parisian child*—to those rotten children, as these unchaste nurses are in the habit of

saying. It happens very often to M. Cullerier and myself to make our observations simultaneously in our two hospitals; he attends the woman at the Lourcine, and I attend the husband at the Hospital du Midi. These poor rustic husbands besides have an extreme candor upon the origin of their verole. The infant is invariably for them the origin of all the evil.

A mode of contagion quite common with nurses is the inoculation of the virus which they themselves convey to the nipple. Affected with a genital chancre, they carry their fingers to the diseased parts, they soil them, and then, without previous washing, they draw upon the nipple, more or less irritated, and thus implant a chancre, which they do not fail to transmit to the child. The position of these mammary chancres, of which I have recently seen a very beautiful example in the wards of M. Cullerier at the Lourcine, is very well explained by the manner in which the women take the breast to give it to the infant. I have caused another very beautiful example to be designed in the *clinique iconographique* (19e livraison.)

Here is another means of contagion in nurses. I have met with one in whom a chancre had been communicated to the breast by an individual affected with a primitive chancre upon the lip, and who thought that he should render a good service to this woman in drawing off the milk by suction. Very recently there was a young man in my hospital having a primary ulcer upon the mamma, with numerous and indolent swellings of the axillary glands, which were followed at the end of six weeks with an enlargement of the posterior cervical glands, and with a confluent roseola. This young man had been contaminated by his mistress, who, with a chancre upon the lips, had lavished upon him some eccentric kisses.

Another mechanism. I have seen a nurse come to Paris to claim indemnity for a syphilis, which she said she had taken from the infant which she nursed. This woman had an indurated chancre upon the inner side of each mamma; these chancres were placed opposite to each other. As to the child, *rotten*, according to the nurse, it was simply suffering under a porrigo larvalis of the most common description. The parents, who were perfectly healthy, little satisfied with the accusation, and especially with the demand, resisted the pretensions of the nurse, from whom I obtained a complete avowal. A man *who was not her husband*, in the fear of begetting a child and altering her milk, had given himself up to acts upon her, which the pen refuses to trace.

An infant can contract chancre at the time of birth, if the mother is so affected at the period of parturition. This is without doubt rare, but it is not impossible. These chancres

which are very often apt to be confounded with secondary accidents on account of their varied and unaccustomed seats, constitute, as we can easily conceive, focuses of infection for the nurses, and are afterwards offered as proofs of the possible contagion of secondary accidents. What again apparently comes to the aid of this manner of viewing things, is that in endeavoring to go back to the source from which the infant could have been contaminated, in the case where we arrive *too late*, we can find nothing upon the mother, the primary accidents which she had at the moment of the parturition having had time to become cicatrised without leaving any traces. Then if the *legal* father has in his recollection the remembrance of any blennorrhagia in his early youth, everything is laid to the charge of heritage. But what can we say, when we do not find anything and have no confessions?

Infants at nurse can be infected by strangers, whom we do not suspect. They might afterwards infect their nurses, and before these latter could perceive the disease of their infant, and especially before they could recognize the nature of it, and account for what they themselves experience, the secondary accidents so prompt to develop themselves in young infants could have already appeared, and masked the point of departure in a manner to render it not easily recognized. I remember a remarkable case in this respect, for which my learned brother and friend, M. Richet, Surgeon at the Hospital de Lourcine, consulted me a few years ago. It was concerning a little daughter of a lawyer of Paris, still entrusted to the care of her nurse, and who was affected with syphilitic ulcerations upon the *ano-genital regions*. The parents being perfectly healthy, and the nurse absolutely in a healthy state, although she might have been suspected, the question arose from whence could come the contagion, when we learned that a clerk in the house, at that time diseased, had the habit of seating this child naked upon his hands, often soiled, and which he had not always taken care to wash. Without this discovery, how would they have explained the disease of this little child, and who would they have accused if the nurse had presented any trace or suspicion of syphilis?

In all these cases, with habit and perseverance we can succeed in discovering the source of the accidents. But it is not always so. The mother of the child is perfectly healthy; the *husband* of the mother is irreproachable; the nurse is free from all suspicion; and yet the child becomes diseased with syphilis. Here, where is the contagion? Permit me to cite to you a fact which could serve as an answer to this delicate question.

A young woman, accompanied with her husband much older

came to consult me for her child which she had just taken from the nurse, and which was infected with a constitutional syphilis which she accused the nurse of having communicated to it. The child was almost entirely covered with a moist scaly syphilitic eruption; the region about the anus and the labia was the seat of ulcerated mucous tubercles. The child was six months old, and according to the nurse it was at the end of six weeks that the first accidents showed themselves. However, the mother and the *husband* affirm to me that they never underwent any contagion, and by a most careful examination, in fact, I could discover nothing either past or present. The nurse, examined in her turn, appeared to me perfectly healthy. Her own child, which she nursed at the same time as the sick infant was in excellent health. I was much embarrassed in the research for the origin of the syphilis of this child, when I received the next day the visit of a young cavalry officer, who came to consult me for a syphilitic plantar and palmar eruption with which he was affected. This officer interrogated me with a touching solicitude upon the disease of the child which had been presented to me the day before, and he made me a confidant in the part which concerned him in this question; but as he did not know the laws of transmission, he was surprised to have begot a diseased child, inasmuch, said he, as he thought himself cured, and that he had no symptoms of the disease when he had connection with the lady, who in fact had not been diseased.

After all that I have told you, my friend, you see how much reserve, prudence, care and attention are necessary, before accepting as a demonstrated fact the contagion of secondary accidents. Do you not think with me, that for establishing definitely this law in syphilography, other facts are necessary than those at present deposited in the annals of science?—[*Boston Med. and Surg. Journal.*

The Speculum as a means of Diagnosis.

[We extract the following remarks of Prof. Henry Miller, of Louisville, from his able Report to the Kentucky State Medical Society. The subject is one of deep interest to the profession as well as to the community at large:]

“In a paper, on the use of the speculum, read before the *Royal Medical and Chirurgical Society*, May 28, 1850, Dr. Robert Lee makes the assertion, that in the two great classes of organic diseases of the uterus—malignant and non-malignant—and in all the displacements of the uterus, he has derived

little or no aid from the speculum, in their diagnosis and treatment. The writer confesses his unfeigned surprise when this assertion, by an author of Dr. Lee's standing in the obstetric department of the profession, first arrested his attention, in perusing the report of his paper in the *London Lancet*. In the discussion which ensued, none of the distinguished gentlemen present appear to have noticed it or animadverted upon it in such terms as it deserves. Let us, then, inquire whether the speculum is indeed superfluous, first, in organic diseases, and secondly, in displacements of the uterus. It will be conceded, we presume, that inflammation is an organic disease, and that it is, moreover, the architect of numerous other diseases of the same class. Now, Dr. Lee virtually affirms that the speculum is not needed to discover the existence of inflammation of the cervix uteri, and upon this we join issue with him, being willing to stake the fortune of the speculum on its trial by a jury of our peers.

"If the speculum be discarded, we cannot discover inflammation in this, its favorite lurking-place, except by the symptoms that accompany it, or by the touch, in the usual mode of examination. Will the symptoms reveal it? Their uncertainty and the dimness of the light they shed, are proverbial. There may be pain or a sense of heat in one of the iliac regions, together with back-ache and neuralgia of the musculo-cutaneous nerves of one or both thighs. There may be frequent and painful micturition or tenesmic irritation of the rectum. The menstrual function may be deranged, and there may be leucorrhœal discharge. But any or all of these symptoms may be present, and yet inflammation may not exist, while there may be inflammation, and few or none of these symptoms be complained of. Of the truth of these remarks no practitioner can be ignorant, who is much conversant with the diseases of females, and is familiar with the use of the speculum. The writer well remembers the case of a lady, the mother of two children, who miscarried in her third pregnancy, and suffered severely with her head for more than a year afterwards. She complained of fullness of the head, with more or less pain continually, and occasionally with very acute pain. On the part of the uterine system there was no evidence of any thing amiss, except that she did not conceive again, and menstruation, though regular, was scanty, seldom lasting more than a day, and amounting to a mere show. There was not, at any time, leucorrhœal discharge, nor did she complain of pelvic pains, and yet when examined with the speculum, chronic inflammation, with hypertrophy of the uterine neck, was discovered. This was cured by the usual treatment: menstruation returned

to its healthy type, and the cephalic symptoms gradually abated.

"Can the touch detect inflammation of the cervix? This question might be answered by another: could a blind surgeon detect cutaneous inflammation by the touch? The truth is (and every accoucheur well knows it) none of our senses is more deceptive than the touch, or more frequently leads to mistakes. The only discovery which can be made by it, in the matter under consideration, might be made as well by any other instrument as by the finger, viz, the existence of morbid sensibility in the cervix uteri. When the inflamed cervix is pressed upon the finger, the patient usually winces, and so she would were it pressed upon by a stick. Morbid sensibility may, however, exist independently of inflammation, and cannot, therefore, be regarded as furnishing conclusive evidence in such an investigation.

"Upon the whole, then, the practitioner who relies on the symptoms and touch only, for his diagnosis in these cases, can never know of a surety that inflammation exists: he may surmise it, but cannot possibly have any greater certitude than could a blind oculist concerning the existence and nature of inflammation of the eyes.

"Ulceration belongs also to the class of diseases, in which, according to Dr. Lee's assertion, little or no aid is to be derived from the speculum,—howbeit he is incredulous as to the occurrence of this morbid state, in the female sexual organs, except to a very limited extent. He says explicitly that he has never seen ulceration of the os and cervix uteri, which was not of a specific character, especially scrofulous and cancerous. To fortify himself in this position, seems to have been the main object of his paper; for could it be proved that ulceration is a rare disease in these parts, the speculum might the more readily be driven from the field. Dr. Lee's clique, who rallied around him in the debate, felt equally with himself the necessity of expunging ulceration from the list of female sexual maladies. To accomplish this, they were forced to maintain that ulceration necessarily involves a palpable loss of substance. It is readily admitted that, in this sense, ulceration is a rare form of disease of the os uteri; we are not sure, indeed, that we have ever once met with it, nor have we a right to look for deeply excavated ulcers in such a situation. The mucous membrane alone is commonly implicated, and this is here of such exceeding tenuity that it cannot be dissected from the subjacent tissue. The nearest approximation to a dissection, which can be made by the most skillful anatomist, is to lift it up, in delicate patches, upon the point of a sharp lancet. Supposing the mem-

brane to be destroyed, in its whole thickness, by the ulcerative process, there would not, therefore, be palpable loss of substance or any thing like an ordinary ulcer upon the skin, or even upon the mucous membrane of the intestines. But there is, nevertheless, what fulfills the definition of ulceration, namely, a solution of continuity, in a soft part, accompanied by a purulent discharge, for it may be brought to light by the speculum, and when wiped with a sponge, a raw and often a bleeding surface is exposed. What matters it, if Dr. Lee and his partisans choose to call it 'abrasion,' 'excoriation,' or by any other name. Such a surface, produced by morbid action, were only the epithelium destroyed, is ulceration; for there is solution of continuity and there is purulent secretion.

"Ulceration of the os uteri is usually accompanied by inflammation, and the symptoms to which it gives rise are nearly the same, only there is more constantly purulent leucorrhœa. But this discharge does not always attend it; for the secretion may be so slight as to be absorbed, and there may be purulent discharge without ulceration. Ulceration cannot, therefore, be predicated of any case from the symptoms only. It may be discovered by the touch, when the roughness of the affected surface is well marked, but in the very great majority of instances, nothing can be positively affirmed until the parts are brought under ocular inspection. Of this, every day's experience convinces the writer more and more firmly. While inditing this report, he had occasion to examine a lady, from a distance, whom one of the most distinguished surgeons in this country, after examination by the touch alone, pronounced to be laboring under displacement of the womb, the organ being, as he assured her, perfectly free from disease: the writer was soon satisfied, by a specular, as well as tactual examination, that there was chronic ulceration of the os uteri, but no displacement of any kind!

"The committee will next attempt to estimate the claims of the speculum, as a means of diagnosis, in displacement of the uterus, the other class of cases, in which Dr. Lee says it is of no value. None of these displacements is clearly indicated by the symptoms alone, except *retroversio uteri* occurring in the pregnant state, in which the sudden and total suppression of urine, together with the severe sufferings of the patient, points plainly enough to its existence. But in the non-gravid state, neither *retroversion*, nor *anteversion*, nor *prolapsus*, (the most common of all the displacements,) is accompanied by such symptoms as throw any satisfactory light on the subject. To the touch, at least, an appeal must be made, and through it we may learn that the organ is displaced, and the manner of its

displacement; but we cannot learn its pathological condition, a capital hiatus in the information we are in quest of; for the speculum has taught us the frequent, nay, the almost constant co-existence of inflammation or ulceration of the cervix uteri. So true is this, that the writer can conscientiously declare that, since he has used the speculum freely in his practice, he has seldom seen an instance of prolapsus or retroversio uteri, uncomplicated with inflammation or ulceration of the cervix; and he is becoming more and more skeptical as to the existence of simple displacement of the uterus. His own view of the pathology of such cases, is that inflammation is the primary and essential disease, while the displacement is merely a sequence. Such is the doctrine advocated by Dr. James Henry Bennet, in his valuable practical work on 'Inflammation of the Uterus,' who attempts to explain the occurrence of prolapsus on the principle of the increased gravity of the uterus, acquired by inflammation. Dr. Meigs rejects the doctrine, and thinks he has most triumphantly refuted it by showing, as we think he has very conclusively, the insufficiency of the explanation.—(*Females and their Diseases*, p. 137.)

"But it does not seem to have occurred to Dr. Meigs that the doctrine may be true, while the explanation may be false. Grant the existence of inflammation of the cervix as the antecedent, and it may be that the irritation, established in the part and propagated to the neck of the bladder and to the rectum, will eventually cause prolapsus by the bearing down efforts which it provokes, and this, we suspect, is the true etiology.

"Be this as it may, and whether inflammation is the antecedent or the consequent of the prolapsus, the writer reaffirms, without the fear of successful contradiction, that inflammation or ulceration exists in nearly every case of displacement of the womb, and that it can be detected only by the speculum.

"But Dr. Lee, as we have seen, not only renounces the speculum in the diagnosis, but also in the treatment of the whole class of diseases we have been considering. It is difficult to imagine the grounds of this renunciation. Can it be that the treatment of these diseases, by other means, has been so successful in his hands as to preclude the hope of improvement? If so, we sincerely congratulate him on his good fortune, in a field where all other practitioners, from time immemorial, have met with little else than discomfiture. For our own part, we are not ashamed to confess that, until we called the speculum to our aid, we were defeated on every hand, or, at best, victory so seldom perched upon our standard, that we were bound to regard our success as fortuitous, rather than merited. We never cured a case of prolapsus by the pessary, or of long-

standing leucorrhœa, connected with inflammatory or ulcerative disease of the cervix, by constitutional treatment and the ordinary local appliances.

"Such *fillibustering* may succeed in recent and trivial cases, but when the disease is more strongly intrenched, it can only be dislodged by a superior force operating directly and systematically upon it.

"These uterine affections are essentially local in their nature: they owe their origin to local causes, and are most successfully treated by local remedies. But the remedies must be sufficiently potent to make an impression upon the disease. The sprinkling of an inflamed or ulcerated os uteri, with simple or medicated water, by means of a syringe (the only local remedies resorted to by the *fillibusters*) cannot be more efficacious than such piddling ablutions upon other parts of the body. What would be thought of a surgeon who should attempt to cure an external chronic inflammation by squirting a little water or solution of lead or zinc upon it, two or three times a day?

"The more potent remedies which are addressed to the affected part through the speculum are, chiefly the local abstraction of blood by scarification or leeching, and superficial or deep cauterization, according to the circumstances of the case. It is not the design of the writer to enter into details on this part of the subject; he begs to refer the Society to practical works, particularly to Dr. Bennet's treatise, already alluded to. He will, nevertheless, submit a few annotations, suggested by his own experience in this branch of practice, which has been pretty extensive.

"*First.* Local depletion may be effected as well by scarification as by leeching, when the inflammatory congestion occupies the superficies of the os uteri, and ought to be preferred, because it may be done more expeditiously, and is far less revolting to the patient. When the inflammation is deep-seated, and there is little or no discoloration upon the surface, leeches should be employed, and half a dozen are commonly sufficient to procure as free bleeding as is desirable. Local bloodletting is a valuable part of the treatment of these cases, and ought always to be premised, whenever there is any considerable degree of inflammation. It is a good preparation for cauterization, and may be advantageously repeated, in conjunction with cauterization, until the inflammatory congestion is subdued.

"*Secondly.* With the same view, cold mucilaginous injections—infusion of flaxseed or slippery elm—should be thrown into the vagina, by the patient, three times a day. But these will accomplish nothing unless a good syringe is provided, and the patient properly instructed in its use. The injections should

be taken in a recumbent posture; the syringe ought to hold several ounces and have a pipe, with a bulbous end, long enough to reach the superior portion of the vagina.

“Thirdly. When the inflammation or ulceration is confined to the mucous membrane, with only slight enlargement, and no induration of the cervix, cauterization with the nitrate of silver in substance, is the only application which will be found necessary in most cases. This ought not to be repeated too frequently—an error, which the writer has reason to believe, is committed by some—not oftener than once a week. Six or eight of these hebdominal cauterizations may suffice to cure the disease; but in some cases, a longer perseverance may be necessary, and in a few, the inflammation may prove altogether refractory. In such instances, the writer’s practice is to cauterize once superficially with the potassa cum calce, and afterward, with nitrate of silver as at first.

“Fourthly. Should the inflammation have extended to the proper tissue of the cervix, and resulted in induration, deep cauterization with the potassa cum calce will be indispensable to restore the part to its normal state, and heal any ulceration which may exist. It is quite useless to treat such a condition with the nitrate of silver; the ulceration will seldom be cured by it, and it can make no impression upon the deeper-seated disease. The writer has practiced deep cauterization, in many cases; in several, he has used the actual cautery, and he has never known any serious accidents to follow. He is always careful, however, to apply the caustic through a tubular speculum, and to sponge off the part, so as to guard against any of the caustic remaining and spreading to the sound parts, after the withdrawal of the speculum. With this precaution, he considers it to be as safe to apply caustic to the cervix uteri as to the skin. Much obloquy has been cast upon the speculum on account of alledged abuses of cauterization, and the writer doubts not that there is some foundation for it; for he can easily conceive that the careless or inexpert use of such a potent agent, may produce extensive inflammation and sloughing, followed by unnatural adhesions and contraction of the genital passage. But such consequences are attributable to the awkwardness or ignorance of the operator, and are no more chargeable to the speculum than is the transfixion of the vein in phlebotomy to the lancet. The writer can truly say that no such consequences have ever happened to him or need happen to any one, fit to be trusted with the speculum.

“Fifthly. Rest in a recumbent posture, more or less strictly guarded, according to the degree of inflammatory action that exists, is a material adjuvant in the treatment of these

cases: and where this cannot be enforced, the disease is greatly prolonged, and may prove altogether ungovernable.

"Exercise, or even the erect or semi-erect position tends in a direct manner, to increase the uterine congestion and aggravate the sufferings of the patient. The writer cannot doubt, from what he has seen, that much mischief is often done by urging the patient to take exercise, under the fallacious idea that weakness is the sum total of her ailments, and that if she can only be strengthened by air and exercise, all will be well with her.

"So strongly is the imagination of some physicians haunted with the bugbear, weakness, that they will persist in keeping the patient in motion, notwithstanding that every step is a dagger to her. When shall more rational views obtain currency in the profession? How long shall a mere effect engross the attention, while the cause is overlooked?

"The writer was recently consulted in the case of a lady, who suffered greatly from pelvic pains after her second confinement, increased by exercise or the erect position. She had hæmorrhagic discharges from the uterus for several weeks after parturition, with almost daily febrile excitement, intense thirst, loss of appetite, and general debility. The debility unfortunately, absorbed the attention of her medical attendant, and to remedy this, exercise in a carriage was commenced on the eleventh day after her accouchment, and persisted in daily in spite of her remonstrances, extorted by the increase of her suffering, and finally, she was sent away on an excursion in pursuit of the *ignis fatuus*, 'strength.' When she returned home, a specular examination was made, and a high degree of inflammatory engorgement of the uterine neck and upper portion of the vagina, with ulceration around the os, was discovered, which had existed doubtless since her delivery.

"*Sixthly.* Although the local treatment is paramount to every thing else the state of the general system must not be overlooked or neglected. If constitutional irritation exist, it must be subdued by appropriate remedies, or if any of the functions are sympathetically deranged, they must be restored to a healthy condition by suitable treatment. In recent cases, some degree of febrile excitement not unfrequently exists, and to allay this, it may be proper to put the patient upon an abstermious regimen, to purge actively every day or every other day, and if there be hardness as well as acceleration of the pulse, general bloodletting may be necessary.

"Dr. Dewees was well aware, though he had not the ocular proof, of the existence of uterine and vaginal inflammation, in many instances of leucorrhea, which is only another name for

the disease we have been considering, and the success of his treatment was doubtless attributable to the bleeding and purging he prescribed, rather than to cantharides, which he regarded as a kind of specific. This is fairly to be inferred, from the fact that none of his contemporaries or successors have been as fortunate in the use of cantharides as himself, which can be accounted for only by supposing that they have relied principally upon the specific, to which the multitude are always prone, to the neglect of due attention to the state of the system. It is not intended to be asserted that cantharides is devoid of all remedial virtues in these cases. By its action upon a contiguous and associated viscus, it may exert some beneficial influence upon the genital organs; nevertheless we are persuaded that the antiphlogistics, so vigorously employed by Dr. Dewees, had a larger share in extinguishing the disease than had the cantharides pushed ever so often *usque ad stranguriam*.

"In more protracted cases, the general state is characterized by veritable debility, a languid circulation, coldness of the extremities, and impaired digestion and assimilation. Under such circumstances, it will be proper to administer tonics, especially some of the preparations of iron, and to regulate the secretions and excretions by the use of alteratives and purgatives. The selection of these will be governed by the indications of each particular case. As to purgatives, it is necessary to observe that only such of them are admissible as may be required to procure one full alvine evacuation daily, to effect which a pill or two of rhubarb and extract of colocynth, or of rhubarb, aloes and soap, may be taken every night.

"Mercury, iodine, arsenic and antimony, are among the most powerful alteratives, and the indications for the use of remedies of this class may be fulfilled by the various preparations and combinations of these agents.

"As to sarsaparilla, which is so often prescribed, we do not know that we have ever obtained any good from it, even when furnished by the regular apothecary; while sure we are, that the quackish preparations of it, which find their way by the hogshead into the stomachs of our nostrum-loving population, are utterly worthless."

Report of Two Cases of Intestinal Fistulæ. By DE LASKIE MILLER, M. D., Chicago, Ill.

CASE 1. Visited — Johnson, aged about 12 years, January 22nd, 1850, found him with considerable febrile movement, with severe pain in the right iliac region, increased on pressure, and continuous, bowels constipated. Directed fomentations to

the painful part, and ordered a Cathartic, to be followed by a full dose of Dover Powder. 22rd—The Cathartic operated kindly, removing a large quantity of hardened fæces. His pulse is 120 in the minute—tongue dry—pain in the right iliac region, about the same as yesterday. Some tumefaction corresponding to the seat of pain.—Continue fomentations, and prescribed the following:—℞ Hyd. Prot. Chlor. grs. viij. Pulv. Doveri ʒj.—M. Div. Chart No. 4. take one every fourth hour. 24th.—Patient much the same as yesterday—pulse rather softer and 110—tumefaction increasing, but circumscribed. Ordered a laxative, to be followed by Dover Powder, sufficient to quiet the pain. Applied a blister to the whole extent of the painful part.

The blister drew well, and discharged freely, the tongue became moist—the pain gradually subsided, though tenderness to pressure remained—the swelling increases, without extending its boundary, which is about five inches in diameter. After several days, detect fluctuation in the part, there seems to be great exhaustion of the vital forces, the patient complains of debility, and a sense of depression. He states that two days before I visited him, he received an injury at the point affected by falling upon a billet of wood.

Punctured the swelling, which discharged a large quantity of fœtid gas, and a small quantity of grumous blood, mixed with unhealthy pus—the tumefaction immediately subsided, and a distinct ring marking the boundary of the swelling remained. Ordered a cataplasma to be applied to the part, and prescribed:—Sulph. Morphia, as occasion required to keep the patient quiet. February 10th.—Three days after the abscess was punctured, the discharge consisted almost entirely of fæces, with portions of partly digested food; no natural evacuation from the bowels during the last three days. Ordered injections.

The anæmia produced a slight evacuation from the bowels; the patient feels but little pain. Advised a nourishing diet, and porter. March 15th.—The patient improving in strength—the Artificial opening assuming the appearance of a fistulous orifice, and discharges as last noted. The lower portion of the bowels have been kept open by the use of injections, alternated with croton oil. April 15th.—The patient so much improved, as to ride 25 miles, to the residence of his father. The discharge the same in quality as last noted, but less in quantity. The orifice is evidently smaller. January.—One year after the attack, the patient has perfectly recovered; the orifice continued to contract, the discharge becoming less, and finally ceased altogether.

CASE 2. Niles had been sick two weeks, and under treat-

ment for inflammation of the bowels, when I was called to see him, in consultation, found him with pain in the right iliac region, which was increased on pressure, had taken Calomel largely, and been cathartised freely. A blister was applied over the seat of the pain, and anodynes prescribed. About two weeks subsequent to this time, I was requested to visit him again, and take charge of the case, he was at this time very much debilitated; pain and tenderness in the iliac region same as before, but found an abscess pointing about two inches to the right side of the spinous process of the second lumbar vertebra: punctured it, when it discharged freely, pus intermingled with flakes of lymph, poultices were applied to the part, and tonics prescribed. On the second day after the abscess was opened, fæces were observed to escape from the orifice, and occasionally portions of undigested food. About three weeks subsequent to this, another opening was formed about two inches internal to the anterior superior spinous process of the ilium, giving exit to a similar discharge. The opening on the back now began to contract, and finally closed, about six weeks after it was formed. After the time the first opening was made, it was difficult to produce an evacuation per anum. Injections were used, alternated with croton oil.

This course was followed for near six months, and the patient sustained by tonics and a nourishing diet, when the fistulous opening gradually contracted, occasionally closing for a few days, then opening for a short time, and discharging as before, till at the end of eighteen months, when it remained closed, and the patient regained his usual strength and spirits.

Remarks. There seems but little doubt, that in the case of Johnson, the inflammatory action, and consequent ulceration were induced by the injury received by the fall, the billet of wood forcing the walls of the abdomen, and coats of the intestine, against the accumulation of hardened fæces, contained within the Colon at the time producing contusion, and possibly slight laceration of the coats of the intestine. We see exemplified in this case the beautiful provision of nature, which by forming adhesions between the various structures, around the injury, prevents the fatal consequences that would ensue, from the escape of foreign substances into the peritoneum. A point of practical importance is the propriety of the early openings, from the great depression of the vital forces; the inference in this case was, that the contents of the abscess were producing serious mischief, which was, manifested upon the escape of the fœtid gas, and the relief which followed.

In the case of Niles, is it not probable that some solid foreign body had found its way, perhaps, into the appendix vermiformis

whence it could not dislodge itself, causing the inflammation and ulceration, and leading to the abscess? None such was discovered in the discharges, although the attention of the parents was directed to it. But it is easy to understand how it might escape unobserved in the great amount of matter that was constantly discharging. Not the least practical point in these cases is the use of croton oil, this article fulfilled the indicatives admirably, it produced a uniform peristaltic action throughout the entire extent of the intestinal canals, without irritating the internal orifice of the fistula, by its bulk. In the management of the above cases, the reader will notice an apparent disregard of the opinions of the older physicians, "who, in injuries of the abdomen, extending to the intestines, never left anything to nature."

[*North-West Med. and Surg. Journal.*]

On the employment of Creasote in Deafness. By Dr. HARRISON CURTIS.

One of the principal causes of deafness is the absence of the secretion of cerumen in consequence of a fault in the action of the ceruminous glands. Often in my clinics, even when the deafness has continued for a long time, I have observed that it has no other cause, and on removing that, I have caused the infirmity to disappear. It is very true that to obtain this result, more or less time is necessary, according to the duration of the infirmity, and in proportion to the gravity of the first cause of the inaction of the glands. After having cleansed the auditory meatus, and re-opened, so to speak, the orifice of the passage, by removing the morbid secretion which obstructs it, the use of a moderate stimulant is indispensable to reestablish the normal action of the glands. But before all, it is necessary to cleanse the auditory meatus, as no remedy can have the least effect, unless this operation has been well performed. In general I employ a preparation composed of half an ounce of beef's gall and a drachm (*un gros*) of tincture of castor or tincture of musk. With it I moisten a piece of cotton, which I place in the auditory meatus at night, to soften the hardened cerumen. In the morning I syringe the ear with warm water, to which may be added an ounce of soap liniment and a little cologne. I have often substituted with advantage, for the preparation of beef's gall and tincture of castor, the solution of potass of our Pharmacopœia London?) with the oil of sweet almonds, to dissolve the cerumen.

I would recommend for this operation, to be particular in the choice of a syringe. When the ear is well cleansed, and the glands are in such a state that a stimulant can act upon them,

I would advise, in accordance with results which I have obtained from my clinical experience, the employment of a solution of creasote in oil of (sweet) almonds, to induce the ceruminous glands to resume their normal action. The following is the formula which I employ—

R Creasote - f. ʒj.

Oil of sweet almonds f. ʒiv. ℥. and with a badger's hair pencil put a small quantity in the auditory passages night and morning. I ordinarily commence with a solution of this strength, and augment the quantity of creasote according to the effects obtained. Cases, however, present themselves, in which no good result will be obtained from this application without applying behind the ear a vesicatory of ointment of tartarized antimony, or other derivatives. In otorrhœa, and always when there is pain or inflammation the creasote is contraindicated.

Its application causes no pain or unpleasant sensation, but only an agreeable feeling of warmth.—[*Lancet*.

Is not Blood-letting sometimes dangerous in Apoplexy? By M. AUSSAGUEL.

This question assuredly merits the attention of every practitioner. M. Aussaguel has collected in his inaugural thesis, from which we borrow this extract, a number of facts upon the subject, which demand grave consideration.

“M. Cruveilhier when he lectures upon the treatment of cerebral hæmorrhage, never fails to say ‘undoubtedly it is necessary to bleed, but be very circumspect,’ * * * and then he relates candidly, that having been sent for to visit a patient in the city, whom he found threatened with an apoplectic attack, he hastened to open a vein; the wound was scarcely closed when the patient was attacked with hemiplegia: and he adds, ‘the relatives of the patient did not hesitate to say that it was my lancet that had done the mischief.’

“Since then we have read the thesis of M. Cornil: ‘A woman,’ says he, ‘whom I observed last year in M. Rostan’s ward, was occupied with her household duties, when she experienced, all at once, a loss of power in her left upper and lower extremity. She with difficulty walked to the house of her physician, who bled her immediately. After the venesection, she was unable to rise: she was completely hemiplegic.’

“The following instance came under our own observation. A friend came to us, stammering in such a manner, that he re-

quired fifteen minutes to make us understand that, the morning of the same day, upon awakening he was greatly surprised to find himself in this condition. There was slight loss of power in the right arm, and its sensibility was diminished. Dr. Batailhé having been summoned, practised venesection. The next day the stammering had increased, and the patient was copiously bled a second time. Syncope ensued, and the patient revived in fifteen minutes in a state of complete hemiplegia. It is now two years since he was able to utter a word.

"I ask, then, if facts of this kind were numerous, would they not have a kind of accusing eloquence against the employment of blood-letting? and when an impartial witness observes their development, is he not tempted to say, with the relation of M. Cruveilhier's patient, 'it is the lancet that has done the mischief?'

The author afterwards attempts to account for these exceptional cases, and his explanations are not without a certain value.

"What occurs after blood-letting in certain cases of pneumonia? Does not the weak, small, pulse become full, strong, and well developed? Do we not observe an increase in the forces of economy, and is it not generally believed that at that moment another congestive movement toward the lung occurs? Therefore it is to combat the results of blood letting by blood-letting itself, that M. Bouillaud advises repeated venesection; in other words, the loss of the same quantity of blood is of greater efficacy when it is abstracted by several operations than by one.

"Should we wonder if this were true also for the brain? Why wonder that this organ, enclosed in its unyielding case, engorged with blood, resists the tendency to hæmorrhage for a time, and then yields to it after venesection, the circulation becoming at that moment more active? In other words, are there not two distinct causes operating in the production of apoplexy; the circulating mass, and the power which propels it? and does it not seem impossible to diminish the one without increasing the other?

"To diminish the first, without increasing the second, such should be the aim of the practitioner.

"It is with a view to attain this end that we propose that a vein should never be opened until the head of the patient is elevated, and cold applications are made to it, and the blood is invited to the lower extremities by sinapisms or pediluvia, and the patient has taken some soothing draught, with a few drops of digitalis."—[*Rev. Med. Chir. de Malgaigne. Virginia Med. and Surg. Journal.*

Treatment of Goitre by Electro-puncture.

M. Jobert, of the Hotel-Dieu, is in the habit of treating goitre by electro-puncture. The operation is very simple; each of the conductors being furnished with a long needle, is plunged into the tumour, and the apparatus then charged. The operation is not very painful, and may be repeated every day. The tumour gradually loses its bulk and hardness. M. Jobert has been successful in many cases, and in some, where this means is not effective or speedy enough, he injects iodine, which, after causing some inflammation, has the effect of causing resolution in the tumour, and curing it effectually.—[*Gaz. des Hop. Ibid.*

Case of Catalepsy, illustrating some new principles of Treatment in Convulsive and Spasmodic Diseases. By Dr. C. B. RADCLIFFE.

William B——, æt. 12, residing with his parents at Shepherd's-bush Bayswater, was seized on the first of February last with a painless rigidity of his forearms and hands, which lasted from tea-time, when it first made its appearance, until bed-time. Similar symptoms also recurred daily for many days, sometimes beginning early in the morning and lasting throughout the day, at other times merely showing themselves for a few minutes, while the patient was preparing for bed; and so they continued to do until the 28th, at which time they extended to the feet and legs, and in less degree to the upper arms, thighs and trunk, consciousness being completely suspended, which was not the case previously. These general symptoms, which were evidently those of catalepsy, alternated with the more partial symptoms, occurring several times in a single day, and recurring throughout the whole of the next week, at the expiration of which period the case was brought to me.

During all these fits, partial or general, the rigid parts were cold,—so cold as to cause some of the bystanders to say they were dead. Cold, also, had evidently much to do in the matter as an efficient cause. In the earlier stages of the affection, playing out of doors (the weather at the time being very severe) invariably sent the patient into the house crying, with his hands and forearms immovable; and the exposure caused by undressing in a cold bed-room at night gave rise to the same symptoms. On the morning in which he was brought to me, also, the cold had acted in the same manner, and it was not until he had been some time seated in the warm room that the stiffening relaxed. I had thus an opportunity of satisfying myself that the parts which had lost their pliability were in reality cold, and that the

pulse in them was almost extinguished, and the sensibility all but departed. I had the opportunity of satisfying myself, also, that this depressed condition extended more or less to the system generally, and that all the mental and bodily functions were as far as possible removed from anything akin to excitement.

The other facts in connection with this case all denoted the want of real power. He was any thing but precocious,—not walking for two years, and not talking for a year later, and now speaking with hesitation,—very slow in apprehension, feeble in memory, and, though twelve years of age, as irritable, uncertain, and fretful as an infant. His head is large; his eyes staring, and the pupils dilated and sluggish; his complexion sallow and venous; his hand cold and clammy; his pulse slow (70) and feeble; his body prominent and hard; his skin unhealthy and covered with boils; his appetite voracious. Both parents appeared to be healthy; but a cousin of the mother suffered from fits of a similar character, and lost her life in one of them, by falling from a foot-bridge into a brook.

Acting upon received views, that *increased* irritation in the chief nervous centres had to do with the muscular rigidity. Mr. Roy, of Brookgreen, had tried the usual course of leeches behind the ears, blisters to the nape of the neck, low diet, mercurials, hot mustard fomentations to the feet, and so on; but finding the symptoms becoming more general and confirmed, he sent the case to me.

The treatment I pursued rested, on the contrary, on the supposition that rigidity was dependent on a *diminished supply* of nervous and other motive stimulus, a view which I have elsewhere endeavored to substantiate in relation to all forms of muscular contraction, normal and abnormal. I, therefore, recommended that the patient should be kept *still*, and as warm and comfortable as possible; that animal food and good table-beer should be given him freely; that he should be undressed before the fire, and carried to bed wrapped in warm flannels, having been first for some time immersed in a hot bath; and that he should take, every few hours, a small teaspoonful of spirits of turpentine, with ten drops of aromatic spirits of ammonia, mixed up with a part of the yolk of an egg, in a little ginger wine.

A fortnight after this first interview, I saw the patient again, and found that the draught had occasioned a little irritation in the bladder, but so little that there was no necessity to discontinue the remedy. On the other hand it had put a stop to a troublesome and old-established habit of micturating in bed. As to the rest, there was a material improvement in every re-

spect, the countenance being brighter and more energetic, and there having been no cataleptic rigidity from the day the stimulant and nutritive plan of treatment had been put in practice.

I saw the patient no more; but learned from the mother, who called two months subsequently, that the treatment had been carried out, for a month longer, that the fits had never returned, and that he was then in better health than he had been before.—[*London Lancet*.

On the Application of Gutta Percha in the Treatment of Diseases of the Skin. By ROBERT J. GRAVES, M.D., F.R.S.

Dr. Graves has been making many successful experiments with Gutta Percha dissolved in Chloroform, in a great variety of cutaneous affections. He finds its application more suited for dry, scaly, tubercular and chronic diseases of the skin than for acute affections, attended with much oozing of fluid and comparatively acute inflammation.

Still, its good effects, he says, are by no means limited to chronic diseases of the skin, or to those of a scaly, dry nature; for, as will hereafter appear, I have seen it decidedly useful in the spreading form of impetigo. My experience of this remedy makes me anxious to witness its application in the first stages of erysipelas, as analogy leads me to hope for good results in such cases.

In *acme of the face*, this application is of essential importance. The pimples should be covered with the solution, and the patient enjoined not to rub off the pellicle by washing, etc.

Finally, in several cases of psoriasis I have applied this solution with great benefit.

When my friend, Dr. Stokes, heard of my success in other cases, he resolved to try the gutta percha solution in small-pox, and it gives me great pleasure to say, the result of two trials is most encouraging, and leads us to hope that at length a means of preventing the formation of disfiguring scars on the face in that disease has been discovered. Dr. Stokes allows me to publish the two following cases:

The first Ann Kenny, aged 18, was admitted into the Meath Hospital, May 11, 1852. She was never vaccinated; her illness began on the 6th of May, with the usual symptoms. On the 8th the eruption appeared, and on coming into the hospital it was well formed and confluent on her face; the fever of a typhoid character, with considerable prostration of strength; she was much annoyed with pain and itching of her face. She was ordered wine and carbonate of ammonia mixture, and her face to be painted with a solution of Gutta Percha in Chloroform.

The solution was applied with a soft brush, the entire face being well coated with it, and after an interval of five minutes (just sufficient time to allow the previous coating to dry,) a second coat of the solution was applied.

This application gave her great relief, and allayed the pain and itching. On the 13th she was much better, and the solution was again applied in the same manner.

May 22d. The crusts came off in large pieces, and there was scarcely any trace of the disease remaining, except a slight discolouration. During the whole time of her illness, from the application of the solution her face continued moist, and there was no ulceration in any place.

The second case presented similar symptoms; was treated in the same manner, with the like happy result.

It is of essential importance to observe that the *gutta percha* solution should be applied to the face until the pustules are fully matured, otherwise injurious consequences may arise.

Lupus supiginosis, psoriasis and other chronic cutaneous complaints, when unattended with constitutional derangement, yield readily to repeated applications of the solution of gutta percha.—[*Dublin Quarterly Jour. of Med. Science.*

Miscellany.

“*The Aztecs.*”—We have, in common with our fellow citizens, recently had an opportunity to see the two little personages, whose arrival in this city was heralded in the public prints and announced by placards upon our street corners as “The Aztecs, a new race of people, two feet high—a complete puzzle to all beholders.” The story of their being the descendants of a dwarfish race of Aztec priests, held in great veneration in a mysterious and hitherto unknown city, hidden amidst the mountains of Central America, is too absurd to arrest the attention of Naturalists, yet remarkably well adapted to the purpose for which it was invented. Every one who knows anything about the history of Dwarfs is aware that they do not procreate—that they are impotent. These little creatures were at one time much sought after and petted by the courts of Europe, which seemed to vie with each other in the possession of the most remarkable specimens of abortive humanity. Attempts were then repeatedly made “by authority” to increase the supply by coupling the parties; but always in vain.

If the “*Aztecs*” are not dwarfs, as alleged by their keeper and even by some Professional gentlemen, we would like to know what they are. Walker defines the word Dwarf, “A man below the com-

mon size of men"; Webster: "a general name for an animal or plant which is much below the ordinary size of the species or kind. A man that never grows beyond two or three feet in height, is a dwarf." Dunglison: "one who is much below the usual stature." Now the male of these "Aztecs" is said to be 18 years of age, $33\frac{3}{4}$ inches high, and to weigh 20 lbs.; the female is said to be 10 years of age, $29\frac{1}{2}$ inches high, and 17 lbs. in weight. They were carefully examined by Dr. J. M. Warren and others in Boston, in the early part of 1851, who estimated, from inspection of their dentition, the age of the boy to be from 7 to 8 years, and that of the girl to be from 4 to 6 years. The boy was $33\frac{3}{4}$ inches high and weighed $20\frac{3}{8}$ lbs. The girl was $29\frac{1}{2}$ inches high and weighed 17 lbs. It appears, therefore, that their stature and weight have undergone little or no change in two years. Their brain is exceedingly small and their mental faculties are correspondingly defective. It is true that they are lively and cheerful, but they have as yet no knowledge of language, beyond the ability to repeat after their "keeper" a few words, such as, papa—mamma—&c., without any apparent consciousness of their meaning. When addressed, they seem to apprehend rather the intonation of the voice, as do brutes, than the articulation of words. Yet we are told that they are neither dwarfs *nor* idiots. Let us again refer to our lexicographers:—Walker's definition of the word Idiot, is "a fool, a natural, a changeling," and "Idiocy, a want of understanding." Webster: "Idiot, a natural fool, or fool from his birth," and "Idiocy, a defect of understanding; *properly* a natural defect." Dunglison: "Idiot—foolish, stupid, ignorant. Now used for one who is fatuous, or who does not possess sufficient mental faculties for the social condition, and for preserving himself from danger. In law, one who has been without understanding from his birth, and whom the law presumes to be never likely to attain any." The "Aztecs" are then certainly not only dwarfs, but also idiots.

In thus endeavoring to remove the mystery attempted to be thrown over the origin and true character of these beings, we do not mean to detract from their interest philosophically considered, for they present certain remarkable peculiarities. They appear to be of a mixed race; probably an admixture of the Negro and American Indian, such as may be found in Mexico and most of the South American States. They differ strikingly from all the dwarfs we have seen, and from most of those upon record, in the smallness of their head; for dwarfs usually have this proportionately more developed than the other parts of the body, whereas in these, the arrest of evolution has

operated equally upon the brain and the general system. Hence it is that whilst dwarfs usually speak correctly and are sometimes quite intelligent, the Aztecs are unable even to ask for the necessaries of life.

Pulmonary Calcareous Concretions.—We are indebted to Dr Jas. M. Scaife, of Claiborn Parish, Louisiana, for several very handsome specimens of calcareous concretions expectorated by a patient affected with consumption. The Doctor states that quite a large number of them were coughed up with purulent matter.

Such concretions, however, are not confined to phthisical subjects, but have been observed in the lungs of persons who have died without any serious lesion of these organs. They usually consist principally of phosphate of lime—and sometimes exist in very great numbers in the pulmonary tissue.

A new remedy for Warts.—A French writer states, in the “Bulletin de Thérapeutique,” that he has observed that the use of a teaspoonful of Carbonate of Magnesia, morning and night, for a few weeks, was generally attended with the disappearance of warts on the fingers. Whether this is to be regarded as an effect rather than a coincidence, seems to us questionable. Warts will often fall off after having existed a certain length of time; hence the success of the incantations and other witcheries resorted to by certain “wart-curers.” One of the most prompt applications we have tried is the Tincture of Iodine. By putting a drop of this upon the wart once a day, it will generally fall off in a week. Lunar caustic, nitric acid, potash, &c., will often succeed very well, but are apt, if incautiously applied, to occasion some inflammation and pain.

A new treatment for Pertussis.—We find in the “Revue de Thérapeutique,” &c. that Prof. Hannon, of Brussels, rejects all the established methods of treating Hooping Cough, and recommends a nourishing diet of bread and meat, with a liberal allowance of wine at least three times a day. His directions are as follows: “In the morning, roasted meat and toasted bread with Port or Madeira; at noon, bread and wine; at 5 o'clock P. M., broth, roasted meat and toast; at night, no supper; at bed time, wine: nothing but cold water between meals.” He adds that, however singular this treatment may appear, the symptoms will begin to yield in a very few days and the cure be complete in from one to two weeks. This plan may be worthy

of trial, but can of course be adapted only to children who have been weaned.

Yellow Wax for making Ointments.—Dr. Henle (An. Médicale) having observed that unguents prepared with white or bleached wax became very soon rancid, urges the use of *yellow wax*—as by this means the unguents will keep fresh four times as long.

“WARRENTON, GA., May 24th, 1853.

“Dr. Dugas :—On the 5th inst., several of the Physicians of Warren County organized themselves into a Medical Society. A Constitution and By-Laws were adopted—the duty of drafting the same having been put upon some of the members of the Profession at a preceding preliminary meeting.

“The meetings of the Warren County Medical Society will be held bi-monthly, beginning on the 1st Wednesday in July next. The officers elected to serve the ensuing year, are : Jas. S. Jones, M. D., President ; Robert W. Hubert, M. D., 1st Vice-President ; M. E. Swinney, M. D., 2d Vice-President ; Wm. H. Pilcher, M. D., Corresponding and Recording Secretary ; Sterling Gibson, M. D., Treasurer.

“I will merely state, for the information of those wishing to become members of the Warren County Medical Society, that all applications for membership must be written, and presented to the Society through its Secretary.

WM. H. PILCHER,

“Corresponding and Recording Secretary.”

BIBLIOGRAPHICAL.

Atlas of Pathological Histology. By Dr. GOTTLIEB GLUGE, Professor &c. in the University of Brussels, &c., &c. Translated from the German, by JOSEPH LEIDY, M. D., &c., &c., of Philadelphia. With 320 figures, plain and colored. Philadelphia: Blanchard & Lea. 1853.

Dr. Leidy certainly deserves the thanks of the profession for having placed within the reach of those who do not understand German, the very valuable work of Prof. Gluge. As normal histology constitutes the fundamental study of physiology, so does morbid histology become indispensable in the acquisition of correct views of Pathology. In the present volume, this study is wonderfully facilitated by the numerous and beautifully executed engravings. The work is divided into sections, thus : 1st. Development of the elements of tissues ; 2d. The elements of tissues combined in perfect or imperfect tissues, and arranged according to the processes of disease ; 3d. Formation

of the Blastema ; 4th. The histological metamorphosis of the blood ; 5th. Pyæmia ; 6th. of Gangrene ; 7th. Observations on Histology.

We take pleasure in recommending this work, not only as highly useful, but also as exceedingly creditable to the publishers.

A Treatise on General Pathology. By Dr. J. HENLÉ, Professor of Anatomy and Physiology in Heidelberg. Translated from the German, by HENRY C. PRESTON, A. M., M. D. Philadelphia : Lindsay & Blakiston. 1853.

This work forms a valuable companion, or rather complement to that of Prof. Gluge, for the study of the topics upon which it treats should follow that of pathological histology. Prof. Henlé is by common consent ranked as one of the ablest pathologists of Europe. His work should therefore be perused attentively by practitioners who do not wish to remain ignorant of the advances made in this department.

The Action of Medicines in the System ; or the mode in which Therapeutic agents introduced into the stomach produce their peculiar effects on the animal economy. Being the Prize Essay to which the Medical Society of London awarded the Fothergillian Gold Medal for 1852. By FR. WM. HEADLAND, B.A., M. R.C.S., &c. Philadelphia : Lindsay & Blakiston. 1853.

The title alone of this work indicates the importance of its subject, and the award of the learned society vouches for the faithfulness of the execution. We have not yet had an opportunity to examine it carefully, but as far as we have looked into it, the impression has been very favorable. The general modes of action of therapeutic agents introduced into the stomach are treated of in ten propositions.

PROP. I. That the great majority of medicines must obtain entry into the blood, or internal fluids of the body, before their action can be manifested.

PROP. II. That the great majority of medicines are capable of solution in the gastric or intestinal secretions, and pass without material change, by a process of absorption, through the coats of the stomach, and intestines, to enter the capillaries of the Portal system of veins.

PROP. III. That those medicines which are completely insoluble in water, and in the gastric and intestinal juices, cannot gain entrance into the circulation.

PROP. IV. That some few remedial agents act locally on the mucous surface, either before absorption, or without being absorbed at all. That they are chiefly as follows:

a. Irritant emetics. b. Stomach anæsthetics. c. Irritant cathartics.

PROP. V. That the medicine, when in the blood, must permeate the mass of the circulation, so far as may be required to reach the parts on which it tends to act.

That there are two possible exceptions to this rule :—

a. The production of sensation or pain at a distant point. *b.* The production of muscular contraction at a distant point.

PROP. VI. That while in the blood the medicine may undergo changes, which in some cases may, in others may not, affect its influence. That these changes may be—

a. of Combination. *b.* of Reconstruction. *c.* of Decomposition.

PROP. VII. That a first class of medicines, called Hæmatics, act while in the blood, which they influence. That their action is permanent. 1. That of these some, called Restoratives, act by supplying, or causing to be supplied, a material wanting, and may remain in the blood. 2. That others, called Catalytics, act so as to counteract a morbid material or process; and must pass out of the body.

PROP. VIII. That a second class of medicines, called Neurotics, act by passing from the blood to the nerves or nerve-centres, which they influence. That they are transitory in action. 1. That of these some, called stimulants, act so as to exalt nervous force, in general or in particular. 2. That others called Narcotics, act so as first to exalt nervous force, and then to depress it; and have also a special influence on the intellectual part of the brain. 3. That others again, called Sedatives, act so as to depress nervous force, in general or in particular.

PROP. IX. That a third class of medicines, called Astringents, act by passing from the blood to muscular fibre, which they excite to contraction.

PROP. X. That a fourth class of Medicines, called Eliminatives, act by passing out the blood through the glands, which they excite to the performance of their functions.

Silliman's Journal—The May number of this national work is exceedingly interesting. Among its original papers we note those of Geo. Mathiot, "On the Electrotyping operations of the United States coast survey"—of Ogden N. Rood, "On a method of exhibiting the Phenomena of Diffraction with the compound microscope"—of J. B. Upham, M. D., "On the Phenomena and Laws of Sound"—of Prof. Wm. A. Norton, "On Ericson's Hot-air or Caloric engine."

Meeting of the American Medical Association, at New York.—The session was opened on the 3rd of May. Dr. Welford, of Virginia, President, in the Chair. The Delegates in attendance numbered upwards of 500. The following officers were elected for the ensuing year: President—Dr. Jonathan Knight, of Connecticut. Vice-Presidents—Dr. Usher Parsons, of Rhode Island; Dr. Lewis Condit, of New Jersey; Dr. H. R. Frost, of South Carolina, and Dr. R. L. Howard, of Ohio. Secretaries—Drs. E. L. Beadle, of N. York, and E. L. Lemoine, of Missouri. Treasurer—Dr. D. F. Condie, of Philadelphia.

Medical Society of the State of Georgia.

SAVANNAH, GA., April 13th, 1853.

The Medical Society of the State of Georgia met at 10 o'clock this morning in St. Andrew's Hall, and was organized by calling Dr. R. Q. Dickenson, of Baker county, to the Chair. The roll of members having been called, the rules were suspended, and eighteen gentlemen admitted to membership in the Society. An election for officers was now held, and the following gentlemen duly elected officers of the Society for the ensuing year :

President—Dr. P. M. KOLLOCK, Savannah, Chatham Co.

1st V. President—Dr. R. Q. DICKENSON, Albany, Baker Co.

2d “ “ Dr. G. F. COOPER, Perry, Houston, Co.

Cor. Secretary—Dr. W. N. KING, Savannah, Chatham Co.

Rec. “ Dr. D. C. O'KEEFFE, Greensboro', Greene Co.

Treasurer—Dr. C. B. NOTTINGHAM, Macon, Bibb Co.

Delegates to American Medical Association.

Dr. E. L. Strohecker, of Bibb Co.	Dr. T. R. Lamar, of Bibb Co.
“ J. C. Gilbert, “ Houston,	“ J. E. Dupree, “ Twiggs,
“ H. F. Campbell, “ Richmond,	“ S. W. Burney, “ Monroe,
“ R. C. Mackall, “ Chatham,	“ M. G. Slaughter, “ Cobb,
“ R. McGoldrick, “ Bibb,	“ Th. Stewardson, “ “
“ L. A. Dugas, “ Richmond,	“ J. M. Simmons, “ Spalding,
“ Ch. West, “ Houston,	“ T. M. Darnell, “ DeKalb,
“ T. B. Hoxey, “ Muscogee,	“ Jno. Le Conte, “ Clark,

Dr. Long, of Athens, stated that he had published, in the Southern Medical and Surgical Journal, for 1849, his claims to priority of discovery of the anæsthetic properties of Sulphuric Ether, and he now presented the subject to the Society to take such action thereon as may seem to it proper. Whereupon, on motion of Dr. Dickenson, a committee of three (Drs. Dickenson, Cooper and S. N. Harris) was appointed to confer with Dr. Long, and investigate his claims to the discovery in question.

On motion of Dr. Nottingham, a committee of one from each Congressional District represented at this meeting was appointed to prepare business for the action of the Society. Drs. Arnold, Hillsman, Gilbert, Long, O'Keeffe and J. Harriss, were appointed that committee.

A proposition was read from Dr. J. S. Wilson, of Alabama, to publish a Medical Almanack, detailing the evils of quackery, and expounding some of the fundamental principles of medicine, to be edited by him, and published at the expense of the Society.

On motion of Dr. Dickenson, the Corresponding Secretary was instructed to inform Dr. Wilson that his proposal was duly considered, but respectfully declined.

The Corresponding Secretary read a letter of resignation from Dr. H. A. Ramsay, which, on motion of Dr. Cooper, was laid on the table for the present.

On motion of Dr. Nottingham, a standing committee of three was appointed, for the current year, to investigate the finances of the Society, and report thereon during the present session. Drs. Charters, J. Harriss and McGoldrick, committee.

On motion of Dr. Bulloch :

Resolved, That the gentlemen connected with the Press of the city be invited to take seats within the Hall, during the sittings of this Society.

On a call from the Chair for written communications, the following Reports were read, and received :

On the existing Laws of Georgia, relating to the Practice of Medicine, and the Sale of Drugs, with suggestions for additional legislation—by Dr. R. Q. Dickenson.

On the Topography and prevalent diseases of the First Congressional District—by Dr. P. M. Kollock.

On the Topography and prevalent diseases of the Third Congressional District—by Dr. G. F. Cooper.

On the remittent peculiarities assumed by Typhoid Fever in Georgia—by Dr. H. R. Casey.

On the best plan of treating Fractures in Country Practice—by Dr. L. A. Dugas.

On the anti-periodic properties of Sulphate of Cinchona—by Dr. Thos. W. Bell.

On the treatment of Strictures of the Urethra, by rapid and free dilatation, illustrated with cases—by Dr. P. F. Eve.

Biographical Sketch of the late Dr. Waring—by Dr. C. W. West.

Biographical Sketch of the late Dr. Baber—by Dr. C. B. Nottingham.

On motion of Dr. Mackall, it was

Resolved, That all the Reports received, and now upon the table, be referred to a Committee on Printing, who shall have discretionary power to print, or omit to print, such portions of them as they shall deem expedient.

Drs. Mackall, Arnold and Posey, were appointed under this resolution ; and on motion, the President, Dr. Kollock, was added to the committee.

Dr. Dickenson read the report of the committee on Dr. Long's claims, which was highly favorable, and the following resolution, appended thereto, *unanimously* adopted.

Resolved, That it is the opinion of this Society that Dr. C. W. Long, now of Athens, Georgia, was the first person who used Sulph. Ether as an anæsthetic agent in surgical operations; and as an act of justice to Dr. Long, individually, and to the honor of the Profession of our State, we recommend him to present his claims to priority in the use of this most important agent to the consideration of the American Medical Association at its next meeting.

Dr. S. N. Harris offered the following resolution which was adopted:

Resolved, That a Committee of five be appointed from this Society, to address the next Legislature of Georgia, and urge upon their consideration the propriety of enacting a law which shall exempt its members from Jury duty, and, except in time of actual war, from military duty, so long as they shall continue in this Society.

Dr. C. W. West moved that all the members of the Society sign this petition. Carried.

Committee appointed under the above resolution—viz: Drs. S. N. Harris of Chatham Co., I. E. Dupree of Twiggs Co., L. A. Dugas of Richmond Co., H. V. M. Miller of Floyd Co., Thomas Hoxey of Muscogee Co.

Dr. Arnold called up Dr. Ramsay's letter of resignation, and offered the following preamble and resolutions, which were adopted, *nem. dis.*

WHEREAS, a communication has been received from Dr. H. A. Ramsay, tendering his resignation as a member—and, whereas, Dr. Ramsay was admitted into this Society, as will appear from the minutes of the meeting of 1852, on the express ground that he had retracted his charges against the Society theretofore published by him; and, whereas, since his admission, he has renewed his charges of a persecution, which exists only in his own brain, and has accompanied those charges with scurrilous abuse of several members of the Society—thus violating at once, professional and social courtesy, which charges he has printed and circulated in various forms:

Resolved, That this Society cannot accept the resignation of Dr. Ramsay, and thus indirectly endorse the propriety of his conduct while a member of the Society.

Resolved, That agreeable to the amendment of the Constitution, adopted at the meeting of 1851, Dr. Ramsay be notified, in writing, that at the next regular meeting he will be arraigned before the Society, as having grossly violated the code of Medical Ethics adopted by it for the government of its members, by various publications made

by him during the past year, derogatory to the views and purposes of the Society, and the character of some of its members.

On motion of Dr. Arnold, a committee of three was appointed to notify Dr. Ramsay of the passage of these resolutions.

Dr. Arnold moved to amend the Constitution, by striking out from the 8th Article the words, "of not more than two dollars each."

Carried.

On motion of Dr. Howard, Dr. S. N. Harris, of Savannah, was appointed Speaker for the next annual meeting of the Society, and the city of Macon selected as the place of meeting.

On motion of Dr. O'Keeffe, Dr. G. F. Cooper, of Perry, was appointed Dr. Harris' alternate.

The President announced the following Committee of Arrangements:—Drs. E. L. Strohecker, H. K. Green, T. R. Lamar, G. Harrison and R. B. Nesbit.

On motion of Dr. O'Keeffe, it was

Resolved, That the thanks of this Society are hereby tendered to the Georgia Medical Society, for their attention and courtesy to its members during their stay in the city.

On motion of Dr. Nottingham:

Resolved, That, to the professional gentlemen of Savannah, the members from the interior cannot fail to cherish feelings of the most kindly regard, in consideration of the cordial reception, affluent hospitality, and high toned spirit of fellowship, with which our arrival has been greeted, and our sojourn rendered pleasant.

Dr. Arnold offered the following resolutions, which were adopted;

Resolved, That an assessment of \$3 00, be made upon each member of this Society to defray the current expenses.

Resolved, That the Treasurer notify all absent members of the above assessment, and request those delinquent for any past assessment to remit their dues to him.

Resolved, That the present Treasurer notify Dr. Alexander, of Atlanta, and Dr. Black, of Augusta, (former Treasurers of this Society,) of their delinquency, and request them to forward to him a full statement of the receipts and expenditures of all monies received by them, and the balance of funds, if any, in their hands.*

The Chairman of the Committee on business, Dr. Arnold, presented the following Report, which was adopted:

Subjects to be reported on at the next annual meeting.

* It is but justice to Dr. Black, to state that he sent his annual Report by mail to Savannah, to the address of the Recording Secretary, during the recent meeting of the Society, but did not reach its destination in time to obviate the passage of this resolution in relation to himself; and from the known financial accuracy of Dr. B. we entertain not the least doubt of its being correct and satisfactory.—(*Rec. Sec'y.*)

Medical Botany of Georgia. Dr. Joseph Le Conte.

On the relative value of Lithotripsy and Lithotomy. Dr. L. A. Dugas.

On the use of Anæsthetic Agents in Obstetrical Practice. Dr. Jos. A. Eve.

On the mutual relations of Yellow and Bilious Fever. Dr. P. M. Kollock.

On the Therapeutical value of the Bark of the *Cornus Florida* (Dogwood), as a substitute for Quinine. Dr. C. B. Nottingham.

On the *Veratrum Viride*, as a medicinal agent. Dr. G. F. Cooper.

And that each person, thus appointed, be authorized to chose his alternate, in case he is not able to perform the duty.

At 8 o'clock of Thursday night, Dr. Juriah Harriss, of Augusta, delivered, to a highly respectable audience, a chaste, instructive, and scientific address, on the History of Medicine in the 18th and 19th centuries.

On motion of Dr. Arnold:

Resolved, That the thanks of the Society be tendered Dr. Harriss, for his interesting address before them, and that a copy be solicited for publication.

Hereupon, the Society adjourned, *sine die*.

D. C. O'KEEFFE, Rec. Sec.

Vermont Medical School.—A cotemporary states that a third Medical School is about to be organized in Vermont—and adds, that “if the three will unite, they may make *one* very respectable school. Rather a hard hit at our Eastern brethren.

Prof. M. Clymer has resigned the Chair of Practice in the University of New York—and Dr. John A. Swett has been appointed in his place.

Dr. Joseph Leidy has been appointed to the Chair vacated by the death of Prof. Horner. A very good selection.

Dublin has lost one of her ablest physicians by the death of Prof. Robert J. Graves.

Dr. Valentine Mott and Dr. John C. Warren have been elected foreign members of the French Academy of Medicine. Why was Dr. Mott not elected to the Presidency of the American Medical Association? He was certainly entitled to the honor.

SOUTHERN MEDICAL AND SURGICAL JOURNAL.

Vol. 9.]

NEW SERIES.—JULY, 1853.

[No. 7.]

PART FIRST.

Original Communications.

ARTICLE XXII.

A brief Summary of my Experience with the Veratrum Viride. By JOHN S. WILSON, M. D., of Airmount, Clarke county, Alabama.

So much has recently been written upon the veratrum by Dr. Norwood, and others, that some may begin to grow weary of the subject; but I feel well assured that such is not the case with those who have witnessed its wonderful powers, and that every physician who has enjoyed this privilege will read with pleasure any additional evidence in its favor, which may tend to bring it more into notice, and thus extend its use. And if any of the readers of this Journal should entertain the idea that an undue proportion of its pages are devoted to this new remedy, they should remember that almost *all* that has been written, up to this time, upon this great medicine, has been published in this Journal *alone*; that but few American and *no* European journals have as yet noticed it; and, consequently, that its transcendent claims to the attention of the profession are still comparatively unknown. Instead, then, of offering an apology for introducing the American hellebore again, I would pay a merited compliment to Dr. Norwood, by saying that he has won immortal honor for himself, and conferred an inestimable favor on mankind, in bringing it first into notice; and I hesitate

not to say that the Journal would be well filled if *every page* contained nothing besides an important account of the effects of *veratrum viride*.

It is not my design to give a minute and tedious detail of cases; I will only mention some of the diseases in which I have used the medicine under consideration—the mode of administration, and the result, together with such general remarks as may be suggested to me in common with each case.

At the beginning of the present year, I succeeded in obtaining some of the root of the *veratrum viride*, after several ineffectual efforts: from this I made a tincture, according to the directions of Dr. Norwood, by adding $\frac{1}{2}$ lb. of the root to 16 oz. of undiluted alcohol. This tincture I have used in the following diseases—viz: Pneumonia, pleuro-pneumonia, bronchitis, broncho-pneumonia, pericarditis, cerebro-spinal meningitis, epileptiform convulsions, acute rheumatism, general dropsy; and in fevers—typhoid, remittent and puerperal. Of the thoracic diseases, sixteen were pulmonary and one cardiac: of meningitis, convulsions, acute rheumatism, and dropsy, there was one case each: of the fevers, three were typhoid, one puerperal, and several remittent. Of the sixteen pulmonary cases, all recovered save one, and this was complicated by the supervention of acute rheumatism. The case of pericarditis had resulted in effusion, before it was put under treatment, and terminated fatally: still the *veratrum* exerted its wonted control over the pulse, reducing its frequency from 140 to 60. The case of cerebro-spinal meningitis was also fatal; but as in the other case, the remedy exerted a marked control over the pulse, and for a while there was a flattering amelioration of all the symptoms.

The subject of the convulsions was an anemic boy of 10 or 12 years of age; the spasms were very frequent and extremely severe, every paroxysm apparently putting the life of the patient in imminent danger: the *veratrum*, to reduce the frequency of the pulse, and ether inhalations, to quiet the spasms, were the remedies almost exclusively relied upon; and these indications were fulfilled in the happiest possible manner, snatching the little sufferer from the very jaws of death.

The case of rheumatism occurred in a little boy about 5 years

of age; it attacked the knee-joint, was very severe, and was treated principally with colchicum and veratrum; but the former medicine did not succeed well until the latter was added to prescription. Under this treatment the disease was subdued in less than six days, instead of running "six weeks," according to the dictum of Dr. Warren.

The case of dropsy was fatal, the patient being moribund when the veratrum was commenced.

We now come to that interesting class of diseases—fevers. Of the three typhoid cases, one terminated favorably in twelve days; but the early convalescence in this case was not attributed to the veratrum, as this was not made a principal remedy, and as the fever was unusually mild. The other two cases were severe and protracted, one continuing three and the other six weeks. Having much confidence in the veratrum, it was relied upon almost exclusively in the treatment of these two cases, the prescriptions being complicated as little as possible. In the case that terminated in convalescence in three weeks, slight ptialism supervened, from the use of small doses of calomel in conjunction with the veratrum.

The general plan of treatment which I have adopted in typhoid fever is to give quinine freely for the first few days, or until the typhoid symptoms are fully developed; I then resort to small doses of calomel and the veratrum, experience having taught me that nothing can be gained by pushing the quinine, *after the disease has become distinctly typhoid*. In giving the veratrum, I generally prescribe 4 gtt. every three hours, for an adult, to be increased 1 gtt. each dose, until a decided effect is produced, either upon the pulse or stomach. I have never been able to carry the dose higher than 14 gtt., and seldom higher than 8 gtt.; and I have even known 2 gtt. to produce a very manifest effect on the pulse of a little girl three years of age. I have never known it to fail in reducing the frequency of the pulse when it produced nausea, but I have frequently succeeded in reducing the pulse, without any nausea, by beginning with very small doses, and increasing gradually.

Having mentioned the diseases in which I have used the veratrum, and the mode of administration, I conclude with a few general remarks.

It will be seen by reference to the figures given, that my experience with it has been more extended in pulmonary diseases than in any other class—sufficiently so, I think, to speak with some degree of confidence. As already stated, only one of those cases proved fatal, and that was complicated. Some of the cases were severe and alarming, as much so as any I have ever seen; and one of the cases of pneumonia was accompanied with active pulmonary hemorrhage. With these facts before me, I can but express a very favorable opinion of the veratrum in pneumonia and bronchitis. Before the introduction of this remedy, I treated the diseases mentioned with antimonials, and generally with success; but every physician of experience knows that pneumonia is not unfrequently complicated with a degree of intestinal irritation which forbids the use of tart. emetic on the Rasorian plan, or, indeed, in any way capable of subduing the pulmonary inflammation: and even where this intestinal complication is not an essential feature of the disease, it is well known that the free use of tart. antimony will produce it, together with other grave, and even fatal symptoms. The veratrum is not obnoxious to those objections; and having in addition to the important negative advantage, the positive one of being more prompt and certain in its effects, than the antimony, I think it entitled to the preference; and I think it possible that time and experience will confirm its claims, and that it will be recognized as the great desideratum in the treatment of pulmonary diseases. In this class of diseases I have found it to act promptly, not only as a sedative, but also as an expectorant and anodyne; and I would here remark, that I consider its primary action, that which gives it its great value, sedative—while its secondary effects are expectorant, emetic, sudorific, and anodyne.

As to its use in fevers, I have but a few more words, as it is my design to confine myself mostly to facts, and as my experience with it in this class of diseases has not been extensive. As before intimated, the veratrum was made the principal remedy in the treatment of two of the typhoid cases—no other remedies being used except blisters to the abdomen, cold sponging, spts. mindereri, and in one case small doses of calomel. If I may venture an opinion as to the virtues of vera-

trum in typhoid fever, from its effects in those two cases, it is this: Its powers in abbreviating the fever are doubtful; but its control over the pulse, the reduction of excessive arterial action, and the consequent mitigation of all the more distressing symptoms, are, comparatively, certain—far more so than any remedy previously in use. But, while I express myself thus favorably, I do not think the veratrum is applicable to *all* cases of typhoid fever; for I would be averse to its use in cases attended with extreme debility and great prostration of the vital powers: if I were to resort to it at all in cases of this kind, it would be in doses too small to produce nausea and prostration, observation having taught me that the patient must have a considerable degree of recuperative energy to withstand its depressing effects, unless it is used with great caution.

The case of puerperal fever was one in which the uterus was the primary seat of inflammation, but the fever was complicated with diarrhœa and well-marked typhoid symptoms. In consideration of this complication and the advanced stage of the disease, I withheld the lancet—of course the purgative plan was wholly inadmissible. Under these circumstances, I prescribed small doses of calomel and opium; veratrum alternately—the latter to be increased until it affected the pulse or stomach. When this treatment was begun, the pulse ranged from 120 to 140, quick and thready: after continuing it forty-eight hours the pulse was reduced nearly to the normal standard, and kept at this until the metritis was subdued. In this case it took as many as 14 gtt. to fulfil the indication—more than I have ever given in any other case, except one of pneumonia, accompanied with pregnancy—the time was much longer than usual also; but whether the puerperal and pregnant states modify the action of the remedy, I am unable to say.

As we have a specific in remittents, of course the veratrum was not the principal remedy in this form of fever: it was only used during the febrile exacerbation, to moderate the arterial action, and this design it accomplished very happily; and although it may possess no specific action in any form of fever, I consider it an invaluable remedy in controlling the action of the heart and mitigating the intensity of the disease, in every variety of fever, idiopathic or symptomatic.

I might multiply remarks in connexion with each of the cases treated, but enough has been said, I think, to convince the most incredulous that the powers of the veratrum have not been exaggerated. I will only add, in conclusion, that I have used it, not only in the twenty-five cases reported in this article, but also in others, not embraced in this report; *and I have never seen it fail in reducing the frequency of the pulse—while there was generally an improvement in its volume:* and although I am naturally more prone to skepticism than credulity, I hesitate not to express the belief that the veratrum viride is one of the greatest additions made to the materia medica the present century, the anæsthetics not excepted: but, while I bestow this high commendation, I must say, by way of caution, that it is potent for evil as well as for good—like all other active remedies—and should therefore be used with prudence and circumspection.

ARTICLE XXIII.

On the proper use and abuse of Medicines. By J. A. LONG, M. D., of Long's Cross roads, Tenn.

To what extent remedial agents may be used in the arrest of disease, without hazarding any thing to the life or constitution of the patient, is a question of vital import, and one to which I fear too little attention has been paid. That diseases are, to a certain extent, under the control of remedial agents, no one familiar with practical medicine will for a moment doubt; but to what extent they are so, is the object of enquiry. That too much may be done, and no doubt often is, in the persevering use of medicine, is certainly a settled fact—more especially by the junior class of the profession, the reckless and the ignorant. Too great a reliance is placed in drugs, and that often to the exclusion of other invaluable means or adjuvants—such as a well regulated dietetic course, nursing, bathing, &c. No man thoroughly imbued with the true principles of medicine and the nature of disease, but knows that there is a strong natural tendency, in a large proportion of cases, to terminate in recovery when left alone to nature's own resources. And it is

one of the popular errors of the day, and probably has been of all ages, to suppose that all recoveries are cures ; and this is the great *basis* upon which quackery rests and the *faculty proper* not only suffer the imposition upon the popular mind, but their silence gives consent : for, in general, we are too ready to admit that all favorable changes are due to the virtues of our medicines, and still more ready to encourage and foster such erroneous opinions, in the minds of others less qualified to judge.

But few young practitioners enter upon the responsible duties of our profession but what are more or less disappointed in the curative powers of medicine : for they have been taught, both by authors and lecturers, that drugs alone could save ; and, in the language of one writer, “ It cannot be doubted that diseases which if undisturbed would have spontaneously terminated in health, have often received an unfavorable turn, from officious interference. If we would study the dangers as well as the virtues, of each and every drug we use, and their relative powers upon the system, in its varied conditions, we would be better qualified to do good and avoid evil. We should ever make it a rule, (and strictly adhere to it)—1st, to do no harm ; 2dly, to do all the good possible in our power. We are not to suppose that drugging is essentially necessary in all cases of disease, for this will often lead to their abusive employment. Medicines, taken as a whole, are not harmless agents, but partisans, acting for or against the patient in a large proportion of cases. How careful, then, ought we to be in their selection, preparation and administration. I do not believe we are justifiable in the use of a drug, without we fully comprehend its therapeutic action, and there is an indication for such medicine in the case under treatment. More risk is certainly incurred in the administration of medicine, in whatever stage of disease, where there is no plain indication, than there possibly can be, in withholding such medicine, until an indication presents itself, sufficiently clear to the practitioner for him to take action with some certainty of success. I do not wish to make an impression upon the reader that I am opposed to the use of drugs, in the treatment of disease, when judiciously employed, for no man believes stronger in the controlling power

of remedial agents over disease than the writer, when properly used and well timed ; but it is to their extravagant and lawless use I wish to call attention. If the taking of medicines, in case of disease, were as plainly indicated as that of aliment in health, then we could not only give the proper quality, but necessary quantity, to effect a radical cure in all cases. But unfortunately this is not the case, for after whole years of attentive and laborious study, and observation, in the boundless plains of our science, we are still left to grope in the dark : hence it is the more necessary to be extremely cautious in the practice of our profession, to guard us against untimely and needless prescriptions, ignorantly or otherwise. Here we might take up the long list of the *materia medica*, and examine each and every drug separately—at least all such as hold any claims to potency—and enquire into their history of use, and abuse, upon the human system ; but a few examples must suffice, with a passing reference to others.

Who of us, that have practiced five years, have not seen patients prostrate under the use of *calomel*, *tartar emetic*, *opium*, *chloroform*, &c. There is nothing more common in this section, than the hourly administration of *calomel*, in the treatment of fevers, and the whole list of phlegmasia, to induce salivation in doses from 1 to 20 grs., and the use of this drug is persevered in until this object is accomplished, or the patient prostrated.

CASE I. (587 of my case book.) I was called to R. N., attacked with bilious fever. This case proved obstinate, and as I was young and inexperienced, Dr. ——— was requested in consultation. He decided that my patient was in a dangerous condition, and of decided bilious temperament ; liver inactive ; and that calomel was imperatively called for, to fill two plain indications that presented in his case : it would bring about action in the liver, and disengage the portal circulation, and lay a good foundation to induce salivation, if that very desirable object became necessary, from the obstinacy of the case. 4 grs. calomel, opium $\frac{1}{2}$ gr. was ordered every four hours, for twenty-four hours ; at the end of which time we visited the patient, and found him somewhat worse. The Doctor attributed this, of course, to the obstinacy of the disease, and the

entire inefficiency of the doses given—consequently he ordered 10 grs. calomel, $\frac{1}{2}$ gr. opium, every three hours : at the end of twelve hours we saw our patient again, and found him in *articulo mortis*. The Doctor was still of opinion that the calomel was not given in sufficient doses, or the intervals were too distant. Up to this time, I had, from my *inexperience* in practical medicine, left the prescriptions entirely to his own judgment ; but being decidedly, by this time, of opinion that the medicine was sinking him, instead of relieving, I opposed its further use, which led to the calling in of Dr. P., an old and *experienced* physician.

After a very minute examination of the patient, and our course of treatment, he said, “ You cannot *force diseases* to yield—rather persuade. He ordered some stimulants to be given first, until the patient should somewhat revive—then he gave *pil. hydrarg.* grs. 3, Doveri grs. 5, every four hours, until three doses were given, using at the same time other adjuvants, such as, blisters, injections, &c. The patient soon convalesced, and rapidly recovered his former state of health.

In the case just related, not a shadow of a doubt was left on my mind, but this patient must inevitably have been lost under the *wholesale calomel* treatment. The Doctor just called in, was a man of some seven or eight years practice, and a graduate of the Jefferson Medical School, Philadelphia.

CASE II. (965 of my case book.) I was called for, in haste, to see S. R., a little daughter of H. C., who had been given up by her attending physician. He had treated her for typhoid fever. Upon examining the little patient, I found she had been taken with *cholera-infantum*, and the only symptom present that possibly could have been taken for typhoid fever was the *diarrhœa*. I examined the medicines that had been left, and which I was told had been administered all the time : they consisted of small powders, to be taken every three hours, composed of about 2 grs. calomel and 2 grs. aloes, which had been taken for some seventy-two hours, and the little sufferer (for it was only about two years old) was perfectly prostrated with excessive purgation, from the worse than needless use of medicine. In this case, the attending physician had been at-

tributing the symptoms to the obstinacy of the fever; hence his perseverance in the use of his remedies. The powders in this case were discontinued; a check given to the bowels in the usual way in cases of *cholera infantum*; frequent warm bathing; a flannel bandage was placed around the abdomen, and the bowels directed to be opened in six hours by a dose of warm castor oil, with proper directions as to its diet, was all that was done; nor was it necessary for me to visit the patient a second time—it was going about on the third day.

CASE III. (1178 of my case book.) On the 20th February, 1845, I was requested to see a little girl about 5 years old, the daughter of H. B., under the care and treatment of Dr. B., for *typhoid fever*. He had left her, and stated that if the medicines left did not relieve the patient he could do no more. I found the patient with high fever, dry tongue, severe pain in the side, and obstinate *diarrhæa*. A troublesome cough had attended her from the onset of the disease. The medicines left consisted of calomel 2 grs., jalap 3 grs., as near as I could tell, to be taken every three hours, until the patient was better, or the whole amount was taken—fifteen doses. Eight doses had been given when I first saw the patient: I found it in an extremely prostrate condition, and would certainly have sunk before the remaining powders were consumed. This patient was suffering under *influenza*, which was then prevailing in the country. I ordered a discontinuance of the powders, checked the bowels with small doses of Doveri, directed fomentations to the painful side; bowels to be opened on the following day by oil or injections. This patient convalesced immediately, and was about in few days.

My note book abounds with such cases; but enough has been given to illustrate my position, and to arouse the interest and energies of those who wield more able pens than myself. The history of the treatment of *typhoid fever* alone, is humiliating in the extreme, to the faculty; for I suppose not a remaining doubt is left upon the mind of any practitioner that the evil arising from the abuse of medicine, by its extravagant use in this disease, far outweighs its benefits. The mortality in this

section of country, in the hands of some practitioners, (more especially those of northern schools,) has been over one hundred per cent., and in some neighborhoods all the patients attacked have died—to the no small *disgrace* of the medical profession. Such results are doubly afflicting, when we reflect and consider the strong natural tendency this disease has to terminate in recovery, if undisturbed and left alone to nature's own resources.

ARTICLE XXIV.

Fetus in Utero, killed by Lightning. By JAMES Y. CARITHERS, M. D., of Hendricksville, Alabama.

Mrs. F——, aged about 40 years, in good health, and eight months advanced in pregnancy, received on the 10th of June, 1852, a severe shock from a streak of lightning, from which she recovered in a few hours—when she was attacked with labour pains which caused me to be sent for. On my arrival, I found her suffering with sharp pains. On examination, per vaginam, no dilatation of the os uteri had taken place. Bled her freely, and ordered her an enema of a gill of starch, with a teaspoonful of laudanum, and to take $\frac{1}{4}$ of a gr. of sulph. morphine every half hour, until she was relieved from pain. After taking the fourth dose the pains subsided. Ordered her to take on the following morning ol. ricini, $\overline{3}$ i. At 2 P. M., oil acted freely on the bowels, and at 4 P. M. I found her resting well. Allowed some light nourishment, from that time until she was delivered, which took place on the tenth day after she complained of being very unwell. The child was dead, and from the appearance, had been so from the time the mother felt the shock.

Eclectic Department.

Letters upon Syphilis. Addressed to the Editor of *L'Union Medicale*, by P. RICORD. Translated from the French, by D. D. SLADE, M. D.

[Continued from Page 361.]

FOURTEENTH LETTER.

My Dear Friend—What did I wish to prove to you in my last letter? That observation had by no means demonstrated the contagion of syphilis from the nurse to the child, and from the child to the nurse, without the presence of primary accidents: that nothing was less established than that pretended contagion of secondary accidents, and that in all the cases invoked as a proof of this mode of transmission, either the essential details were wanting to bring about conviction, or evidently it was a question of primary accidents.

Mark well, I beg of you, that I do not reject in an absolute manner this mode of the transmission of syphilis. I only say, not quitting the field of strict observation and the rigid analysis of facts, that the existence of this mode of transmission is not yet proved, and I add that if it is ever proved, it will only be by inoculation; inoculation alone being able to furnish the undeniable demonstration of this, and to put the subject forever at rest.

But are you going to say to me—do you forget, then, that some persons pretend to have proved by inoculation even, the contagious properties of secondary accidents? No! certainly not, I have not forgotten it. I wish that I could. I should not thus find myself under the painful obligation to cast too legitimate doubts upon the experiments made by men whose works I honor, but who appear to me to have concluded upon this subject with a little precipitation.

Wallace has published two observations of secondary inoculation followed by results which appear positive. This writer upon syphilis well says (*Syphilidologie* de Behrend, 1841, page 60 et suiv.) that he has determined in healthy individuals inoculated with pus taken from patients laboring under the influence of secondary accidents, first, primitive, followed later by confirmed secondary accidents. It is very certain that as far as effects produced and as results, the observations of Wallace have at first something plausible. But what is not demonstrated at all, is the nature of the accidents reputed secondary in the patients from whom the inoculated pus has been taken. Here the most important details are wanting. They are content with saying that in the first observation the patient had syphili-

tic psydracious pustules of fourteen days' standing. In the second observation the same pustules are mentioned as dating from four weeks, and forming little crusts. In the first case the subject was inoculated upon the shoulders; in the second, upon the prepuce.

But, first nothing proves that the psydracious pustules from which Wallace had taken the pus were secondary accidents. The form, the number, the seat of the pustules, would not suffice to give them this character; for this, another thing is necessary, which we do not find in the observations of Wallace.

On the other hand, what precautions did he take after having inoculated? In a venereal hospital, where we find the virulent matter everywhere, the subsequent contacts are so easy, if after artificial inoculation the punctures are not guarded from every contact, as we are in the habit of doing, by placing them under a watch-glass, and causing this *syphilitic grain* to germinate under cover; if the instruments of which we make use have not been washed with the greatest care; if, in a word the most minute precautions have not been taken, it is impossible, in circumstances so serious and important, to draw strict conclusions.

I am much the more exacting in these observations of Wallace, inasmuch as there passed something unusual in the results of the inoculation.

In the first subject inoculated, the 15th November, *it is not until the 14th December following*, that there formed upon the place of the inoculation a little papule, covered with crusts below which a small superficial ulcer was discovered. From this the evolution of the symptoms described by Wallace, and which might have an entirely different origin.

In the second subject inoculated upon the prepuce the 1st of June, *it is not until the 28th of June* that a little crust of a dirty-yellow color, surrounded by an areola, is found upon the parts until then abandoned to themselves without any precautions. The glands in the two groins are swollen, the spot covered with crusts is scarcely excoriated; the 24th July, the entire body is covered with an exanthema, the characters of which appear to be syphilitic. At a later period, some accidents are discovered about the anus, the origin of which is not ascertained; without doubt from the description, these accidents greatly resemble the mucous tubercles, and these tubercles exist also upon the scrotum, upon the back of the tongue and upon the tonsils; but the raphé of the patient is *red and much tumefied*; the patient says that in walking, *a very considerable oozing escapes from the anus*. Now, the tumefaction of the raphé and the intra-anal suppuration are often met with in the chancre or

primary ulcer of this region. The primary accident contracted à *prepostera venere* has for its favorite seat the anterior portion of the anus where the raphé meets it. There is, then, in the case of this patient, more probability for the existence of a primary accident which had commenced in that region, and about which no previous inquiries had been made, than there is in placing the commencement of the disease in what had been observed upon the prepuce, which had not presented any of the symptoms by which syphilis commences. I add that in well-made inoculations, the evolution of the symptoms may be sometimes slow, but it is always constant, and we never see the interval of a *month or twenty-eight days* between the inoculation and the appearance of the accidents.

Thus my dear friend, what motives there are for doubt in these two observations of Wallace! After the analysis that I have just made of them, I cannot think that they will still serve as a support to the doctrine of the inoculation of secondary accidents.

I have just told you of the possibility of *an anal chancre* in the case of the second patient. This supposition appears to me to be so much the more well founded, as that in England they seldom search for this seat of chancre—the English medical customs reflect that sort of far-fetched modesty which characterizes this nation. I recollect that in a trip to London, they showed me at St. Bartholomew's hospital, with a kind of earnestness, some males and females affected with secondary accidents which were considered as the immediate result of contagion. My friend Dr. Acton was present at this exhibition. You are aware that I think infinitely little of constitutional syphilis d'emblée, by way of contagion; so that, making use of my *right of search*, I put myself upon the way. I laugh still at the startled air of the house-surgeon and his assistants, when carrying a bold finger and a scrutinizing look into certain mucous folds, I succeeded in discovering in the *perfidious Albion* a back door. I ought to add, that immediately the house surgeon threw a veil, or, less poetically, let fall the sheet upon these too visible marks of a contagion very easily explained.

To return to Wallace; it is very singular that he who has made such a great number of inoculations, has succeeded in inoculating secondary accidents only in two cases, and that he has so badly demonstrated these. These cases constitute an exception, and there cannot be an exception here. The secondary accidents either do or do not inoculate. Please to recall what I have said upon those cases of blennorrhagia of Bell reputed exceptional; there could not be for them any exception, and experimentation has in fact proved that the *exceptional cases* came under the law of inoculable chancre.

But if the facts which have passed upon the other side of the channel can, as I think to have proved it, raise up very reasonable doubts, here is a fact which has taken place very near me, and which appears to present more value.

It was at the Hospital du Midi that this fact took place. I should not have the liberty to speak to you of this, had not an interested party, too interested in fact, given me the right.

It is concerning secondary accidents inoculated from a patient upon a healthy individual. The inoculation has perfectly succeeded. One of our brethren, who without being a *casuist*, is not, however, favorable to experimental researches, has himself practised this inoculation, and has planted upon each of the fore-arms of one of the internes of the hospital a chancre which has indurated, and which has determined the indolent enlargement of the axillary glands, and which in the four months which followed has given place to perfectly well-characterized secondary accidents, nocturnal cephalalgia, falling out of the hair, scabby eruptions upon the scalp, mucous tubercles upon the velum palati (psoriasis of the mucous membranes,) &c. ; it is the constitutional verole, the least contestable possible, and I have no desire to contest it.

But—and there is all the question—of what nature were the accidents which furnished the pus inoculated? The patient from whom was taken the inoculable matter, according to the observation which has been given me by the interne inoculated was affected with an indurated chancre of six weeks' standing and cicatrized; he had mucous tubercles about the anus—ulcerations about the great toes, pustules accumulated upon the thoracic region; large pustules covered with crusts, below which, ulcerations progressing and having a tendency even to spread, were seen; there existed some of these in the inguinal regions and upon the side of the chest where the principal group was seated.

Before the pupil was inoculated, the pus of these pustules had been inoculated upon the two thighs of the patient himself. This inoculation had given a positive result, a circumstance which, *without a great passion for experimentation, ought to have prevented the inoculation upon a healthy individual.*

This patient had then very certainly a constitutional syphilis, which presented characteristic accidents, and of a nature incontestable. *But were all the accidents in him absolutely of the same nature?* The constitutional verole, as we know, does not in any way prevent the contraction of new primary accidents, accidents unlimited in their number, and infinitely varied in their seat. In this particular case, the accidents from which the pus had been taken: *ulcers increasing, covered with crusts,*

very extensive, in an individual only six weeks under the influence of the syphilitic diathesis, offering in the other regions the regular evolution of secondary accidents of that period, permit me to offer a doubt, which for the student who has undergone the inoculation, is to-day a certainty, viz., that the accidents from which the pus had been taken *were not secondary accidents.*

I did not see the patient who furnished the inoculable pus; he soon quitted the hospital after this experimentation, and *the pupil interested could not find him again.* But the importance of this fact, however contestable it may be, has induced us, my honorable colleague M. Puche and myself, to recommence a series of experiments upon the inoculation of the secondary accidents. We have already made twenty experiments, all of which have only afforded us the results formerly obtained, that is to say, *negative results.* The inoculations have been made with the pus of mucous tubercles, of the ecthyma, of rupia, of ulcerated tubercles, of secondary serpiginous ulcerations; never have we obtained anything. Here, upon this subject, are two curious observations which have had as witnesses the numerous students who follow my clinique.

Two patients, lying side by side, ward 1st, Nos. 16 and 17, had, No. 16 a scabby ulceration upon the axillary region, progressing and serpiginous; the other, No. 17, an ulceration upon the posterior and right side of the neck, of from six to eight centimetres in diameter, progressing, healing in the centre and extending itself in circumference; this patient had still upon other regions, isolated rupia, ecthyma confluent, and upon the greatest part of the trunk and of the limbs he had characteristic cicatrices due to pustulo-crustaceous syphilitic eruptions.

These two patients were inoculated upon the thigh. Upon No. 16 the inoculation succeeded; success had been predicted: upon No. 17, we had announced that the inoculation would be *negative—it was negative.* Why? Because that the ulceration of No. 17 was truly secondary; while in the case of No. 16 the scabby ulcerative eruption of the axillary region, which had the aspect of pustular crustaceous eruptions belonging to constitutional syphilis, had been the result itself of an inoculation; and mark how. This patient had at first a scrofulous abscess in the hollow of the arm-pit; this abscess had been opened at the hospital; the dressing of it was difficult for the patient himself; one of his neighbors, affected with a phagedenic chancre of the genital organs, rendered him the service of dressing it, and with his fingers soiled by *the virulent pus of his own chancre*, had inoculated him. Without the very precise etiology of this case, the patient having had himself formerly symp-

toms of constitutional syphilis, this accident could have been attributed to the diathesis, and have been given as an example of secondary inoculation.

See, then, what care and precautions are necessary in order to avoid error.

FIFTEENTH LETTER.

My Dear Friend—From the numerous observations collected with care, from the many experiments made by myself, and from the more numerous ones, still made after my example, I have the right to conclude, that up to this day, secondary accidents have not been inoculated. I have told you that the new experiments which I have very recently made; that the experiments again repeated by M. Puche and by M. Cullerier, have remained confirmatory of the first. But these experiments having been always practised upon the patient himself, a capital objection might be made against me. It could be said, the secondary accidents cannot be inoculated in those who are already affected; but they can be perfectly inoculated upon a healthy individual. This objection could be made even by those who partake of my doctrines; for I do not think that it has entered the mind of all that school which is opposed to me, and which professes that, so far from syphilis preventing a new contagion, it is sufficient to make a single wound in a syphilitic patient in order that this wound should take on immediately a venereal character. I have already elsewhere said, and I shall ask of you the permission to soon recall what I think of this opinion. However it may be, the first objection remained; and if the observations of Wallace had been more probable, and less contestable, I should have taken the trouble to answer them, for I was completely destitute of experiments which proved the contrary. It is under these circumstances that has been presented the fact of the inoculation from a diseased to a healthy man, of which I have given you a sketch in my last letter. I have spoken of this fact upon the special authority of the person the most interested in it, he who has voluntarily submitted to the experiment, who undergoes the consequences of it, and with a legitimacy which we cannot reasonably contest, raises up pretensions to the scientific right to this fact; who believes in having become absolutely the master of it, and having the right to draw from it all the scientific and practical consequences which he shall judge proper, leaving to all the liberty to do as much; it is, I say, under these circumstances, that I have thought myself permitted to give my opinion upon this fact.

I repeat then, that this fact has appeared to me very grave, very serious, and well worthy of being taken into consideration; this is the reason why I have wished to examine it with care. We do not preoccupy ourselves with common facts, and those without value. This one derives its importance both from the nature of the experiment which could have great influence in the elucidation of grave practical questions, and from the individual who has submitted himself to the experiment. It is an interne in pharmacy, a distinguished and intelligent pupil, who has been occupied with medical studies, and more particularly with syphilis. I considered the fact merited our attention, on account of the experimenter, whose science, talents or character, as you know, dear friend, I have never wished to attack. If needful, you could attest to this. I have always deeply detested attacks of this kind, not only because they have often been unjustly employed against me, but because it is not my custom, and because my disposition is repugnant to it.

In these letters, rapidly conceived, and more rapidly written, benevolent expressions might sometimes fail me, but the intention never. Let this be said once for all, and put to silence susceptibilities which have no right to exist.

I return to the scientific fact which alone occupies me—all the value and all the importance of this fact is in the diagnosis. Has the pus of a secondary or of a primary syphilitic accident been inoculated upon a healthy individual? I believe, I think, and I have given my motives, that from this fact alone, viz., that the patient who furnished the pus has been inoculated positively himself, that this experiment enters completely into the domain of those which I myself have made. Thus, if success has been met with in this case, it is because, according to my numerous experiments, we have had to do with primary accidents. Unless, which I do not contest, *but which remains to be proved*, we have discovered for the inoculation of secondary accidents, *a particular form, a special period*, which until now has escaped us, and which we ought to be able definitely to determine.

For, in fine, this result cannot be an exception, or the effect of chance. If we could succeed in establishing the circumstances in which secondary accidents could be inoculated, and therefore could be contagious, we shall have made a great step in syphilogeny, and rendered a great service to science. In all cases, this experiment will confirm this law—that an accident actually contagious is inoculable; that there is no difference between artificial and physiological inoculation. It would prove that this mode of experimentation can well have some value, and it would be for me a veritable pleasure to see ranged

under this opinion those persons who have made the most of the *uncertainties and the difficulties of syphilitic inoculations*.

Let me tell you, my friend, that I have no intention, as you will see, of changing my position. I do not attack, I defend myself. I do not criticize, I examine. I am not ambitious for the success of the *polémiste*; I hold to the more modest pretensions of the practical observer. Nobody is more ready than myself to receive light, from whatever source it may come; or to recognize the truth, whatever may be the voice that proclaims it. I have always uttered what I knew, or thought that I knew, with loyalty and firmness. My experiments I have never made secretly; they have become the property of all, they have enjoyed the right to see them, to judge of them, and to discuss them, and certainly in justice they have not found fault with them; and without asking me permission, it was a common right. I have entertained opinions which time and experience have modified. I shall cite an actual example of this, and one to the point.

With all the earnest writers on syphilis, past and present, I have thought that *syphilis was not transmissible to animals*. I have made experiments, which like those of Hunter, of Turnbull, and especially of M. Cullerier, who has made more numerous ones, have always conducted to negative results. All these experiments gave me the right to conclude upon the *non-transmissibility of syphilis to animals*, until the contrary is proved.

However, *I was not too hasty to teach and to publish these negative results*, as M. Robert de Welz has imagined, since I had also the essays of Hunter, of Turnbull, of M. Cullerier, and moreover the numerous unsuccessful experiments, publicly stated, of M. Auzias Turenne. M. Auzias has experimented perhaps more than all of us together, and had also more numerous negative results. But more persevering in his researches, he has studied the conditions which could prevent the inoculation of animals; he says that he has recognized them, and that he has at last succeeded in inoculating the primitive accidents from man to the monkey, and in return from the monkey upon man. M. Auzias assures us that one of the principal causes of the want of success was that the animals licked themselves after the inoculation. He had thought, originally, that the saliva neutralized the virus; but this opinion could not be entertained in presence of the numerous instances that we see in man of the primary accidents having for seat the lips, the tongue, and different points of the buccal cavity. The whole secret was, that the animals, in licking themselves, must necessarily cleanse the wound of the inoculation.

But the true reason which must have caused the experiment

to fail, and upon which M. Auzias Turenne insists the most at the present time, is the very great plasticity of the blood in animals, which allows it to interpose itself between the bleeding part and the virulent matter. It is in taking care to constantly soak the wound with pus after the inoculation, that it has succeeded. I have witnessed the experiments, and I can vouch for the authenticity of them. It is with eagerness that I have been able to rectify this point in the history of syphilis, in my clinical lessons.

Until then I had professed, with our predecessors and with our cotemporaries, that syphilis was the unhappy prerogative of man, and yet that it was not spontaneous in him. I have always greatly insisted upon these two facts, which appear contradictory, *specialty of the disease in man, and not spontaneousness*. I have always thought that syphilis had an origin somewhere, and that it was necessary to search for it. Is the problem resolved? The monkeys have not always escaped from wicked insinuations. Already Overcamp and Linder had accused them of playing an evil trick upon the human race, by giving it syphilis; but before M. Auzias, Overcamp and Linder have been considered as calumniators of monkeys. Were they right?

What is incontestible is, that since man was acquainted with monkeys, since he has seen them multiply in the Garden of Plants in Paris and in other capitals, since he has observed them either in a state of nature or in captivity, nothing has ever been seen upon them or among them which resembled primitive syphilis, and more especially constitutional syphilis.

However, M. Auzias has succeeded in planting upon the ear of a monkey a primary ulcer. The pus which served for the inoculation having been taken from a patient in my wards, I ought to note with care the circumstances in which this pus was taken. The patient who furnished it was affected with confluent chancres upon the gland, upon the prepuce and upon the rectum—*chancres non-indurated*, and at the period of specific progress. These chancres were the result of a recent contagion in an individual under the influence of a constitutional syphilis, at the secondary period; and this is very important to note, for according to the principles that I have given out, this circumstance explains why the chancres were not indurated in this patient. Again, these chancres, by their *multiplicity, by the variety of their seat*, could have been, in the eyes of inattentive or of superficial observers, confounded with other constitutional accidents, and served as a pretext to conclude upon the possible inoculation of secondary accidents.

A previous inoculation had been made upon the patient and

had succeeded. It was with the pus of the pustule of the inoculation that the monkey was inoculated the first time. A second inoculation was made upon the monkey with the pus of his first pustule, and this second inoculation again succeeded.

It was then that one of our young brethren interposed. M. Robert de Welz, associated professor of a German university, asked to be inoculated, and was effectually inoculated—first with the pus of the first pustule of the monkey, and then with that of the second. These inoculations succeeded. But until then, the patient who first furnished the pus had not had any specific induration; the monkey whose pustules became a little thickened, *had not presented the certain characters of this induration; the neighboring glands were not enlarged*; in fine our German brother, who of his own accord submitted to a perilous experiment, in whom, moreover, the pustules of inoculation were not destroyed until at quite a late period, has not experienced the *specific induration*. The pustules of inoculation presented at their base, sub-phlegmonous engorgement, very common, but which might often be confounded with specific indurations by inexperienced observers. The axillary glands (the inoculations having been made upon the two arms,) were not enlarged.

For the inoculation at which I assisted, and which was made upon M. Robert de Welz, a new lancet was used, but the pus upon the monkey was taken up with a spatula which was not new. Since then, M. Robert de Welz has made a new inoculation, which succeeded, with new instruments.

Thus far, then, we have only purely primary accidents, essentially local; but this is not yet the verole. Has the monkey served only as a soil for the transplantation of the chancre? This is very possible. We have the right to think so, until we succeed in producing in him constitutional accidents. This opinion is so much the more maintainable, inasmuch as many writers upon syphilis, especially in England, pretend *that the chancre which does not become indurated* is not a syphilitic accident. Will the experiments of M. Auzias come to confirm this opinion? I shall inform you at a later period what I think of this, and what I think upon the induration of chancre.

However it may be, I shall say to you, meanwhile, that if the *primary accidents* incontestably inoculable upon man, can be inoculated upon the monkey, the *secondary* accidents ought also to be inoculated, if, perchance, they have very recently become inoculable.

Is there, then, for each particular disease, as for the epidemics in general, a versatile character? Or, rather, is it not the genius of observers which changes?—[*Boston Med. and Surg. Jour.*

On the Adulteration of Medicines.

Dr. Richard D. Currey has made an excellent Report to the Tennessee Medical Society, from which we cull the following notice of some of the articles most frequently adulterated, and the mode of detecting the imposition:

Aloes.—Of the six varieties of this resinous extract, only three are of sufficient importance to merit notice, the *cape*, *hepatic*, and *socotrine*, and of these the last is the most valuable. The usual adulterations are sand, earth, &c.; but a great fraud is practiced in palming off the two inferior for the more valuable *socotrine*. There are characteristic properties belonging to each, however, which will guard the purchaser, in some degree, against imposition.

Cape aloes possesses a shining, resinous appearance, a deep brown color, and a glossy, resinous fracture. Its powder is greenish yellow, and produces a paler colored decoction than the other kinds. Its odor is strong. Thin laminæ present a yellowish, red color, when held up to the light.

Hepatic aloes is opaque—of a *liver* color, whence its name—of a moderate degree of fragrance, it might be called unpleasant. Fracture rough and semi-transparent on edges. Powder dull olive yellow. When held in the hand, it softens and adheres.

Socotrine aloes gives a glossy, smooth and conchoidal fracture—is of a garnet red color, and yields a golden yellow powder. Thin laminæ are translucent or nearly transparent on the edges. Its odor is highly fragrant, which is, to a great degree given off, when a portion is heated. I take the liberty of presenting samples for the inspection of the association.

Alum, which is the sulphate of alumina and potassa, crystallizes in regular octohedrons, but is, in the commercial article, without regular form. It is adulterated with iron, thus rendering it unfit for the dyer. It should give a colorless solution. Tincture of nutgalls or prussiate of potash, will detect, in such solution, the presence of iron. Unslacked lime, added to the dry powder, will give off the odor of ammonia, if that alkali be present, and nitrate of silver will detect the existence of chloride of sodium, or common salt.

Ammonia Aqua.—We have, in commerce, five strengths of this alkali, designated as F., FF., FFF., FFFF., and blistering ammonia. When adulterated with organic matter, a carbonaceous substance will be left after evaporation by heat. The addition of muriatic acid forms sal ammoniac, which, if resinous matter be present, will not be pure white or clear, when evaporated. Lime water will detect the presence of carbonate of

ammonia, by forming a white, insoluble carb. lime. Nitrate of silver will detect the presence of muriate of ammonia by forming the white chloride of silver, usually termed horn-silver, and chloride of barium will throw down an insoluble sulphate, if sulphate of ammonia be present.

Carbonate of ammonia volatilizes entirely by the application of heat, and effervesces on the addition of an acid.

Tartrate of antimony and potassa, when in the crystalline form, is readily recognized, being in white, transparent, rhombic octohedrons, whose lateral planes present a striated appearance. When powdered it readily admits of foreign admixtures, the most usual of which are the bitartrate of potash. If the powder has a yellowish tinge, the presence of iron may be suspected, which may be detected by adding to its solution a few drops of sulphuric acid, and subsequently the prussiate of potash. If then the characteristic blue prussiate results, iron is present. If the bitartrate of potash be present, its solution in 40 parts of water will be effected by its own volume of 8 parts acetate of lead in 32 parts water and 15 parts acetic acid.

Arrow root, from the East Indies, does not crepitate between the fingers, like that from Bermuda or the West Indies. When pure, full fifteen minutes are required for it to gelatinize and become adhesive, whereas, potato starch, with which it is adulterated, rapidly stiffens and becomes tenacious. The microscope will detect the different varieties of starch granules that may exist in the arrow root.

Arsenic, being very volatile, a residue will be left if white sand, chalk, bone dust, or heavy spar, its usual adulterations, be present.

Sub-nitrate of bismuth effervesces, if chalk or carbonate of lead be present, on the addition of nitric acid. Diluted sulphuric acid will throw down from this nitric acid solution a white precipitate, if lead be present.

Cantharides often deteriorate by age, and are subject to injury from mites. In the powder or plaster they are adulterated with euphorbium, which also possesses irritating and vesicating properties. The presence of this adulteration may be ascertained by heating the substance, when, if a benzoic acid vapor be given off, its presence is established. The microscope may also detect its presence. To this may be attributed the frequent inertness of the cantharides plaster of the shops, as well as to an insufficient quantity of the Spanish fly.

Castor oil is the only one of the fixed oils soluble in alcohol: hence this affords a test for lard oil or any other cheaper admixture. Castor oil capsules, containing but a very small proportion in each sac, would be insufficient to produce a pur-

gative effect, were not this compensated by the addition of a minute portion of croton oil. Instances have occurred of painful and even fatal gastritis, resulting from the administration of these capsules.

Chloroform, when pure, is perfectly transparent, possessing a sp. gr. 1, 5. It possesses an extensive solvent power. This property enables us to use it as an analytical agent in separating substances from their foreign admixtures, as resin of guaiac from resin of jalap—cinchonia from quinia, and narcotina from morphia. Pure chloroform, when allowed to evaporate on the hand, leaves no smell. If an unpleasant odor remains, it is evidence of the presence of some peculiar oil, which is not volatile, and which has been generated during the manufacturing process. The presence of these oils is also supposed to be tested by the discoloration of pure sulphuric acid, though the experiments of others have not confirmed the supposition. Instances have frequently occurred of death resulting from the use of chloroform. This is doubtless owing to the impurity of the article, not because it is of itself thus fatal. Perfectly pure chloroform has never produced such results. Can it be otherwise when we may find it varying in strength from one-twentieth to three-fourths of what it should be. Free chlorine and hydrochloric acid have been detected in it by the litmus paper. If alcohol be present as a diluent, the bichromate of potash, with a few drops of sulphuric acid, will cause the production of the green oxide of chromium, floating on the surface. There has also been noticed the formation of minute crystals, of a pink color, on the sides of the bottle at the upper surface of the fluid, an impurity resulting from the oxide of manganese, used in its purification. To be safe in its use, its sp. gr. should not be less than that indicated, 1, 5; while it should be fragrant and clear—no unpleasant odor on evaporation—should not redden or bleach litmus paper, nor give a greenish tinge when agitated with sulphuric acid.

Cinchona is estimated by its external as well as chemical characters. So many different varieties of this truly valuable bark have been found, that the strictest vigilance is necessary to prevent fraud in its importation into our ports. Its value is determined according to the quantity of quinia it yields. Recently, a new principle has been discovered in some barks brought from Carthagena and Maracaibo, called quinidine, from its great resemblance, in all its characters, to quinine, though as an anti-periodic it is valueless. M. Guibourt reckons the value of the different cinchonas thus:

1. *Calisaya cinchona*, royal yellow.
2. *Cin. micrantha*, orange yellow.

3. Pitaya, “ “
4. Verrucous true, red.
5. Non-verrucous true, red.
6. Red Lima.
7. Grey Lima.

The calisaya bark is distinguished from all others which are fraudulently mixed with it, by “the shortness of the fibres of the entire transverse fracture, and the ease with which they are broken, instead of bending and remaining adherent.”—(Prof. Carson, in *Am. Jour. Pharm.*, April, 1850.) Its color is uniformly yellow, and possesses no white marbling in its thickness. The nail, when drawn over its internal surface, leaves a shining mark, and its outer surface is conchoidal. Calisaya has, also, a more intensely aromatic, bitter taste than the other barks, this characteristic varying even in its different varieties. At the recent National Pharmaceutical Association, Dr. Bailey reported that he had rejected at the port of New York, 300,000 lbs. of spurious bark, especially of that variety that contained exclusively the newly discovered principle, quinidine. He was led to reject them from the conviction that they were to be employed in the adulteration of powdered calisaya, as well as of quinine. The ostensible object for which they were imported was for the manufacture of *tooth powders*. The price of barks, in good condition and of good quality, at the place of collection, is said to be not less than from 60 cts. to \$1,00 per lb., for those yielding quinine—on the contrary, it is frequently invoiced to this country at 10 cts. per lb. As it takes 2 oz. to make one ounce of quinine, it may be readily conjectured why it is that quinine can be sold sometimes as low as \$2,00 per lb., and the powdered barks as low as 50 cts. and \$1,00 per lb.

Citric acid belongs exclusively to the vegetable kingdom, being found in lemons, limes, sour oranges, tamarinds and tomatoes. The juice obtained by expression is readily crystallized in white, inodorous, rhomboidal prisms. They possess a sharp, sour taste, are soluble partially in water, and insoluble in alcohol. Its value subjects it to sundry adulterations, as lime, tartaric acid and sulphuric acid. With chloride of barium, sulphuric acid, if present, is precipitated as an insoluble sulphate of baryta. Muriate of potash detects the tartaric acid by the formation of a tartrate of potash, and lime will be left after incineration. The lemon syrup of commerce is more frequently made of tartaric acid than of either citric acid or of the pure juice of lemons. According to Soubeiran, citric acid precipitates baryta, and not lime, from their solutions, whereas tartaric acid exerts the contrary effect. Moreover, citric acid is deliquescent, while tartaric acid is not.

Cod liver oil has become so popular in the treatment of diseases of the chest, simulating phthisis, that it is subject to various adulterations, the most common of which is sperm oil. Pure cod oil has the odor of sardines, and is free from the lamp oil odor—sp. gr. 9, 7. With nitric acid of sp. gr. 1, 215, a dull green hue is gradually assumed, which, after several days, changes to brown, whereas sperm oil, with an acid of same sp. gr., produces a pale brown without a shade of green. Besides this impurity, which is intentionally mixed with it, others are attributable to want of care in assorting the cod from the *haddock*, the *hake* and the *pollock*, certain species of the finny tribe which accompany the cod in their haunts, and which are, consequently, caught at the same time.

Balsam Copaiva—M. Guibourt, acting under the authority of a commission from "Ecole de Pharmacie de Paris," after examining a variety of samples of this oleo-resinous balsam, asserts that the four following properties indicate a balsam that is certainly pure: 1. Entirely soluble in 2 parts of absolute alcohol. 2. Forming at 60 F. a transparent mixture with two-fifths of its weight of a strong solution of ammonia. 3. Solidifying with one-sixteenth of its weight of calcined magnesia. 4. Producing a dry and brittle resin after prolonged ebullition with water.

Its usual impurities are castor oil and turpentine. The presence of this fixed oil will prevent its solubility, according to the first property; neither will the resin left after ebullition harden, but will remain soft. A drop on paper, volatalized by heat, leaves a translucent spot, if pure, but if impure, this spot will be surrounded by a fatty areola. Castor oil will also prevent the transparency of the mixture with the solution of ammonia. Turpentine is detected by its odor.

Creosote.—I present for examination, two samples of creosote—the one pure and colorless, the other impure and reddish, with an empyreumatic odor. The adulteration in this sample is owing either to rectified oil of tar, or a peculiar article called *capnomer*, which, in many of its physical and chemical properties, is similar to creosote. The presence of these impurities may be detected by acetic acid and caustic potash, which completely dissolve the creosote, if pure, but if adulterated, will not.

Cubebs, in the powder, is adulterated with ground allspice, by which the taste will be so altered as to lead to their detection.

Ergot, in the powder, is rendered inert by the admixture with plaster and paste.

Ether, sulphuric, has a permanent boiling point at 98 F. The addition of impurities will have the effect to raise it to a

higher point. Turpentine will also cause a nauseous odor to remain after moistening a handkerchief with it.

Gum Arabic, when pure, is in round, white lumps, or in masses presenting a brittle and crackly appearance. Its pure appearance distinguishes it from the inferior sorts with which it is sometimes mixed. Gum Senegal is, however, bleached for this express purpose. The pure gum, however, is perfectly soluble in water without swelling, from which solution it is precipitated by alcohol. The salts of the peroxide of iron yield an ochre precipitate with gum senegal, while a red coloration will be afforded with the true gum Arabic. It is in the powdered state that these tests are of the most value, when it is impossible to detect by the eye, as can be done in the solid state. If starch is with the powder, it may be tested by iodine—the characteristic blue being produced.

Iodine, that has not been *resublimed*, is always more or less impure, hence we have a *commercial* and a *resublimed* article. It has been a mooted question whether the operation of the law is adverse to the admission of the commercial iodine, on account of its impurities, into our ports, and so far the action of our inspectors, as well as instructions from the Secretary of the Treasury, have been against its admission. The most common impurity with which the *commercial* is sophisticated is the large proportion of *water*. The standard of purity adopted by the inspectors, will not allow of more than 2 per cent. of water, yet it is found of all grades, varying from 2 to 20 per cent. An argument offered for the admission of such is drawn from its employment in the manufacture of the iodides, and for daguerrean purposes, for which it is said to answer as well as the *resublimed*. The law, however, is express on this subject, forbidding the importation of any medicine or chemical that is not pure enough for medicinal purposes, and, as it is conceded by both parties, that the commercial is not of that purity, it is very properly excluded. At the recent National Pharmaceutical Association, Dr. Stewart, to get at the sense of the members on this subject, introduced a resolution to the effect, that all articles, good for their kind, should be passed at our ports. As the importers of this commercial article acknowledge their design to be for manufacturing purposes, it was urged that it should be admitted. The whole subject was discussed—Prof. Carson, of the University of Pennsylvania, among others, taking a part in it, yet the resolution was rejected.

Other impurities found in this chemical are *black lead*, *charcoal*, *black oxide of manganese*, *Galena*, *sand*, *clay* and *chlorides of magnesium and of calcium*, and free *chlorine*. Pure *resublimed* iodine is in dry crystalline scales. If they adhere to

the sides of the vial, it is an evidence of the presence of *water*. If entirely vaporizable on application of heat, it is an evidence of the absence of inorganic impurities. The magnet will separate the black lead, which is truly a carburet of iron, from the mass, if present; it is also insoluble in alcohol. Muriatic acid, mixed with it, will evolve chlorine, affording evidence of the presence of oxide of manganese.

Ipecacuanha root is composed of two parts, the cortex and woody fibre, in the proportion of 4 to 1. The virtues of the root reside exclusively in the bark, the woody fibre being inert. Hence it may be seen that when the root is ground up, regardless of these distinctions, one-fifth of its active properties is destroyed, consequently the choicest powdered ipecac is now labelled *cortex sine ligno*. The root is annulated, rendering its recognition easy. Hence it is only in its preparations and powder that it admits of adulteration. Liquorice root is frequently ground up to a large extent with it, and again the peculiar powder, called "powder of post," is also added to it. This, of course, would weaken its emetic properties, to restore which its adulterators resort to a certain proportion of tartar emetic. With the microscope we can detect the different starch granules of these several powders—while sulphuretted hydrogen will produce an orange precipitate, the golden sulphuret of antimony, if tartar emetic be present. The presence of foreign substances will also affect its action with certain chemical agents.

Magnesia calcined This substance admits of such ready adulteration, that it is rarely found absolutely pure. It is known to contain carb. lime, carb. magnesia, alumina and silica. With the carbonates, an effervescence will take place on the addition of an acid. A complete solution cannot be effected if silica be present, and the alumina will afford a white precipitate with ammonia, if present in the muriatic acid solution. If lime be present, oxalate of ammonia produces a precipitate from the same acid solution. *Carbonate of magnesia* is also adulterated with the same substances, and may be detected by the same tests.

Blue mass—pilula hydrargyri, if of the officinal strength, should contain one-third mercury. While, therefore, it frequently falls far short of this standard, it is also found to contain a strange medley of impurities, as compensating agents. The following analysis, made by Prof. Reid, of New York, upon a sample of imported blue mass, presents these filthy substitutions in a striking manner. In one hundred grains there were of

Mercury,	7 $\frac{1}{2}$
Earthy Clay,	27
Prussian Blue,	1 $\frac{1}{2}$

Lard,	2
Soluble saccharine matter,	34
Insoluble organic	12
Water,	16

Pure blue mass should contain $33\frac{1}{3}$ grains mercury in 100. Here we have only $7\frac{1}{2}$ —nearly four-fifths less than there should be. Sulphate of mercury is also found in it, and when so, it renders the mass highly injurious. This impurity arises from the gross error of endeavoring to heighten the color of the conserve of roses by means of sulphuric acid. Consequently, on mixing the ingredients into a mass, a chemical action takes place—the deep blue color of the mass partakes of a golden tinge, from the formation of the sulphate of mercury. Its presence may be suspected when this particular tinge is found, but readily ascertained after washing, by means of the baryta test. The inorganic earths will be left after incineration of the mass, and Prussian blue by striking an ink with sulphate of iron.

Mercury ammoniated, white precipitate, so closely resembles dry white lead, calomel, chalk and gypsum, in several of their physical properties, that it readily admits of adulteration. Pure white precipitate may be entirely sublimed on the application of heat, being resolved into mercury, nitrogen and hydrogen. If, therefore, any adulterations exist, there will be a residue left.

Peroxide of mercury, red precipitate, is also readily susceptible of adulteration with red lead, brick dust, Armenian bole, nitric acid and red oxide of iron. A strong heat will also readily vaporize this substance, if pure; if impure, a residue will be left. It is also entirely soluble in muriatic acid, and hence its impurities, if any, will subside.

Protochloride of mercury, pure calomel, acted upon with a solution of iodide of potassium, changes to a greenish yellow, and with lime water or the caustic alkalies, it blackens. Treated with nitric acid, it affords a white precipitate with nitrate of silver, soluble in an excess of ammonia. The physical properties of calomel are well known, being insoluble in water, alcohol, ether, and muriatic or acetic acids, inodorous and tasteless. Its usual adulterations are chalk, gypsum, phosphate of lime, carb. lead, sulphate of baryta, sal ammoniac, &c., and corrosive sublimate through faulty preparations. The test of its adulterations is easy, for it is entirely vaporizable by heat, if pure. The presence of corrosive sublimate is manifested by the yellow, not black color, afforded by lime water, and a scarlet color with iodide of potassium. Again, corrosive sublimate is soluble in ether and calomel not. It may thus be separated, if present, and then, by the evaporation of the ether, the corro-

sive sublimate will assume a crystalline character. While writing this report, I procured three samples of calomel severally of the manufacture of Mander, Weaver, & Co., Howard & Farr—and not one failed to afford very unequivocal indications of the presence of corrosive sublimate. That of Mander, Weaver & Co., was the most impure.

After the application of a strong heat, if a residue is left its character may be ascertained, for if a carbonate of lime or lead, it is soluble, with effervescence, in dilute muriatic acid—if an oxide of lead, hydrosulphuret of ammonia will change it black, and yellow by iodide of potassium—and if a white precipitate is afforded with oxalic acid and oxalate of ammonia, the residue is lime.

Bichloride of Mercury.—Corrosive sublimate forms a *yellow wash* with lime water or caustic alkalies, thus distinguished from the black wash of calomel. It is also soluble in ether, and less so in alcohol and water. With iodide of potassium a scarlet precipitate is produced.

Mercury, red sulphuret.—Vermillion is a paint, but the following analysis shows its great impurity. Out of ten samples, 3 of the Triest vermilion were pure.

1	“	“	contained 15 per ct. carb. magnesia.
1	“	“	contained 26½ “ “ “
1	French	“	pure
1	“	“	contained 35 per ct. sulphate of lime.
2	Chinese,	“	contained 48 and 62 per ct. chromate lead.
1	American,	“	was entirely red lead.

Morphia and its salts are soluble in water and alcohol, and insoluble in ether. Of the three salts the sulphate is determined by the baryta test, the muriate by the silver test, and the acetate by the odor of vinegar. Their impurities are sugar, narcotine, and perhaps starch. Narcotine is soluble in water, chloroform and solution of potash—and soluble in ether, the action of solvents differing, in a remarkable manner, with the two substances. The crystalline character affords a good test, pure sulphate of morphia being in small filiform crystals. The impurities present no such regularity. If pure, nitric acid gives the salts of morphia a blood red color, changing to yellow—perchloride of gold affords a yellow, and the salts of peroxide of iron a blue precipitate.

Essential and fixed oils are also contra-distinguished as *volatile* and *fat* oils—the former being the more valuable, and adulterated with the latter. The detection of such adulteration is, however, easily made, for if a transparent, greasy stain is left, after the application of heat to a drop on paper, it affords evidence of the presence of such adulteration. All of the fixed

oils, except the single instance of castor oil, are insoluble in alcohol, while the essential oils are entirely so. Chloroform is also a solvent for the essential and not for the fixed oils. The fixed oils afford a saponaceous compound with alkalies—the essential oils do not.

Opium is estimated according to the amount of morphia it contains. While, therefore, its adulterations may consist of sand, earths, leaves, vegetable extracts, &c., these are nothing compared with the fraud sometimes practiced of reducing the per centage of morphia contained in the mass, by extracting it, to a certain extent, and then remoulding the cakes and passing them into the current of trade. Opium has been presented for admission into our ports, that was so impure as to be actually alive with *worms*. This statement is made by one of the examiners.

Pure Smyrna opium is in irregular, roundish masses, of a highly bitter, nauseous taste, and brownish, red color, and more or less soft, according as it is fresh or old. In Mohr's Treatise, a method is given for ascertaining the proportion of morphia, and I beg leave to incorporate a succinct account of it in this report: Half an ounce of opium is to be macerated in an ounce and a half of cold water, for twenty-four hours, then strain and strongly express. Repeat the maceration for six, and then again for three hours. Prepare a thick milk of lime with one drachm of quick lime, to which add the infusion when boiling hot. Boil for five minutes, then strain through a cotton cloth, wash the residue with boiling water, and express. Evaporate the liquor thus obtained to one ounce, filter through paper, and heat to boiling. Then, by the addition of one scruple of muriate of ammonia, the impure morphia will be precipitated. Allowing from six to twelve hours for the precipitate to collect, it is then to be dried and weighed. This will afford the morphia exclusive of narcotine.

Bicarbonate of potassa crystallizes in oblique prisms, thus distinguished from the carbonate, which exists in a granular form. It may be remarked here, that the crystalline form of the various chemical compounds affords ready means for ascertaining their character. Every inorganic mineral, whether compounded in the laboratory of nature or of the chemist, possesses a certain definite crystalline form, which it will assume at all times, unless the presence of a foreign substance gives to the crystal-making particles different axes, and thereby create a new and different form; and such will be the result. Crystallography, therefore, furnishes invaluable aid in chemical analysis. This salt of potash is adulterated with the sulphate, muriate and carbonate. The sulphate is detected by nitrate of

baryta, the muriate by the silver test, and carbonate of potash by the action of corrosive sublimate—if even a trace is present, a brick red precipitate being produced.

Bitartrate of potash is known in commerce only in the powdered form, and is, therefore, frequently adulterated. Marble, alum, gypsum, bisulphate of potassa, flour, rice and starch, are used for this purpose. C. V. Hagner, in the *American Journal of Pharmacy*, gives his experience in drug grinding. It was with this article that he commenced this business. For many years he had the entire control of such work, until his process became known, and rival establishments sprang into existence. One of these establishments failed, and one of the hands sought employment with him. He could do every thing—"powder this, powder that, and grind 'tother—never had any difficulty except in grinding cream tartar." I expressed, says Mr. Hagner, my astonishment, knowing it to be the easiest thing in the whole catalogue of drugs to powder, and on asking him what the difficulty was, he replied, quite innocently, "*the alum always sticks the stones fast.*"

In testing it for its impurities, marble and gypsum will remain undissolved in any amount of water—alum will be precipitated in a gelatinous form, by heating the solution to the boiling point, and then adding ammonia until the liquid acquires an alkaline character.

Iodide of Potassium.—The principal adulterations of this valuable medicine are carbonate of potassa and chloride of sodium. Iodide of potassium is very soluble in water, and in five times its weight of alcohol—is *slightly* deliquescent and of a pungent taste. Carbonate of potassa is insoluble in alcohol—is very deliquescent, and consequently its presence will cause the iodide to assume a pasty, semi-fluid condition. It will also effervesce with acids, and redden turmeric paper. Either of the chlorides may be detected by the silver test. The quantity of iodine present may be ascertained by separating it from the potassium by heat, condensing the scales and weighing either of the ingredients separately. In a sample analyzed by Dr. Christison, he found 74 parts of carbonate of potassa, 16 of water, and only 9 of the iodine.

Sulphate of Quinine.—The ingenuity of man has been at work to discover substances with which to adulterate this valuable medicine. Whatever would the more closely resemble it in its physical properties, seemed to have been sought out with avidity for this base purpose. At one time cinchonine was made to retain its place with the chemical—then salacine was ascertained to possess somewhat similar properties, and was mixed with it in large quantities—and then again mannite, and

stearine, and starch, were severally used for the purpose. But a new principle has been recently discovered in a species of Peruvian bark found in Maracaibo and Carthagena, which, from its great resemblance to quinine, has been called *quinidine*. There is not the least doubt that our valuable sulphate is largely adulterated with this inert substance. So fully impressed have our examiners been with this belief, that they have rejected large quantities of these false barks. Zimmer has published a severe test for this new principle, and I here present the association with the appearance presented by two samples of quinine, tested according to his mode. The one, which was taken from an ounce jar of Powers & Weightman's manufacture, is almost transparent, there being, however, a thin film between the separating fluids. The other, taken from a jar labelled Rosengarten & Denis, is, as you see, a dense mass of filthy impurities, suspended between two fluids. Zimmer's test is as follows: To ten grains of the salt add ten drops of diluted sulphuric acid and fifteen drops of pure water, in a strong test tube fitted tightly with a cork, and apply a gentle heat to accelerate the solution. When cooled, add to the solution fifty drops of officinal sulphuric ether, with twenty drops of spirits of ammonia, and shake the mixture while the top is closed by the thumb. The tube is to be then closely stopped and shaken gently from time to time, so that the bubbles of air may more readily enter the layer of ether. "If the salt examined be free from cinchonine and quinidine, or contain the latter in no greater proportion than ten per cent., it will be completely dissolved; while, on the surface, where contact of the two layers of clear liquid takes place, the mechanical impurities only will be separated. If more than a tenth of cinchonine or quinidine be present, an insoluble precipitate will be found interposed between the two fluids. If it be quinidine, it will be dissolved on the addition of ether, while cinchonine will remain unaffected." Other impurities may be detected as follows: Gypsum, chalk and magnesia, are left undissolved—boracic acid is soluble in alcohol, and tinges the flame green, if ignited. Cold sulphuric acid, C. P., will not form a colorless solution if stearine, sugar, mannite or salacine be present.

Rhubarb may be obtained of two varieties, the Chinese or India, and Russian or Turkey. The English, though sometimes substituted for the other sorts, is very inferior, and not at all reliable. This, and also the India, are assorted, and the better specimens disfigured to resemble the Turkey; as much as the fifth of a cargo being found to be these inferior sorts. To lighten the color, tumeric is rubbed over the root, or mixed with the powder, to which there is also added a proportional

quantity of gamboge to increase its cathartic effect. This may be detected by digesting in ether, and pouring a few drops of the solution on water: "if gamboge be present, a film of opaque yellow color will be found floating on the surface, which is dissolved by potash, and is changed to an intense red color." It is stated that within eighty days previous to the passage of the Drug Law by Congress, 7,000 pounds had been passed through the Custom House of New York, not one pound of which was fit for use. Some were worm-eaten, others deteriorated by age, and the remainder had evidently undergone maceration to extract the active properties for the preparation of extract of Rhubarb. And doubtless all of this was subsequently powdered, and so fixed up as to sell for a high price as true Turkey Rhubarb.

Scammony.—One of the rarest medicines to be found is *Pure* or *Virgin Scammony*. It bears a high price, and both physicians and apothecaries seem more content with the cheaper adulterated article, than to risk the sale, or incur the expense of the genuine. Of the three varieties, the Smyrna is good for nothing, the Aleppo contains about 30 per cent, and the Virgin Scammony 78 per cent. The Lancet for February, gives an account of the examination of thirty samples procured in London. Out of thirteen samples of Scammony in cake, *one* only was *genuine*; the others being adulterated from 8 to 75 per cent. One sample was entirely *factitious*, being composed of the resins of guaiacum and jalap, with woody fibre, cellular tissues, &c.

Out of seventeen samples of powdered Scammony, only *one* was *genuine*, the others varying in adulteration from 18 to 65 per cent. The adulterating substances consisted of chalk, wheat flour, sand and earthy substances, gum, woody fibre and cellular tissue. I take pleasure in presenting samples of this gum resin, in cake and in powder. Pure Virgin Scammony should contain 78 per cent. of resin. Chalk will be detected by effervescence with acids, and flour or starch, by the Iodide test, and by the microscope.

Nitrate of Silver is offered for sale in the form of pure crystals, or in rolls or sticks, designated as No. 1. and No. 2. The three varying in purity from the pure to 60 per cent. and 30 per cent. The adulterations of this last, are so abundant as to render it unsafe for administration. For internal use none but the *pure* should be used. The usual adulterations are nitre, lead, zinc and copper. Hydrochloric acid will detect nitre, by separating the silver as an insoluble chloride, and then testing for the nitre in the solution. Copper will afford a blue color, with caustic ammonia, and lead will be blackened by sulphuretted hydrogen.

Spirits of Nitric Ether.—Nothing is more common than to dilute this valuable diaphoretic with variable proportions of water and alcohol. Hence the physician is often disappointed in the result derived from its employment. Water having the effect to render it heavier, will raise its specific gravity, and alcohol will change its odor. The commercial articles generally contains 33 per cent. of water.

Thus much for the analytical view of our subject. There is another means within our power for exposing these base frauds. The eye aided by the microscope, can be familiarized with the appearance of pure and genuine substances, and fails not to detect when one is mixed with another. The forms of crystals readily reveal to us their chemical character, it being a fixed law of the crystalline forces, that a substance will assume the same crystalline form, if left alone or unmixed, wherever made; but it was not until recently, that the microscope was discovered to subserve such an important purpose in the examination of powders and other medicines. Crystalline organic products have been found to possess as great a diversity in structure and form, as inorganic crystals, and according to Dr. Pereira and Mr. Queckett, may be used as a criterion of their purity. They may thus be referred to their respective organs. These indications are offered, it matters not how minute they may be, in the form of the cells, the shape of the starch granules, and the condition of the spiral vessels of the organic product. Of course, the examiner should be acquainted with the appearance of undoubtedly pure substances, so as to be enabled to form a correct conclusion.—[*Southern Jour. of Med. and Phys. Sciences.*

New Splint for Fracture of the Radius. By Dr. BOND.

[We give below some of the directions presented by Dr. Bond to the Philadelphia College of Physicians, for the use of the splint referred to in this Journal of the 12th inst. It may be well to mention that Dr. Hays suggests, in the last number of the American Journal of the Medical Sciences, a substitute for the splint of Dr. Bond in fractures of the lower end of the radius. He thus describes the simple manner in which he has made it. The cover of a cigar box being cut into a proper shape, a double layer of cotton wadding was laid over it. A strip of muslin, six yards long and three inches wide, was rolled up to within two yards of its end, and then a pin placed at each edge, not opposite to each other, but one a little nearer to the end than the other, so that the roll made, with the straight piece, an angle, the same as that made by the bottom of the splint

with its front edge. The roll was then placed on the end of the splint, the strip carried over the back, then turned over the front, over the roll, and again to the back, when the angle gave to the strip a direction which carried it over the edge and in front of the roll to the opposite edge. The strip was then continued over from edge to edge until the whole splint was covered. The following is Dr. Bond's account of his apparatus.]

1. With a light board, of proper thickness for a splint, take a profile of the well fore arm and hand of the patient, placing the hand in its habitual inclination towards the ulnar side of the arm, and extending the profile from the elbow downwards, so that it will reach the second joint of the fingers on the inside, when these are moderately flexed—as much flexed as they are when the points of the thumb and fingers are brought into contact. The lower end of the board must be cut off obliquely (at an angle of fifteen or eighteen degrees) in a direction corresponding with that of a body grasped in the hand, when the hand is inclined to the ulna, as above indicated.

2. Cover the board, thus prepared, with sheeting, or other strong fabric. This may be done by winding around it, from end to end, a narrow rolling bandage, covering all of it as nearly as may be, with few or no duplications. This is the most expeditious method. A neater one is to cut a piece of sheeting of the general form of the board, but extending beyond the board on every side, and fastening it upon the board either by a few stitches, drawing towards each other the overlapping edges, or glueing down those edges upon that side of the board which is to be towards the arm, and which edges are to be covered with the pasteboard.

3. Prepare a block of soft, light wood, from seven-eighths to eleven-eighths of an inch thick, and from two to two and a half inches wide, according to the size of the patient's hand, and of a length corresponding with the width of the board in the palm of the hand. This block is to be carved and rounded, so as to adapt it to the form of the hand, and make it easy for the thumb, and in the grasp of the hand when it is placed on the board. It is to be fastened there by screws or nails, so that the remote edge of it shall correspond exactly with the lower oblique end of the board.

4. Upon that part of the board not covered by the palm-block, fasten, by means of small carpet tacks, a piece of book-binder's pasteboard, extending on each side beyond the edges of the board about an inch. If the pasteboard be very thick and stiff, make a slight incision in it along the edge of the board, in order to bend more easily the two projecting por-

tions of it, thereby making a kind of box for the lodgment of the arm.

It seems to me that this splint, or one constructed on the same principles, will meet the above mentioned indications in the following manner:—First. The form given to the board retains the hand in its habitual inclination towards the ulnar edge of the arm, accomplishing the object aimed at by Dupuytren's *attelle cubitale*, with as much certainty, with more simplicity, and more comfort to the patient. Second. The palmer-block retains the hand in its habitual inclination backwards, and it gives the fingers that moderate flexion which most relieves the muscles from tension, and likewise that position which, if stiffness should result, will not only save the hand from a most inconvenient, ungraceful deformity, but will reserve to it the power of performing very many of its most frequent and useful functions. In addition to these advantages, this block contributes much to the comfort of the patient. Third. The object in covering the board with a strong fabric, as above described, is to retain the bandage with certainty in its place, without applying it with a dangerous tightness; for, by fastening the roller to this covering with pins, the surgeon need never have his patience tried by finding his dressing deranged, at his next visit. I can speak with confidence on this point, from having used it repeatedly in cases where this quality was fully tested. Fourth. The pastboard is not an essentially necessary part of the splint, but it will be found to contribute to the comfort of the patient and the convenience of the surgeon.

The requisites for dressing with this splint are flannel or other soft fabric, to cover or line the inside of the splint; two compresses; a roller; sometimes, but not always, a dorsal splint.

The flannel or other fabric with which the splint is lined should extend a little beyond the edge of the paste-board, and the same piece may be extended over the palmer block; but it will be better to cover this block with a separate piece. For this purpose take a piece of flannel large enough, when it is doubled to cover the block. Through the doubled edge, with a proper needle, carry a small string (such as ligature-twine,) and tie this around the splint immediately above the block. The covering of the block thus applied may be conveniently changed, without removing the arm from its bed.

Two compresses will generally be required: the anterior or palmer, and the posterior or dorsal. The proper construction and application of the former of these are a most important point in this dressing, and certainly not less so when long, straight splints are employed; and deformity of the radius or wrist will most frequently result from negligence or want of

skill in its use. If the compress be deficient in thickness, and the bandage be applied with its usual tightness, there will not fail to be either a curvature forwards, or a sigmoid flexure, which are the usual deformities. If the thickness of this compress be excessive, there may be a curvature backwards, which I think seldom occurs; but there will be such undue pressure by such a compress as to increase the danger of adhesions, and to aggravate the discomfort of the patient.—[*Boston Med. and Surg. Journal*.

On the Treatment of Erysipelas by the Muriated Tincture of Iron. By W. H. RANKING, M. D., Cantab., Physician to the Norfolk and Norwich Hospital.

Charlotte Andrews, æt. 23, a fat, strumous girl, was admitted into the Norfolk and Norwich Hospital, under my care, in April, for engorged and suppurating cervical glands, which were treated by generous diet, cod-liver oil, free lancing, and subsequent dressing with the iodide of lead ointment, an application which I may state in passing, appears to me to be more suitable to scrofulous ulcers than any other with which I am acquainted. Under this treatment her progress was all that could be desired, until the 26th of May, when my attention was called to an erysipelatous redness over the right breast. This quickly extended, and in spite of the assiduous endeavor on the part of our intelligent house-surgeon, to limit its boundaries by the nitrate of silver, it soon invaded the head and face, inducing those serious symptoms which are observed in these cases. As soon as the disease fully declared itself, no time was lost, as is my invariable custom, in sustaining the powers of the patient by wine, beef tea and ammonia, the local application being flour.

On the 28th the disease had continued to extend, and as the patient was losing strength, the ammonia was replaced by quinine.

On the 29th, the report is, that the erysipelas had occupied the entire head and face, and was creeping down the back. The patient was delirious, with dry tongue and feeble pulse of 130.

On the 30th she was still worse, the tongue was more dry and dark, and diarrhœa was added to the other unfavorable symptoms. Wine was given in increasing quantities, so that she took more than a bottle in the twenty-four hours.

June 1st.—She was still more depressed; pulse 140. fluttering; the face livid, and she appeared to be fast sinking.

At this time, I determined to administer the muriated tincture of iron, as recommended by Mr. Bell, of Edingburgh, and did so, as will be seen, with the best results. The dose was forty minims in water, every three hours. In the evening, after three doses, the pulse was still 140, but had more resistance to the finger, and she was left for the night, with orders to continue the medicine and wine.

On the 2d, there was a most marvelous change for the better. The tongue, which the day before was dry and dark, was now moist and cleaner, the pulse had sunk to 120, and the patient was able to raise herself in bed. The face was desquamating, and her only complaint was urgent thirst, which was gratified with water *ad libitum*.

3d.—I continued the iron in doses of thirty-six drops. The improvement was still more manifest; the face had become more natural in appearance, and she was pronounced out of danger. From this time her recovery was uninterrupted.

Knowing how difficult it is to establish a medical *fact*, I am prepared to find that some of my hearers may dispute the agency of the iron in the girl's recovery. On this point I would only say, that prior to her taking that medicine, she lost ground hourly, in spite of the freest exhibition of wine, ammonia and quinine; and that after three or four doses a perceptible advantage had been gained, which advantage was rendered indisputable on the next day, by the improved condition of the pulse and tongue, and the rapid subsidence of the cutaneous engorgement.

I take no credit to myself for this mode of treating erysipelas, but most willingly record my obligation to Mr. Bell of Edingburgh, who published a paper on the subject in the "Monthly Journal of Medical Sciences" for June, 1851. That gentleman's testimony as to the value of this treatment is most strong; he says that in every instance in his practice it has been successful. His brother, Dr. Charles Bell, is equally impressed in its favor; and he states that it not only removes the disease in a short time, but also renders the patient less susceptible of returns of the disease. In pursuing the chalybeate treatment of erysipelas, Mr. Bell regards it as important to bring the system rapidly under its influence, and acting on this conviction, I gave, in the case above related, even larger doses than Mr. Bell had sanctioned.

Although the case I have related was an instance of idiopathic erysipelas, the treatment is said to be equally beneficial in the traumatic form, and in infantile erysipelas. Of the latter, Mr. Bell, details some remarkably interesting cases.—[*Prov. Med. and Surg. Journal*.

Dr. Bernard's Experiments on the Elective Elimination of certain Substances by the Secretions.

Dr. Cl. Bernard has written a very interesting account of certain experiments made by him "on the elective elimination of certain substances by the secretions, and in particular by the salivary secretion." He remarks *in limine* that this secretion has not been examined with the same care as the urine, bile, milk, &c., in regard of the circumstances alluded to; and that it remains to be explained how the saliva chooses some, while it rejects other substances equally soluble in it. He performed two sets of experiments, which were ingeniously contrived and carefully repeated.

In the first series of experiments he injected into the right jugular vein a solution of yellow prussiate of potash, of iodide of potassium, and of grape sugar, and immediately thereafter he detected the second substance in the saliva, but neither of the other two; while in the urine, the prussiate of potash could be detected, but neither the iodide nor the sugar. Twenty-five minutes after the injection, abundance of the prussiate of potash was found in the urine, only a trace of sugar but still no iodide. The saliva remained as before. The secretions were tested every half-hour, but no change occurred till the end of two hours, from the time after injection, when the iodide of potassium at length appeared in the urine. Neither the prussiate nor the sugar appeared at any time in the saliva, which eliminated only the iodide of potassium; and it is worthy of remark that this salt appeared immediately in the saliva, while it was not detected in the urine for two hours. When, however, the solution of the iodide was stronger, it appeared sooner in the urine, though never within the hour.

In the second series of experiments, M. Bernard injected these three substances in solution, severally into the veins of the same animal at different times, as well as into the veins of different animals, and he always found that they comported themselves in exactly the same manner. Such was the case, likewise when they were introduced into the stomach. Both grape and cane sugars, like the prussiate of potash, never appeared in the saliva, while they were eliminated more slowly by the urine. This observation seemed to contradict the assertions of some authors, regarding the saliva of diabetic patients, but on actually testing this secretion in 'la Charité,' M. Bernard found that there was no trace of sugar in the saliva, while however, it could be detected in the expectoration from the bronchi, of such patients as had phthisis combined with diabetes. Neither sugar nor prussiate of potash would seem to pass into the bile, or into the pancreatic juice, in ordinary circumstances,

but when sugar was strong in the blood, it was found in the bile, but never in the pancreatic juice.

In regard to the elimination of sugar from the economy, M. Bernard has noted a very curious fact, viz., that though the mammary secretion naturally contains a kind of sugar, it does not allow either cane or grape sugar to pass by it. Sugar of milk is much more difficult of fermentation than the other kinds of sugar, and may thus be distinguished from them.

When the iodide of potassium was injected into the vein of a dog, or introduced into its stomach, it could always be detected in the saliva within forty seconds. It also passed with rapidity into the tears, and into the pancreatic juice, while it passed with much greater slowness into the bile, in which it was often difficult of detection.

M. Bernard also injected lactate of iron into the veins of dogs, and never found it eliminated by the saliva; in which respect it agreed with the sugars and prussiate of potash. When iodide of iron was carefully injected into the vein of a dog, both iodine and iron were found in the saliva. In another dog, a solution of the iodide of potash was introduced into the stomach by a fistulous opening, and afterwards another solution of lactate of iron; both substances were thereafter detected in the saliva, showing that the iodine gave to the iron the capability of being eliminated by the saliva.

M. Bernard at present merely wishes to call attention to these interesting facts, and does not offer any explanation of them. But the property which certain substances seem to possess, of being eliminated by different secretions, is not their only peculiarity. Their period of sojourn in the economy is also importantly different. Thus, M. Bernard remarks that the iodide of potassium and other substances, perfectly soluble, and really dissolved in the blood, remain for a certain time within some of the organs of the body; and he finishes his paper with the following account of the experiments made by him, in order to investigate this sojourn of the iodide of potassium in the animal economy. He introduced into the stomachs of several dogs, which had permanent salivary and biliary fistulæ, a solution of two grammes of iodide of potassium. The same day the urine of these dogs exhibited the reaction of the iodide; next day it could be detected neither in the bile nor in the urine; and on several days following, no trace of it was found in these secretions. It seemed to be completely eliminated from the system; but examination of the saliva showed its presence still. The gastric juice also contained the iodide, both because it contained saliva, and also because it was furnished directly with it from the mucous membrane of the stomach. This per-

sistence of the iodide in the saliva and gastric juice continued for three weeks, and possibly it may have continued longer. Purgatives have a great effect on the sojourn of the iodide in the economy; indeed, so much is this the case, that if purgatives were employed, soon after the introduction of the salt into the stomach, a few days sufficed for its total disappearance from all the secretions.

In concluding M. Bernard observes, that these experiments show that substances which are soluble, and capable of circulating in the organism without producing mischief, present two sets of phenomena worthy of remark.

"1st. Some substances never pass into certain determinate secretions, *e. g.* the yellow prussiate of potash, cane and grape sugars; others show themselves in all the secretions, only with greater or less rapidity, *e. g.* the iodide of potassium.

"2d. Some of these substances are eliminated completely and rapidly from the economy, *e. g.* the yellow prussiate, sugars, &c.; while others are only *partially* eliminated by the urine, and may remain in the organism, showing themselves in other secretions for a longer or shorter time. The iodide of potassium offers a remarkable example of this prolonged sojourn of soluble substances in the organism,—a sojourn which, in the case of that salt, is prolonged, because the portion not eliminated and re-appearing in the saliva, instead of being expelled from the system is constantly thrown back into the stomach, whence it is taken up by the circulation, and returned to the saliva, and so on.

"The chief conclusion," he continues, 'to be drawn from this work is, that one cannot refer to any general law, the manner in which these substances act in the organism. The experiments made on one saline substance, can teach nothing regarding another: no one could have foreseen, for example, that the iodide of potassium, and the yellow prussiate of potash, salts equally soluble, should differ, in respect of their passage into the secretions, and of their elimination from the body, differences very striking. Special researches on each particular substance are necessary in order to establish physiological history, which ought to be intimately connected with its mode of action as a therapeutic agent.'—*Archives Générales de Médecine. Med. Examiner.*

Treatment of Acute Rheumatism. By EDWIN R. MAXSON, M.D.,
Adams' Centre, Jefferson Co., New York.

As acute rheumatism is a disease the pathology of which has been a matter of doubt, and the treatment consequently various,

I here offer a plan of treatment which I have found very satisfactory.

In a severe case, in which the inflammatory fever runs high, if the arms are suffering, I draw from two to four ounces of blood, by cups, from each side of the spine, near the origin of the brachial nerves, between the shoulders. If the lower limbs are suffering, I draw the same quantity, by cups, from each side of the spine, in the lumbar region, near the origin of the crural nerves. This generally relieves the pain in the limbs immediately, and checks the progress of the disease. I procure an evacuation of the bowels by mag. sulph. \bar{z} ss., repeated, if necessary, and then give potassa nit. $\Theta i.$, dissolved in a tea-cupfull of warm gruel, every three hours, and continue this till the fever and inflammatory symptoms subside, which will generally occur in from four to six days.

At this stage, there is generally left slight swelling and some stiffness of the joints. I then discontinue the nitrate, and give potassa iodide, gr. x., with vin. colch., gtt. xx., every six hours. This should be continued till the slight swelling and stiffness of the joints subside, which may take place in from four to six days. The appetite will, in the mean time, generally become good. The patient then need only be directed to take potassa iodide, gr. v., three times per day, for a few days more, to prevent a relapse and render the cure permanent.—[*Buffalo Med. Jour.*

On a simple Method of ascertaining, without the use of the Catheter whether the Eustachian Tubes are pervious; with some observations on the Treatment of cases of Obstruction in these Tubes. By JOSEPH TOYNBEE, F. R. S.

The author pointed out the objections to the two ordinary modes of exploring the Eustachian tubes—viz. that the use of the catheter is liable to produce pain and discomfort; that without experience, it is not easy to ascertain whether it be really in the tube; that the plan of attempting to distend the tympanum by a forcible expiration, while the mouth and nostrils are kept closed, is not always successful, from the fact that the young and nervous cannot be taught to perform the act, and that sometimes, when it is properly done, the guttural orifices of the tubes seem to be pressed together so as to preclude the air from entering. In a paper recently read before the Royal Society, the author endeavored to show that the guttural orifice of each Eustachian tube is generally closed, and that the air in the tympanum is not continuous with that in the cavity of the fauces, except during the momentary act of deglutition. In proof of this the following experiment was cited: If the mouth

be shut, and the nostrils be held closed by the finger and thumb, and then the act of swallowing be performed, a sensation of fullness or pressure is experienced in each ear; and this sensation does not disappear upon the removal of the pressure from the nose, but it vanishes at once when the act of swallowing is again performed, while the mouth and nostrils are open. During the first act of swallowing, a small quantity of air was forced into the tympanic cavities through the Eustachian tubes, and it therein remained until the second act of swallowing again opened the tubes and permitted the air to escape. The muscles whereby the Eustachian tubes are opened are the tensor and levator palati, which it is well known take origins from the cartilaginous walls of the tubes. As, during the act of swallowing with closed mouth and nostrils, air is forced through the Eustachian tubes into the tympanic cavities, it is evident that the permeability of these tubes can be ascertained by making the patient swallow some saliva while the mouth and nose are shut. Nor need the surgeon depend upon the statement of the patient respecting the sensation of distension felt in the ears; for, by listening with the *otoscope*, should the Eustachian tubes be pervious, the air will be distinctly heard to enter the tympanic cavities, and produce a gentle crackling sound. The author next proceeded to consider the treatment of cases of obstruction of the Eustachian tubes, especially in reference to the use of the catheter. It having been ascertained that these tubes are obstructed, is it desirable to attempt to open them by means of the catheter? Believing that obstruction in the Eustachian tubes generally depends upon a thickened state of the mucous membrane covering the guttural orifice, and that this state is always associated with a thickened condition of the faucial mucous membrane and of the mucous membrane of the tympanum, the author suggests—especially to those inexperienced in the use of the catheter, not to attempt to pass this instrument—firstly, because, in such cases the mucous membrane of the Eustachian tube is often so tumefied that no ordinary degree of pressure will force the air into the tympanum; and, secondly, because, should the surgeon succeed in transmitting a few air-bubbles, the relief obtained is only partial and endures for a very brief period, since the mucous membrane remains as thick as before, and the ill effects of the obstruction soon recur, from the air in the tympanum becoming of a different density from that without. The *membrana tympani* becomes more or less fixed. The treatment recommended is such as shall tend to reduce the thickened mucous membrane of the guttural orifices of the Eustachian tubes to a healthy size, so that there muscles may be able to open them. For this purpose besides

the use of general remedies, the solid nitrate of silver, or a strong solution of hydrochloric acid, may be applied to the mucous membrane of the fauces and to the apertures of the tubes, and gentle counter-irritation is to be kept up over the region of the fauces. By these measures, as a general rule, the mucous membrane can be reduced to its natural state, and the tubes become again opened by their muscles. Should this not take place, the Eustachian catheter may now and then be introduced and air be gently blown through it. A modification in the shape of the Eustachian catheter is suggested—viz. that it should be oval instead of round, the advantages derived being that it not only can be passed through the nose with less discomfort to the patient, but its presence in the Eustachian tube is much less disagreeable from the absence of the convex surfaces which, in the rounded catheter, press against the nearly flat surfaces of the tube. In conclusion, the author expresses his concurrence in the opinion of Harvey and Kramer, that enlarged tonsils are never the cause of obstruction in the Eustachian tubes, and that any benefit that may have followed their extirpation has arisen from the loss of blood consequent upon the operation.—[*Lancet*.

Cotyledon Umbilicus in Epilepsy.

This agent has gained some reputation in the treatment of that obstinate malady, epilepsy. Dr. Abbot submitted the following to the above society:

He had administered in the dose of five grains of the extract, night and morning, for eight months, with entire cessation of the fits during that period. The patient was a gentleman, upwards of fifty years old, who had been subject to the disorder for five or six years; the epileptic fits occurring, at the time the use of the remedy was commenced, as often as once in two or three weeks, with one or more attacks of epileptic vertigo daily. The vertigo, under the use of the remedy, diminished in severity, but not in frequency. At the end of eight months, the patient began to totter very much in his gait, with frequent "dropping turns," as if from his legs "giving way" under him, with only partial loss of consciousness. No convulsion, but a slight general tremor, with some rigidity of the limbs. A feeble, unexcited pulse, and no flush or heat of countenance. At this time, the use of the cotyledon umbilicus was discontinued, and a small quantity of phosphoretted oil was given, which was followed on the third day by a bad epileptic fit, and its use was consequently abandoned. Since that time, the epileptic attacks have occurred at intervals of three or four

weeks, and the other morbid phenomena are of daily occurrence. Under these the powers of life appear to be gradually failing.

Dr. H. O. Stone referred to two cases in which the cotyledon umbilicus had been used with good effect. In one case, there was an interval of one hundred days between the fits, after using the medicine a short time.—[*Western Lancet*.

Chlorate of Potash in Ulcerative Stomatitis and Cancrum Oris.

Dr. J. H. Babington states (*Dublin Quarterly Journal*, Feb. 1853) that, in an epidemic of ulcerative stomatitis which occurred in the Coleraine Union Workhouse, in 1849, he used the chlorate of potash with great success. The treatment adopted was a mild aperient of rhubarb and magnesia, and the administration of chlorate of potash, dissolved in water, sweetened with syrup, in doses of four grains every fourth hour; the mouth was also washed with a weak lotion of solution of chloride of soda. They all recovered in about six days. Dr. B. treated one case with alteratives and tonics, and it was three weeks before it got quite well; thereby proving the efficacy of the chlorate of potash.—[*American Jour. Med. Science*.

Case of Accidental Poisoning with half an ounce of Tartar Emetic, successfully treated with Green Tea and Tannin.

By STEPHEN A. MCCREERY, M. D., U. S. N. (Communicated by THOMAS HARRIS, M. D., Chief Bureau Med. and Surg., U. S. N.

On the morning of the 28th of September, 1852, Dr. — feeling a little indisposed took twelve grains of blue mass. At half past two o'clock, P. M., some hours later, he took on an empty stomach what he believed to be (and what he had ordered) half an ounce of Rochelle salts with forty grains of bicarbonate of soda and as much tartaric acid. Immediately afterwards he dined sparingly on ham, the breast of chicken, and, tomatoes, and after dinner ate two ripe figs. In about thirty-five or forty minutes after taking the medicine, he experienced some nausea, but attributing it to his indiscretion in having eaten so soon after taking the powder, he resisted the disposition to vomit which every moment became more urgent. In the course of four or five minutes, however, he was obliged to yield to it, and vomited twice very freely, after which he felt relieved. In two or three minutes the nausea and vomiting returned, and he then began to suspect that he had received the wrong medicine. Instant inquiry was made, and

it was discovered that the person who had put it up had mistaken the *antimonii et potassæ tartras* for the *sodæ et potassæ tartras*. Dr. Thomas Williamson, of the Navy, saw the patient at twenty-five minutes past 3 P. M., and immediately ordered for him copious draughts of green tea and large doses of tannin. Albumen, the infusion of flaxseed, and of the slippery elm, and iced water were also freely administered. The vomiting, which was very distressing, continued with little intermission until 9 or 10 o'clock in the evening. There was also very severe purging with most violent cramps of the legs, and slighter ones of the wrists. The first evacuation from the bowels was purely serous; those which followed were of a bilious character, but very loose. There were no cramps of the stomach. When it was thought that the stomach and bowels had been cleared of the poison, an injection containing tincture of opium was given and repeated in a few moments. The injections not being retained, a full dose of the acetate of opium was administered by the mouth. Brandy, mint julep was also freely given as the patient was very much prostrated. A large sinapism was applied over the epigastric region, and frictions were used to the extremities during the paroxysms of cramp. Iced tea and iced mucilaginous drinks were continued through the night.

September 29. The patient passed a better night than could have been expected; was still nauseated, and complained of great thirst, and had some headache; the tongue was moist; there was no abdominal pain or soreness, or any burning sensation about the stomach; the bowels were twice moved. Iced tea, and iced milk, and arrowroot diet were directed.

30th. There was no vomiting or purging; no thirst; diet and drinks were continued as before.

October 1. Rested badly the past night. Had headache and nausea, and the tongue was coated with white fur, but there was no febrile excitement. The iced drinks were continued, and toast, and coffee, chicken broth, and oyster liquor were allowed the patient.

2d. There was some slight irritation of the mucous membrane of the throat, and pain on pressure in the upper and right side of the breast; no fever. A sinapism was applied over the seat of pain, and the slippery elm bark and gum Arabic were used to allay the irritation of the throat. The diet and drinks were continued.

4th. There was still some slight irritation of the throat, and some cough, but no pain in the chest nor fever. The patient was allowed full diet. From this date he continued to improve, and on the 13th was able to resume his ordinary occupations.—[*Ibid.*

Regeneration of Nerves.

Dr. Augustus Waller has been engaged in a very interesting series of investigations on the regeneration of nerves.* While previous observers were contented with examining the nerve-tubes at the point of section or in the cicatrix, this author has pursued the investigation to the peripheral ends; and has arrived at the interesting and unexpected result, *that the old fibres of a divided nerve never recover their original functions, and that reproduction of a nerve takes place not only in the cicatrix itself, but throughout the terminal ramifications.* The vagus of a dog having been divided, was examined after twelve days, when it was found that the inferior segment was completely disorganized, the fibres being all converted into black or irregular and opaque parcels, and the membranous tubes destroyed. At the end of a month the condition was different: almost all the disorganized substance had been removed; new fibres were found in place of the old, possessing all the characters of young fibres, and being very difficult of recognition, owing to their grey colour, intimate adherence, and want of double contour; but on the addition of organic acids—concentrated acetic especially—they were readily recognised as embryonic fibres. The disorganized nerve presents nothing similar, there being only an amorphous tissue, which dissolves readily in acetic acid, without any residue. The areolar tissue which surrounds the nerves presents nuclei, which, however, it is easy to distinguish from those of nerve-fibres,—they are shorter, thicker, irregularly scattered on the surface of the membrane, showing no approach to parallelism, and the tissue itself does not separate into cylindrical fibres. The gelatinous fibres of Remak, which present the same structure and reactions as the young nerve-fibres, do not exist in appreciable quantity in the vagus before its distribution to the œsophagus, and hence cannot be a source of error in these observations. The author thinks that the neurilemma plays an important part in the regeneration of nerve-fibres: it remains intact during the changes just described. The results of section applied to the sympathetic fibres show that regeneration takes place in them in a similar manner. The following remarkable results were observed with regard to nerves in connexion with ganglia: the roots of a spinal nerve were laid bare, and cut above the ganglion, in such a way as to leave a portion of them in connexion with it; the animal was again examined after twelve days, when it was found that the sensitive part of the root

* Philosophical Transactions, 1850; various papers in the Comptes Rendus of the Académie des Sciences, 1851, 1852.

attached to the superior part of the ganglion was altogether disorganized, in the same manner as when a nerve is cut in its peripheral portion. The nerve, followed into the ganglion, exhibited its branches disorganized, subdividing in the body, and mixing with fibres altogether normal, and appearing to terminate in a collection of ganglionic structures equally altered. All the fibres which passed out of the ganglion preserved their normal condition, the state of the fibres being found the same, after a month or more, as at first. The regeneration of the superior fibres between the ganglion and the spinal marrow takes place in the ordinary manner. The motor fibres were completely altered and disorganized to their extremities. These observations appear to be decidedly opposed to the views of Valentin, who has advanced the opinion that the ganglia are not centres of innervation, but only a mechanical means for the arrangement of the fibres.

After sections of the chorda tympani,—which was effected by passing a cutting instrument into the tympanal cavity, and turning it in different directions—the inferior portion was found, at the end of from ten to twenty days, in the cat, dog, and rabbit, to be completely disorganized. In all these animals only about twelve or twenty normal tubes were to be found in the midst of the disorganized textures. These tubes appeared to the author to come from the lingual nerve, and to follow an ascending course in the chorda tympani.—[*British and Foreign Med. Chir. Rev.*

Tetanic Symptoms from the use of Iodide of Potassium. By D. P. PHILLIPS, M. D., Passed Assis't Surgeon U.S.N.

A case of some singularity having occurred under my own observation, and thinking that it might not be devoid of interest to you, I have concluded briefly to give its history.

Whilst Acting Surgeon of the U. S. ship Massachusetts, a fireman, named J. White, was admitted upon my sick list with rheumatism. I ordered the administration of iodide of potassium, grs. viii. ter in die, to be taken before meals in a spoonful of water. Soon after commencing with the remedy (probably the second day) he complained of some uneasiness and stiffness in the jaws; but supposing it to be some trivial affair, I paid but little attention to it. On the next day the difficulty had increased, and I directed frictions with some stimulating liniment; but when I saw him the day after, the jaws were immovable. Upon careful inquiry, I ascertained that ever since he had been using the iodide he had experienced a burning and uneasy sensation in the œsophagus and stomach. Upon learn-

ing this, I discontinued the medicine, and ordered counter-irritation over the stomach. In a few days the tetanic symptoms entirely disappeared, and the iodide of potassium was renewed, but diluted in a tumbler half full of water, and given *after* each meal. The patient entirely recovered from rheumatism, and had no return of the trismus. I attributed the unusual symptoms entirely to the use of iodide of potassium in too concentrated a form.—[*Philad. Med. Examiner.*]

Miscellany.

Chloride of Soda.—The efficacy of Chloride of Soda in the treatment of Burns, Wounds, Ulcers, &c., has been so decided in our hands, that we feel impelled to say a few words on the subject in consequence of having, not unfrequently, found it to fail in the practice of others; and having always in such cases ascertained that the solution used was much stronger than that *we* are in the habit of prescribing.

The American chloride of soda being very seldom good, we never use any other than the French. The directions usually accompanying Labarraque's bottles are, however, apt to mislead those who look to them as a guide in its use. They state, for example, that "a mixture of one part of the chloride with seven or eight of water has been successfully used in chilblains and burns, and also for bad wounds, especially for sores in which mortification has commenced." Now, although so strong a solution may be useful when "*mortification has commenced*," it will almost invariably aggravate the condition of burns and recent wounds. Lisfranc, to whom we are indebted for its use in burns, always recommended a solution of half an ounce in a quart of water,—insisting that the application must be weak enough to give *no pain*, but, on the contrary, to *allay* this. The sudden relief of the pain of burns which attends the application of this weak solution by means of cloths dipped into it, and kept wet, is truly astonishing. Whether the cuticle be removed or not, the relief is equally prompt, and the ultimate cure more rapid than by any other medication. One important effect of the remedy, especially in children, is, that it prevents the excessive suppuration which is itself so often fatal in such cases.

As an application, or wash, to recent wounds of a lacerated character and to chronic ulcers, especially of the legs, we use a mixture of the same strength; and it is only in cases tending to mortification that we ever apply it sufficiently strong to produce smarting. In short, if its

antiphlogistic effect be desired, it must give *no pain* when applied—whereas, if it be used as a styptic or stimulant it must be made stronger. We know of nothing more irritating, and consequently injurious, to a burn, new wound, or ulcer, than this solution, when made too strong.

Collodion for Chordee.—We read in the “*Revue de Thérapeutique, &c.*” that Dr. Doringier has recently tried the use of Collodion in a case of Chordee, with good effect. The case was that of a young man, who, in his third attack of gonorrhœa, was suffering very much with chordee, which resisted the usual remedies. Dr. D. ordered cold affusions to the organ until relaxation should occur, and then applied a thick coat of collodion to the entire organ. The erections ceased from that moment; but on the next day, the collodion having been removed, they returned, and were again arrested with another application of the coating.

The above appears to us to be an ingenious and useful application of collodion, but also one not entirely free from danger. If the coating be not carefully applied over the entire organ, including of course the glands, and if it be not of uniform resistance, strangulation might result from the unequal compression whenever there occurred a tendency to erection. The coating should be thick enough to prevent its yielding at any point.

Fly Poison.—The danger of using the article commonly called Cobalt or Fly-Stone, the activity of which depends upon the arsenic it contains, renders it desirable to get an innocuous substitute for it. The Swiss, it is said, are in the habit of using, very effectually, a strong decoction of quassia amara well sweetened with molasses. The flies being fond of it seek it with avidity and are very promptly destroyed.

It is said that an infusion or decoction of our poke root or the juice of its berries, made sweet with molasses, will very effectually destroy cockroaches. We have tried strewing the floor with cucumber peelings, and found it better than anything we ever used for getting rid of these filthy insects.

Deaths by Chloroform.—The New York Journal of Medicine contains a tabular statement of the thirty-three cases of death from Chloroform recorded in the European and American periodicals up to the end of 1852, not including “those imperfectly reported, or those in

which the persons have taken it without the advice and aid of a professional person." From these tables, it appears that the anæsthetic was administered fatally ten times for operations on the toes and fingers, six times for the extraction of teeth, three times for operating on fistula in ano and hemorrhoids, twice for the application of caustic, once for opening abscess, and eleven times for other operations. Ten of the deaths occurred before the operation was performed.

Treatment of Ulcers.—We find in the "*Revue de Thérapeutique, &c.*" that Dr. Lasanna, an Italian physician, recommends highly an ointment made of equal parts of tincture of Iodine and Lard in the treatment of ulcers in general. The application is made twice a day, and has, according to him, healed in ten days most obstinate ulcers.

Death of Dr. Beaumont.—Dr. Beaumont, the author of the justly celebrated researches on the subject of Digestion in a case of fistulous opening into the stomach, has recently died at St. Louis, aged about sixty-eight years.

American Medical Association.

We are indebted to the kind attention of Dr. Bulkley, Editor of the New York Medical Times, for a copy of the proceedings of the American Medical Association, from which we derive the following summary :

The Association held its sixth annual meeting on Tuesday, May 3d, in the Presbyterian Church, Bleeker-street, the President, Dr. Beverley Wellford, of Virginia, in the Chair. The morning was occupied by the Committee of Arrangements, in receiving delegates from the several States. At 11½ A. M., the President declared the meeting organized, and welcomed the delegates to the city. There were nearly five hundred gentlemen present.

On motion, a recess of fifteen minutes was taken to allow the delegates to select one of their number from each State, as a committee to nominate officers for the ensuing year.

The following gentlemen were selected as the Nominating Committee :

District of Columbia, Thomas Miller; Illinois, N. S. Davis; Massachusetts, A. L. Pierson; Connecticut, Archibald Welch; Ohio, R. L. Howard; New Jersey, Lewis Condit; South Carolina, H. R. Frost; Pennsylvania, F. West; Vermont, E. S. Carr; Tennessee, J. B. Lindsley; Missouri, C. A. Pope; New Hampshire, J. Crosby; Michigan, H. Taylor; Rhode Island, H. Allen; North Carolina, J. G. Tall; Indiana, Joseph Somes; Delaware, H. F. Askew; Alabama, J. A. English; Maryland, Joel Hopkins; Iowa, J. C. Hughes; New York, Joseph M. Smith; Georgia, H. F. Campbell; Kentucky, J. B. Flint; Maine, Isaac Lincoln; Virginia, T. P. Atkinson.

Dr. Condie, of Pennsylvania, Chairman of Committee on Publica-

tions, submitted a Report from the Committee, with resolutions appended, making the assessment for the present year \$5; authorizing the committee to decide upon the terms at which the volume of Transactions for this year shall be furnished; and further authorizing them to take such measures in relation to the disposal of the copies as they may deem expedient, and that it should be their duty to inform the members of the terms agreed upon by a circular. Dr. Condie stated that a valuable paper would be submitted at this meeting, the mere illustrations of which would cost \$1,000 for printing.

The resolutions were adopted.

The Treasurer's Report showed the total receipts for the past year to be, \$1,905; paid out \$2,015; balance due to Treasurer, \$110.

On motion of Dr. Condie, the Committee was authorized to furnish the Chairman of Committees on Epidemics with extra copies of their reports, respectively, at the expense of the Association,—the number of said extra copies not to exceed one hundred.

The President then read a long and very able address, reviewing the origin, progress and benefits achieved by the Association.

Dr. Hays, of Pennsylvania, moved the thanks of the meeting be presented to the President for his elegant, appropriate and eloquent address, and requesting a copy for publication in the Transactions of the Association. Carried.

The Secretary read a resolution passed by the Medical Society of Virginia, recommending the appointment of a well-qualified chemist to analyze the most prominent nostrums of the day, and publish the results monthly in the leading newspapers of each State. Also, a communication from the President of the American Medical Society at Paris, appointing Drs. Pittman, Walton and McIlvaine to attend this meeting.

On motion of Dr. Atlee, the Committee on Publications were directed to send a full set of the Transactions of the Association to the Society in Paris.

A communication was received from Dr. Ramsay, of Georgia, inclosing documents on personal matter, which, on motion of Dr. Leving, of South Carolina, were laid on the table.

Dr. Joseph M. Smith, Chairman of the Committee on Nominations, reported the following officers for the ensuing year:—

For President—Dr. JONATHAN KNIGHT, of Connecticut.

Vice-Presidents—Drs. USHER PARSONS, of R. I.; LEWIS CONDICT, of N. J.; HENRY R. FROST, of S. C.; R. L. HOWARD, of Ohio.

Secretaries—Drs. EDWARD L. BEADLE, of N. Y., and EDWIN S. LEMOINE, of Missouri.

Treasurer—Dr. FRANCIS CONDIE, of Penn.—who were unanimously elected.

The committee reported St. Louis, Mo., as the place to hold the next annual meeting.

The report was adopted.

On motion, the Secretary proceeded to call up reports for Special Committees.

Dr. C. D. Meigs, of Philadelphia, presented a report on "Acute and Chronic Diseases of the Neck of the Uterus," with a request

that it should be referred to Committee on Publication, without reading.

The report was adopted and referred.

Dr. Condie, of Pennsylvania, Chairman of Committee on Causes of Tubercular Diseases, stated that in consequence of his duties as Treasurer and Chairman of Committee on Publication, he had not been able to complete his report.

The committee had considered the subject very attentively, and the more they did so the more a new light broke upon them, until they began to doubt the orthodoxy of many of the received opinions regarding tuberculosis, its causes, and the proper course of medical treatment to be pursued. An abundance of material was furnished—in fact, the report was almost ready; but he had such onerous duties to perform during the year, that it was utterly impossible that he could put it in proper shape.

Dr. Atlee (Pa.) moved that the explanation be accepted, and the committee continued to the next session of the Association.

On the part of Dr. Porcher, of South Carolina, Dr. Condie stated that he had sent to him the Report of Committee on "Toxicological and Medicinal Properties of our Cryptogamic Plants," but with the request that it should be left, for further additions, to the committee. The committee was continued.

Dr. G. Emerson, of Pennsylvania, Chairman of Committee on "Agency of the Refrigeration produced through upward radiation of heat, as an exciting cause of Disease," presented his report, which was referred to Committee on Publication, and read an abstract. The sanitary lesson designed to be inculcated in this paper is the importance of guarding against exposure to the refrigerating effects of nocturnal radiation, especially in sickly places and during epidemic periods. The means of effecting this are shown to be extremely simple and always at hand, as any thing will answer the purpose which may be interposed to cut off the view of the open sky, and thus prevent "upward radiation."

Dr. H. F. Campbell, of Georgia, presented a report on Typhoid Fever, which was referred to Committee on Publication.

Dr. Campbell, said he was not aware, until too late an hour to do so, of the fact that a written synopsis of each report was required by the rules of the Association. If permission were granted, he would make a verbal one, and explain to the convention the views he had taken regarding this class of fever. The permission was granted.

Dr. Campbell* said—I have, Sir, comparatively little experience in the actual treatment of typhoid fever, as it is a disease which rarely prevails in the district where I am located. I have therefore given in this treatise a condensed history of the symptoms and pathology of the disease, as now exists in the works of the best writers. From these data, I have deduced a pathology which I locate in the ganglionic system of nerves. In corroboration of these views, I refer to various experiments on these nerves. If you divide some of the superior branches of this system, near the eye, there occurs an immediate ecchymosis of

* Dr. Campbell's synopsis, as it was reported, contained several errors: he has had an opportunity to make the necessary corrections in this Journal.

the conjunctiva, which is very similar to the capillary congestions observable in typhoid and typhus fevers. I have called attention to the existence and causes of the maculæ, which appear upon the surface, in the one variety (typhus) of fever, and extend through the alimentary canal in the other, (typhoid) and reason that the former are the result of disease in the ganglia along the spinal column which supply the circulation of the cutaneous surface, while the latter are the result of disease in the abdominal ganglia, (as the semi lunar, &c.) which supply the intestines. I have also recorded the morbid appearances in the larynx, pharynx and œsophagus, and refer them to disease in the pharyngeal plexus of nerves which supply these parts. I have examined the theory of some authors who refer the pathology of the two diseases to deficiency of fibrine in the blood, and have endeavored to show that all the phenomena cannot thus be accounted for. Finally, I do not consider typhoid and typhus fever distinct diseases, but that they are the same disease affecting different portions of the ganglionic system of nerves; in typhoid, the *visceral* ganglia are affected; in typhus, the *vertebral* ganglia.

The synopsis was received with loud applause, and the report referred to the Committee on Publication.

Dr. Sutton, of Kentucky, presented a report on Epidemics of Tennessee and Kentucky, with an abstract, which was read by the Secretary, and the report referred to the Committee on Publication.

Dr. Pitcher, of Michigan, presented a report on the subject of Medical Education, which he was requested to read at length. The report was a long and able document, containing many valuable suggestions to prevent the spread of quackery, and on the best means of training the medical student. The committee proposed that all candidates for degrees shall have studied at least three years, and recommend the extension of lecture seasons to six months. The committee repeated their high opinion of the benefits to be derived by students from bed-side experience, as superior to lectures and flitting hospital visits, and suggested a supplementary school of practice. They would not discourage medical schools through the country, but foster them as useful, and trust to the private instructor and the hospitals as schools of practice. The committee asked leave to conclude their report by presenting the following resolutions:

Resolved, That the Association re-affirm its formerly expressed opinions, on the value and importance of general education to the student and practitioner of medicine, and that it would gladly enlarge its rule on this subject, so as to include the Humanities of the schools, and the Natural Sciences.

Resolved, That in the opinion of this Association, a familiar knowledge of the elements of Medical Science should precede clinical instruction.

Resolved, That in order to accomplish the latter, the hospitals, when elevated to the rank of schools of practice, and the intelligent private preceptor, are the most efficient instrumentalities to be used for that purpose.

On motion of Dr. Atlee, the report and resolutions were adopted.

Dr. Joseph M. Smith, of New York, Chairman of Committee on Prize Essays, reported that the committee had received fifteen Essays, which they had critically examined, and that they had awarded the two prizes of one hundred dollars each to the Essays bearing the following titles: The first, entitled "The Cell—its Physiology, Pathology and Philosophy; by WALDO J. BURNETT, M. D., Boston, Mass. "*Natura in minimis maxima est.*" The second, on the "Surgical Treatment of certain Fibrous Tumours of the Uterus, heretofore

considered beyond the Resources of Art;" by WASHINGTON L. AT-LEE, M. D., Philadelphia. "*Palnam qui meruit ferat.*"

The committee also stated that the following named papers are worthy of being honorably mentioned: 1st, on Erysipelatous Fever of the United States; 2d, on Chloroform; 3d, on Subacute Functional Disorders of the Urinary Bladder of Females; and of three papers bearing the names of the authors, they asked that the authors of two of them, viz., Dr. MARCH, of Albany, and Dr. BUCK, of New York, be allowed to submit abstracts to the Association.

Dr. Alden March, of New York, made a verbal abstract of his paper on "Diseases of the Hip Joint," which was favorably reported on by the committee, and on motion of Dr. A. Smith, he was requested to read the paper, during the recess to-morrow, in Crosby-street Medical College.

Dr. March considers hip disease as produced in the head of the bone by upward and inward pressure, from muscular action, against the acetabulum.

Prof. Palmer, of Chicago, moved the following:

Resolved, That this Association earnestly recommend to the local Societies in different portions of our country, to appoint Committees, whose duties it shall be to record the prevalence of epidemics or other diseases, and the general state of health in their respective localities, and to transmit said reports to the Committees of the Society on Epidemics, through the State Societies where they exist.

Resolved, That the Secretaries be requested to secure a wide publicity to the above resolutions, by such means as they may deem proper.

The resolutions were adopted.

Dr. G. Buck, of New York, read his paper "on the Surgical treatment of Morbid Growths in the Larynx," upon which the committee had made a formal report, illustrating a case which had been under his care by means of the specimen and a wax model.

On motion, the paper was referred to Committee on Publication.

Dr. Mitchell, of Pennsylvania, here proceeded to speak of the various difficulties and grievances under which the physicians and surgeons in the Navy labored, and recommended the Convention to take some measures for their relief. He concluded by presenting the following resolution, which was, after considerable discussion, adopted:

Whereas, the claim of naval medical officers to defined rank, assimilated with the grades of officers of the line of the navy, has not yet been decided upon by Congress; therefore,

Resolved, That the President of this meeting appoint a committee of three, which is hereby instructed to communicate to Congress, through the presiding officer of each House, at the commencement of the next session, an expression of the interest felt by the American Medical Association of the United States for their professional brethren employed in the Navy, as set forth in the resolutions unanimously adopted at several sessions of this body.

The resolution was put and adopted.

The following resolution was presented by Dr. Charles Hooker, of Connecticut, and adopted unanimously:—

Resolved, That the delegates from the several States be requested to appoint committees, who shall aid the Committee of Publication in procuring subscribers, and in distributing the Annual Transactions of this Association.

Dr. Mauran, of Rhode Island, presented a report from Dr. Simons, of South Carolina, chairman of a committee appointed by the Association for that purpose, in relation to the necessity of each emigrant ship being provided with a surgeon, which was read by the Secretary.

The report was accepted, and the committee directed to prepare a memorial to Congress.

A report was then presented from Dr. Simons, chairman of a committee of the American Medical Association, to memorialize Congress in accordance with a resolution of Dr. Sutton, of Georgetown, Kentucky, to have the medical statistics of the United States census printed separately, for the use of the medical profession.

This report was also accepted, and the committee requested to prepare a memorial to Congress on the subject.

Dr. N. S. Davis, of Illinois, reported at length, and lucidly, on the Medical Literature of 1853.

Dr. Yandell, of Kentucky, presented a report received from Dr. S. D. Gross, of Kentucky, on the results of surgical operations for the relief of malignant diseases, which was referred to a Committee on Publication. Dr. G. read a brief abstract.

Dr. Gooch called up the subject of the graduating pledge, proposed by Dr. Peaslee, of New Hampshire, last evening, and laid over, and proposed the following resolutions :

Resolved, That this Association earnestly recommends to all the respectable Medical Colleges of the United States to administer to their graduates, previous to their receiving the diploma, some pledge that they will maintain, to the best of their abilities, the honor and dignity of the profession ; and that they will forfeit their degrees, whenever they desert the orthodox system of medicine.

Resolved, That the schools be urged not to graduate any man without requiring him to read the National Code of Ethics and publicly give his consent to abide by it, and that they will reserve to themselves the right to withdraw the diploma, publicly, whenever the graduating pledge has been violated.

There were, said Dr. G., two schools which had already adopted such rules. Unfortunately, as things stood, gentlemen were allowed to graduate on payment of their fees, without knowing there was such a thing as a "Code of Ethics" in existence, and permitted to go forth among medical men as their equals in practice. The consequence was, systems of quackery, and want of proper *esprit du corps* among members of the profession, in many instances.

Dr. Garnett, of Washington, spoke to the resolution.

Dr. Atkinson, of Virginia, inquired how the diploma could be withdrawn ?

Dr. Gooch replied that the diploma could be always withdrawn, if given under these conditions.

Dr. John H. Phillips of New Jersey, offered the following amendment :

Resolved, That it is the duty of all Boards of Examiners, to whom candidates may apply for examination or approval, to admit none but those who give satisfactory evidence of a good preliminary education, and that a regular Course of Medical Practice will afterwards be pursued, and who shall subscribe to the Code of Ethics adopted by this Association."

Dr. Cox, of Maryland, thought the resolution contemplated an ex-

traordinary act of legislation, and there would be great difficulty in applying the principle. The power of revoking a diploma, once given under the legal sanction of a charter, was a dangerous one to be intrusted to any set of men.

Dr. Atkinson, of Virginia, thought that, until the millenium, quackery would exist in the profession to some extent, and it was vain to legislate against it.

On motion of Dr. Sayre, of New York, the Association took a recess for one hour, without disposing of the resolution.

Dr. Blatchford, of New York, moved to have a resolution, offered yesterday, on the licensing power, with amendments by Dr. Garnett, taken up for consideration.

After discussion by Drs. W. Hooker, of Connecticut, Johnson, of Missouri, and Atlee, of Pennsylvania, on motion of Dr. Atlee, the whole subject was referred to a committee, to report at the next meeting.

The subject was referred to the following committee: Drs. Wellford, of Virginia; Samuel Jackson, of Pennsylvania; T. W. Blatchford, Johnson, of Missouri; and Peaslee of New Hampshire.

Dr. T. W. Blatchford, of New York, proposed the following resolution:—

That a committee of five be appointed, of which Dr. J. O. Edwards, of Ohio, shall be chairman, whose duty it shall be to report on the best mode of preventing the domestic adulteration of drugs. The Chair appointed Dr. Edwards, Drs. Wood and Carson, of Philadelphia, Dr. C. T. Jackson, of Boston, and Dr. J. H. Griscom, of New York.

Dr. Joseph M. Smith, of New York, read the following:—

REPORT OF COMMITTEE ON NOMINATIONS.

The Committee on Nominations, in fulfilling the duty of their appointment, propose to continue most of the Special Committees appointed by the Association, in May, 1851, and May, 1852, and to appoint several new Special Committees. They, therefore, submit the following list of Chairmen for Special Committees, with the subjects to them committed:

1. Dr. D. F. Condie, of Philadelphia, Penn., "On the Causes of Tubercular Disease."
2. Dr. James Jones, of New Orleans, La., "On the Mutual Relations of Yellow and Bilious Remittent Fever."
3. Dr. R. S. Holmes, of St. Louis, Mo., "On Epidemic Erysipelas."
4. Dr. George B. Wood, of Philadelphia, Penn., "On Diseases of Parasitic Origin."
5. Dr. R. D. Arnold, of Savannah, Ga., "On the Physiological Peculiarities and Diseases of Negroes."
6. Dr. James R. Wood, of New York, "On Statistics of the Operation for the removal of Stone in the Bladder."
7. Dr. F. Peyre Porcher, of Charleston, So. Ca., "On the Toxicological and Medicinal Properties of our Cryptogamic Plants."
8. Dr. Goodrich A. Wilson, of Virginia, "On Cholera and its Relation to Congestive Fever—their Analogy or Identity."
9. Dr. Worthington Hooker, of Connecticut, "On Epidemics of New England and New York."
10. Dr. John L. Atlee, of Lancaster, Penn., "On Epidemics of New Jersey, Pennsylvania, Delaware and Maryland."
11. Dr. D. J. Cain, of Charleston, S. C., "On Epidemics of South Carolina, Florida, Georgia, and Alabama."
12. Dr. W. L. Suttan, of Georgetown, Ky., "On Epidemics of Tennessee and Kentucky."

13. Dr. Thomas Reyburn, of St. Louis, Mo., "On Epidemics of Missouri, Illinois, Iowa and Wisconsin."

14. Dr. George Mendenhall, of Cincinnati, Ohio, "On Epidemics of Ohio, Indiana and Michigan."

15. Dr. E. D. Fenner, of New Orleans, La., "On Epidemics of Mississippi, Louisiana, Texas and Arkansas."

16. Dr. Chas. A. Lee, of New York, "On Domestic Hygiene."

17. Dr. Daniel Brainard, of Chicago, Ill., "On the Constitutional and Local Treatment of Carcinoma."

18. Dr. N. S. Davis, of Chicago, Ill., "On the Influence of Local Circumstances on the Origin and Prevalence of Typhoid Fever."

19. Dr. Geo. Engelmann, of St. Louis, Mo., "On the Influence of Geological Formation on the Character of Disease."

20. Dr. Henry M. Bullitt, of Louisville, Ky., "On the Use and Effect of Applications of Nitrate of Silver to the Throat, either in local or general disease."

21. Dr. Robert F. Campbell, of Augusta, Ga., "On the Pathogenic Influence of Feather Beds."

22. Dr. James Bolton, of Richmond, Va., "On the Administration of Anæsthetic Agents during Parturition."

23. Dr. Henry Taylor, of Mount Clements, Mich., "On Dysentery."

24. Dr. F. Donaldson, of Baltimore, Md., "On the Present and Prospective Value of the Microscope in Disease."

25. Dr. R. L. Howard, of Columbus, Ohio, "On the Pathology and Treatment of Scrofula."

Committee on Plans of Organization for State and County Societies.—Isaac Hays, M. D., of Pennsylvania, Chairman; Worthington Hooker, M. D., of Connecticut; Josiah Andrews, M. D., of Michigan; B. R. Wellford, M. D., of Virginia; A. L. Pierson, M. D., of Massachusetts.

Committee on Medical Literature.—T. S. Bell, M. D., of Kentucky, Chairman; Samuel H. Pennington, M. D., of New Jersey; Ed. H. Parker, M. D., of New Hampshire; William K. Bowling, M. D., of Tennessee; Zina Pitcher, M. D., of Michigan.

Committee on Medical Education.—B. R. Wellford, M. D., of Virginia, Chairman; Resign Lowe, M. D., of Iowa; Lyndon A. Smith, M. D., of New Jersey; Jacob Bigelow, M. D., of Massachusetts; L. A. Dugas, M. D., of Georgia.

Committee on Volunteer Communications.—Drs. C. A. Pope, Thos. Reyburn, John S. Moore, J. B. Johnson and A. Litton, of St. Louis, Mo.

Committee of Arrangements.—Drs. J. R. Washington, J. S. Moore, S. Pollok, Thos. Reyburn, J. O'Farrar, W. M. M'Pheeters, C. W. Hempstead and E. S. Lemoine, of St. Louis, Mo.

Committee on Publications.—Dr. D. F. Condie, Pennsylvania, Chairman; Dr. E. L. Beadle, of New York; Dr. A. Stillé, Pennsylvania; Dr. I. Hays, Pennsylvania; Dr. E. S. Lemoine, of Missouri; Dr. G. Emerson, Pennsylvania; Dr. G. W. Norris, Pennsylvania.

On motion of Dr. Watson, of New York, the name of the Committee on Volunteer Communications was changed to that of Committee on Prize Essays.

Dr. Wellford resigned his place as Chairman of Committee on Medical Education, and the President was authorized to fill the vacancy.

The vacancy was subsequently filled by the appointment of Dr. J. L. Cabell, of the University of Virginia.

Dr. Bolton, of Virginia, gave notice that at the next meeting of the Association, at St. Louis, he would propose the adoption of the amendments to the Constitution, submitted to this Association by the meeting at Richmond last year, and which have been here indefinitely postponed.

Dr. Alfred Stillé, Chairman of the Committee to whom was refer-

red sundry memorials touching the course to be pursued by Medical Colleges and other Boards in the examination of candidates and the granting of Diplomas, reported, submitting the following resolutions for adoption :

Resolved, That in order to preserve the purity and honor of the Medical Profession, and to place around young practitioners additional safeguards against temptations to do wrong, as well as to draw a more distinct line of separation between true and false physicians, it be and is hereby recommended, that every graduate in Medicine be required to subscribe a pledge to submit to the revocation of his Diploma upon conviction of having knowingly violated the Code of Ethics of this Association. It is also recommended to the several Medical Colleges and such other Boards as are by law authorized to examine candidates for admission into the Medical Profession to require from every graduate or licensee his signature to the Code of Ethics of this Association, and to furnish him with a copy of the same. It is further recommended that the formal administration of a pledge, faithfully to observe and keep the said Code, form part of the public exercises of Medical Commencements.

The following form of Promise was among the documents referred to Committee on Pledge :

I, A. B., of —, in the State of —, do hereby promise, on the honor of a gentleman, that I will conform strictly to the Code of Ethics of this my Alma Mater. in all things pertaining to the practice of my profession; and, when I shall fail to do so, I hereby grant to the Faculty of said School full power and authority to withdraw said Diploma, and all the rights and privileges which it is intended to confer.

Dr. Palmer, of Chicago, and other delegates opposed that part of the report proposing to clothe Colleges with the power of revoking diplomas for a breach of the "Code of Ethics."

Several motions and counter-motions were made. The Chairman decided on the right of the Committee to withdraw the objectionable resolution, when the second and third recommendations of the report were adopted.

Dr. Sayre, of New York, moved that the resolution withdrawn be taken up, and passed as the sense of the meeting. It was taken up, and referred to the Committee on the licensing power, of which Dr. Wellford is Chairman.

Dr. Parker, of Virginia, moved the following, which was adopted :

Resolved, That the Standing Committee, of which Dr. Bolton is Chairman, be instructed to inquire into all cases of death that may be reported as occurring from the use of anæsthetic agents during the present year in the United States, and report to the next meeting of the Association.

Dr. Campbell, of Georgia, submitted a paper, on a question of priority in reference to the discovery of the reflex relation subsisting between the cerebro-spinal and sympathetic system of nerves.

Referred to Committee on Publication.

The President congratulated members on the close of their deliberations, and expressed his wish that they should have a safe return to their homes, and should all meet at St. Louis next year.

The Association then adjourned *sine die*.

Use and abuse of Medicines.—The communication of our friend, Dr. Long, contained in this number, recalls to memory a satire published not long since in the Buffalo Medical Journal by the inimitable "Dr. Smelfungus," and which we hope we may be pardoned for reproducing. We do not know that a little "spice" should be deemed altogether out of place in a Journal so strictly practical as ours claims to be.

Pneumonia. By SMELFUNGUS.

"During the angry controversy between the Nominalists and the Realists, certain offensive books were ordered to be chained in the libraries. It were well if, by a similar decree, nineteen-twentieths of the *materia medica* were locked up in the cabinets of the curious."—PROF. ABNER H. BROWN.

Smelfungus, standing a few days since on the icy bank by the little watchhouse beside the railroad bridge at Portageville, upraised his hat in reverence to the mind that planned it. There stood the bridge, a structure, firm, sure, and steadfast, yet light, graceful, and symmetrical. Down hundreds of feet in the abysm flowed the dark river, fretting about the solid piers, where,

"Far, far beneath the vast incumbent pile
Slept the broad rock!"

"Here," he exclaimed, "here at last is the true emblem of medical science. Every stick in that vast congeries of tressle-work is isolated and capable, in event of decay, of removal and replacement by another. On no one timber rests any special importance. Any worthless piece may be cast aside without detriment to the unity of the whole. And so in medicine. Our temple is not built of impoverishable materials, neither does the safety of our art rest upon any one fact, medicine, or theory. In the progress of discovery our facts may become worthless, our medicines inert or hurtful, our theories 'the baseless fabric of a vision,' and still the goodly structure stands; for a new and truer fact is substituted; by a better interpretation of nature we have some better treatment of disease, and for our withered theories we gain some surer basis. No mortis or tenon is in the edifice, and the casting out of an erroneous idea does not involve the destruction of its neighbor, or endanger the stability of the whole.

"Therefore, oh watchman! scan closely all the parts—reject fearlessly all decayed and broken elements. Go on, ye builders, taking from and adding to, until the temple of Esculapius stands a perfect whole of solid and enduring material!"

Having had an easy delivery of these pregnant remarks, Smelfungus retired to the Lauman House to warm his feet and get a cigar. Once thoroughly warmed and under the influence of a specially good cigar, he went into a discussion of medical matters—things new and old—with all that homely enthusiasm for the good, and peppery indignation for the bad that characterize him.

"*In medio tutissimus ibis*, said the olden poet, and ever since, like

wine long kept, the pithy adage has grown richer and more truthful. In all the little eddies and whirlpools of medical inquiry, (with little great men floating like chips upon the seething waters) we recognize two mighty currents, deep flowing and rapid in their diverse ways, uplifting huge contentious waves of difference. The one of these sweeps on with gathering force against old theories, and already it threatens the gorgeous though composite pile of our old *materia medica* with destruction. And this is the tide, or the gulf-stream, or if you please the all-enveloping maelstrom of natural medicine."

Here Smelfungus sticks in his allegory, and like Mr. Micawber, descending abruptly from his lofty periods, he "docks the tail of sentiment."

"In short," (growing red in the face,) "here are two separate packs of fools annoying sober men with their nonsense. Here is one class, who tell us that medicine is an invention of the enemy—who call aloud for another Hercules to turn another Alpheus through those Augean stables the apothecary shops, and sweep from the learned shelves the latin-labeled drugs as things of no account or value. Oh ye cold blooded animals who look upon a patient as an interesting specimen of natural history, and turn a deaf ear to his earnest cry for pills and potions—the *world loves medicine!* Man, quoth Cuvier, (?) is a pill-taking animal, and you who deny this first want of his nature he will not call upon.

"And yet woe to the poor patient, if in shunning the natural historian he falls upon the other extreme of unlimited control over diseased action by medicines! He will surely find himself *in extremis!* These men of blood and guts, who remove a pint of disease from a hole in the brachial vein, and a half a gallon more *per anum*, are worse than their do-nothing antagonists. They are no rare birds either. I have a dozen in my circle of acquaintance whose latest author is good old Dr. Thomas. Why sir! when my poor friend Dr. T. was coming down with consumption, we met in consultation, a half a dozen brother chips, when, as the youngest man, I was first called on for an opinion, I proposed such a mild febrifuge, anodyne, and counter-irritant course as should allay the pressing inflammatory symptoms then present, and restore the tone of the stomach, old Dr. W's gray hairs stood up in mingled wonder and disdain. 'Give him an emetic!' thundered the doughty old Hunker. 'He's got the consumption, *that's* the treatment for consumption, and *that* will do him good!' The good old gentleman evidently fancied that poor T. was going to puke up a phthisis pulmonalis! And every day, with younger men, we see this same insane notion of a routine of pills, powders, and emetics, for a disease of assimilation.

"I would to God," (and Smelfungus crushes his cigar in his righteous indignation,) "that I had the power to make these men read, learn, and inwardly digest the whole of Martyn Paine's abstrusely learned volumes!* After such a course as that I fancy they would

* "Man's inhumanity to man
Makes countless thousands mourn."

turn to our lighter periodical literature with a relish which they do not manifest at present.

“*Quò me vehis?*”

‘Prone to wander, Lord I feel it!’

sings Smelfungus in answer to a gentle hint as to the subject matter of conversation, ‘and if my text be true I have been wandering in dangerous paths. Now let us get back to this middle ground if we can find it.

“The revelations constantly making in the natural history of disease have taught all seekers after truth, that medicines are not the actual necessities we have deemed them. Yet it does not hence follow that they are *per se*, useless or injurious. One great general principle may be laid down, viz., that when we can with safety omit a medicine, it is our duty so to do. But this word safety should imply not only immunity from death, but from unnecessary suffering. I like much, (always excepting a certain timidity in its tone,) the article on pneumonia, in the last ‘*Braithwaite*,’ by Dr. R. B. Todd. Dr. Todd proposes to strike out from the list of medicines in this disease, all the weighty items, such as tartar emetic, calomel, bloodletting, etc. and to substitute for them in increased doses a medicine long used in pneumonia, but considered merely as an adjunct, viz., the acetate of ammonia given in six dram doses. Externally he makes frequent use of the turpentine stupe. Thus Dr. Todd relieves us from the use of drugs frequently hurtful and unmanageable. By some process of reasoning not very lucid, he connects this change of treatment with the idea of blood poison.

“Smelfungus will help Dr. Todd through in this matter. Perhaps this blood poison may be an excess of albumen in the fluids. We have been in the habit of calling the albuminous sputa of pneumonia the result of inflammation, but the occurrence of critical albuminuria in this disease would seem to indicate that albumen was in excess throughout the system, independant of phlogistic action. Dr. Smelfungus stands ready to receive the thanks of Dr. Todd for this explanation. But, seriously, the presence of a poison in the blood has something to do with pneumonia typhoides, and who can tell us the pathological differences between that and the acute form.

“The self-limitation of pneumonia is another idea advanced by Dr. Todd. Now I have had the good fortune to see several cases of this disease, which, from the stupidity of the friends, had no medical treatment until the occurrence of bloody expectoration in the second stage alarmed them. In all these cases, the disease had reached its acme, and was on the decline, having involved only a limited portion of the lower lobe. Now, my good Sir Hunker! these cases (three or four in number) are, so far as they go, positive contradictions to your pet notion, that an inflammation once lighted in the lung, will spread like wildfire through its whole parenchyma. It may do so, mind you, but you may safely draw the conclusion that the almost uniform departure of the inflammation at a certain point in the lung, is not *all* owing to your own skill. For if you have such unlimited control over inflam-

mation, why can you not bring it to bear on an erysipelas, or a synovitis? As Allapod says, 'hence we view,' that you do not in every case cut short the disease with your routine of bloodletting, calomel, antimony, and blistering. *It cuts itself short*, and therein only manifests its natural tendency.

"Shall we, then, abandon bloodletting in pneumonia? Even so; for it is generally unnecessary. Not so; for cases there be when the delirious mind grows rational, the swollen countenance natural, and the choked and labored pulse grows soft and easy, from the lancet.

"Shall we abandon calomel and antimony? Again, yes; and again, no; for cases will occur, when every means that science can prompt or art direct, are necessary to guide and govern the lava tide of inflammation, to prevent effusion and abscess, and the whole dark array of *sequelæ*.

"And blistering:" and Smelfungus speaks tenderly as a lover of his mistress, 'blisters are always good, and never disappoint us.' If in all the pauseous scented armamentaria of therapeutics there is one thing that Smelfungus is willing (metaphorically speaking) to take to his heart, it is Emplastrum Catharidis! He loves his blisters as fervently as old Dr. Clysterpipe his syringes, for the old man based his claim to Christian character *on the love he bore his enemies*. Pardon the pun!

"Sir Hunker! from premises like these we predict the dawning of a milder day. Pneumonia is still, and ever will be, a disease eminently requiring the guiding hand of the physician. The old (and we may still call it the usual and authorized) treatment of pneumonia, is a club in the hands of Hercules wherewith we may deal mighty blows. But a pounding less severe will answer in a majority of cases, and we shall yet learn that a rat-trap is no better than a smaller tool for catching the 'small deer' that infest our crania.

"Listen, then, to the truthful lesson! Pneumonia is, in a majority of cases, a self-limited and little dangerous disease, but it should be closely watched, lest, as sometimes happens, the diseased action may not stop at the usual point in the lower lobe, but rage on unchecked throughout its utmost borders. And mark you, man of the lancet! He who cures a pneumonia predestined to occupy a whole lung, does a goodly thing and may congratulate himself. Here come in your whole catalogue of remedies. The God Antiphlogos alone is mighty to save!"

Neuralgia.—Cazenave recommends in facial neuralgia, an ointment composed of chlorform 20 parts, prussiate of potash 10 parts, and lard 60 parts; a piece the size of a walnut to be rubbed over the painful part. An oiled-silk cap is then to be worn for some hours.

[*Rev. Med. Chir. Med. Times and Gaz.*]

SOUTHERN MEDICAL AND SURGICAL JOURNAL.

Vol. 9.]

NEW SERIES.—AUGUST, 1853.

[No. 8.]

PART FIRST.

Original Communications.

ARTICLE XXV.

Foreign Bodies in the Air Passages. By L. A. DUGAS, M. D.,
&c.

The accidental introduction of foreign bodies into the air passages is sufficiently common and important to merit the serious attention of the profession in reference to the means to be employed for their removal; and every practitioner should be provided with suitable instruments to meet such contingencies. Foreign bodies may be lodged in the nostrils, the pharynx, the rima-glottidis, the larynx, the trachea, or the bronchi, and the degree of inconvenience they may occasion will vary according to their position, size and nature.

The objects most frequently introduced into the nostrils by children are peas, beans, berries, grains of corn, buttons, pebbles, small coin, &c. In this section of country, the berries of the Pride of India or China tree, (*Melia Zedarach*), a common shade tree, are very often forced up the nostrils by our children, and are sometimes found difficult to remove in consequence of their hardness and polished surface. By dilating the nostril with a speculum auri, the body may be easily seen, and generally removed without difficulty with a pair of small dissecting forceps. In other cases, a very small tenaculum or a pair of small *dentated* forceps may become necessary. The

ear-scoop will sometimes answer the purpose remarkably well. If the body be engaged so far back as to render its withdrawal by the interior nares impossible, it may then be forced back into the pharynx with a small probang or a female catheter. There is some danger, however, that, in so doing, the body may pass into the larynx and cause suffocation. This may be prevented by requiring the patient not to breathe at that moment, or (if it be a child who cannot be controlled) by forcing back the object during *expiration*. By carrying a wooden spatula upon the tongue as far as the posterior wall of the pharynx at the same time that the body is pushed back, the danger might also be lessened. It would, perhaps, be advantageous to place the patient horizontally upon his back, with the head lowered, for the operation.

While on this part of our subject, we may be permitted to state a singular case which occurred a few years since. A gentleman was eating a bit of beef-steak, when he suddenly sneezed and forced the morsel from his mouth up into his nostril. He hastened to a physician for relief, who, finding the foreign body visible through the anterior nares, endeavored, in vain, to draw it out with forceps. It was firmly wedged, and the fragments seized with the forceps would break, without relief. The manipulations had in the mean time so much irritated the mucous surface that farther efforts were deemed unadvisable, or would not be tolerated by the patient. Still very much concerned, however, the patient applied to us. With a female catheter, the steak was without difficulty forced back into the pharynx, and spit out. It was found to be as large as the end of his thumb, and but slightly masticated.

Bodies lodged in the nostrils should always be removed as early as possible, for they soon occasion pain, tumefaction, and subsequently distressing inflammatory symptoms in the nasal passages, which increase very materially the difficulty of the operation. Instances are on record in which beans have been allowed to remain until they have germinated, and given rise to the most distressing train of symptoms.

Foreign bodies lodged in the pharynx may rest upon the larynx so as to impede respiration more or less seriously—or they may produce a tumefaction of the mucous membrane,

which, by extending to the rima-glottidis, will simulate the peculiar affection termed Œdema Glottidis—or they may be so situated and small as to occasion more inconvenience than real danger.

When visible, these objects may be easily removed with a pair of ordinary dressing forceps. They may often be made to drop out by holding up the patient by the heels, if a child, or by causing him, if an adult, to incline his chest over the edge of a bed or table, so as to rest his head upon the floor a few moments. Sudden succussion of the body whilst in this position will favor the dislodgment of the object. We have fresh in memory a case in which an intelligent mother seized her child by the heels, and with a few strong shakes relieved it from impending suffocation by the dislodgement of a toy that had passed into the pharynx.

When such small objects as pins, needles, fish-bones, rye-beards, &c., get into the pharynx, they are sometimes hidden behind the pillars of the soft palate or the tonsils, or so deeply situated that we cannot see them. In such cases, an exploration with the finger will often enable us to feel the object, and thus to guide the forceps to it. If we cannot feel it, we should resort to the probang in the hope of entangling the object in the sponge so that it may be withdrawn. This very often succeeds. If not, we may pass down a pair of curved forceps, (œsophagus forceps will answer, if we have none shorter,) and by carefully opening and closing them when in the region indicated by the patient as the seat of pain, we may sometimes seize and remove the object. Care should be taken, however, not to wound the epiglottis nor other soft parts. If all our efforts prove abortive, it may be well to administer an emetic and to provoke free vomiting by large draughts of water.

But by far the most serious cases are those in which the foreign body is engaged in the rima-glottidis or has passed into the wind-pipe. These accidents are recognized by the immediate difficulty of breathing, which is more or less great, according to the size and shape of the object, and which, although slight at first, may increase to an alarming degree more or less rapidly, and occasion suffocation, unless the obstructing agent be removed. The patient coughs violently, and is usually able

to indicate the cause of his distress. Attempts should be immediately made to remove it. The patient should be at once placed with the head in the most depending position, so as to facilitate the ejection of the foreign body by coughing. If the object be heavy, the chance of success by this method will be correspondingly increased. If suffocation be imminent, no time should be lost in performing laryngotomy, or rather tracheotomy, and in endeavoring to remove the object with small curved dressing forceps if it can be reached. If the opening in the wind-pipe be made large, as it should be, the foreign body will, not unfrequently, be forced out through it; if not, it should be removed with forceps. When the object is situated within the rima-glottidis, or between it and the artificial opening, the patient experiences a sense of relief as soon as this opening is made, which might lead to the supposition that the obstruction was removed. In such cases the foreign body must be either pushed up into the pharynx with a catheter or seized and extracted with forceps, as may be most convenient.

In Surgery as well as in Medicine, every practitioner will occasionally encounter anomalous cases—cases for the management of which written authorities have not provided. As in our judicial tribunals cases are continually met for the adjudication of which the Statute books are vainly ransacked, and even the Common Law found to yield but faint light—so will the physician at the bed-side, not unfrequently find himself utterly unaided by systematic writers and thrown upon general principles, perhaps of doubtful value. It is to supply this deficiency that “cases” are reported by the legal profession, and likewise by practitioners of medicine. We have just epitomized the subject of foreign bodies in the air-passages, as usually treated, and will now relate the history of an anomalous case that came under our notice during the last winter.

York, a negro boy, 12 years of age, belonging to Mr. —, of Columbia county, was engaged in November last in removing cockle-burs from the mane of a horse, and put one of them in his mouth. By a sudden inspiration the bur was carried down his throat, and he immediately experienced some difficulty in breathing, attended with frequent coughing. Medical aid was invoked and an emetic administered without relief.

The boy continued in this state several days, and was then brought to this city. We found that he breathed and coughed as though affected with œdema glottidis or with membranous croup; his voice was extinct and he spoke in a whisper; on walking briskly he suffered for want of breath; he pointed to the thyroid cartilage as the seat of soreness; had some arterial excitement; nothing abnormal heard on auscultating the lungs, but a whiz was perceived on placing the stethoscope upon the larynx. By the most careful ocular inspection of the pharynx the bur could not be seen. The finger being, however, carried down below the epiglottis, would feel the bur rise up against its extremity whenever the larynx was elevated by an attempt at deglutition. The cockle-bur was evidently situated vertically, with one end within the laryngeal aperture, and so securely fixed by means of its minute hooks into the mucous membrane, that its position could not be changed by such delicate touches with the finger as I thought it prudent to make during the momentary contact alluded to. A pair of œsophageal forceps being at hand, I made, in vain, repeated attempts to seize the bur, until the patient became very much exhausted. The continual movements of the larynx presented an insuperable difficulty. He was then allowed to rest, and an emetic of ipecacuanha administered in the evening, in the hope that the bur might be dislodged during the efforts to vomit. This also failed, as it had done before.

On the following morning (8th Nov.) I provided myself with a pair of small curved polypus forceps, and carrying the index finger of the left hand down below the epiglottis, forcibly drew this upwards, and at the same time glided the finger still lower, until its extremity rested in contact with the bur. The forceps were now, with the right hand, carried along the finger, and the bur effectually seized and extracted, after but one failure.

In looking over the standard surgical authorities, we find no allusion whatever to the method adopted in this case to bring the finger in contact with the larynx, and to stay its movements during the introduction of the necessary instruments. Some of those who treat at all of the removal of bodies lodged in the rima-glottidis, recommend in general terms their extraction with forceps if possible, and, when this cannot be done,

advise immediate recourse to Tracheotomy. Others make no mention whatever of the use of forceps, but resort at once to the knife. We have not had the leisure to examine *all* the written authorities at hand, and would therefore simply lay the subject before the profession, who may judge of its value as well as its originality. It may be sometimes difficult or impossible, in adults, to introduce the finger sufficiently far below the epiglottis to draw this forwards effectually and to carry the finger to the larynx at the same time that its movements are prevented. But in children this can very rarely be the case.

ARTICLE XXVI.

Tetanus, successfully treated. By J. C. BILLINGSLEA, M. D., of Tuscaloosa Co., Alabama.

My Dear Sir—Below you will find an account of a case of tetanus which fell under my care recently. If you think it worthy of a place in your journal you can publish it.

April 27th, 1853. Was called to see a young negro man, aged 18 years, the property of Mr. F. I was informed that, four days previous to this, he had been thrown from a mule, and after the fall, he complained of "the small of his back and breast hurting him." I found the boy sitting up at 12 M. He had considerable fever; pulse 120 per minute, and resisting; tongue foul; skin very dry; not much ability on the part of the patient to move his lower extremities; considerable tenderness of the spine about the last dorsal and first lumbar vertebræ; his chest somewhat tumefied and painful to touch, just above the epigastrium. He said the mule had trodden on him. In addition to these, my attention was called to his left foot, the great toe of which had been frostbitten nearly eighteen months before, but was thought by his master to be *well*, until he received the fall. The toe was much swollen, and had a cavity in the end, about a quarter of an inch in diameter, which extended to the bone and was filled with pus. The whole foot was very much swollen and tender. He did not know whether it was hurt in the fall or not, but said it did not commence swelling till after the fall. He was robust and strong. I bled

him from a large orifice, but took only a small quantity before a sensible impression was made on his pulse, and he expressed himself as free of pain, and much better. I then applied cups over the tender portion of the spine, and afterwards sinapisms to the same—then ordered him the following: ℞. Calomel, Dover's powders and sulph. quinine, each 12 grs., divided into six powders—one to be taken every two hours. Tinct. of iodine to be applied to the foot, followed by a cold poultice. He had no tetanic symptoms that I discovered at this visit, although he said the pain in his chest came on by paroxysms.

28th, 11 o'clock, P. M. Was called in haste to him. I found him with pulse 120 per minute; tongue moist and cleaning; slight diaphoresis; and whenever touched, or spoken to, he would be thrown into violent opisthotonos, and during each spasm would complain of his chest. I think there was not more than three minutes of repose between the spasms. I was informed that he had been in this condition all day—his medicine had not acted on his bowels; fortunately, he could open his mouth about half an inch, and deglutition was not impaired. I gave him an enema of castor oil and spts. of turpentine, which I repeated frequently, until I procured a full evacuation of his bowels—then gave him a warm bath, applied cups to the spine, and afterwards a blister to the same. After the bath, he seemed some better. Between that time and day-light I gave him about an ounce of paregoric. In the meantime his paroxysms were not so frequent, and he slept some.

29th. I had the room darkened early and kept quiet. At 6 o'clock, A. M., ordered 4 grs. of quinine every hour, with just enough paregoric to keep him dozing (which was not much, as he was very susceptible of its effects). The most nutritious diet admitted.

30th, 8 o'clock, A. M. I am informed that the paroxysms were not so frequent or severe yesterday as they were the day before; slept well last night, and was not then conscious of spasms, but now having one every few minutes—pulse 110 per minute. Evacuated his bowels by enema; continued the quinine as yesterday. At this visit, I discovered a fluctuating tumour about the size of a hen's egg in the instep of the diseased foot, which was very painful on pressure. The fluid did

not feel like pus. The swelling was fast leaving the foot on the application of poultices and tinct. of iodine. I now began to think that this toe was the cause of all the trouble; and as the foot was in a very unhealthy condition I did not like to remove it (the toe) without first advising with some older surgeon.

May 1st. Was met by Dr. Hayward, of Tuscaloosa, in consultation. After a thorough examination, he also concluded the toe was the cause of the tetanus, but thought it best not to remove it in the present condition of the foot. His symptoms were a little better to-day: pulse 100 per minute; spasms not quite so frequent. Dr. H. could see no reason for changing a practice which seemed beneficial, but looked upon his recovery as *very* doubtful. We ordered 10 grs. quinine and $\frac{1}{4}$ gr. morph. every two hours; an enema of castor oil and turpentine.

May 2d, 8 o'clock, A. M. Just as he was yesterday. I ordered 15 grs. quinine and $\frac{1}{4}$ gr. morphine every two hours. This was kept up all day and night.

May 3d, 8 o'clock, A. M. Complains of his head roaring; pulse 90 per minute; profuse diaphoresis; skin cool; a spasm about every 15 minutes—said he was very weak, and appetite not good. Ordered 10 grs. quinine and $\frac{1}{4}$ gr. morphine every three hours, and food *ad libitum*.

May 4th. Very deaf; had no spasm last night, but slept all the time, only when awaked to take his medicine—pulse 72, and weak; skin cool; profuse diaphoresis; appetite returning; has a spasm about every half an hour. I ordered 5 grs. of quinine, and $\frac{1}{4}$ gr. of morphine every two hours. In the afternoon he became partially blind from the effects of the quinine, and I omitted it for six hours, when this unpleasant symptom subsided.

May 5th. No spasm last night, but an occasional one through the day. Ordered quinine 4 grs. and $\frac{1}{4}$ gr. morphine every three hours.

It is useless to give farther details. Suffice it to say that his improvement was gradual and continuous, and in two weeks he had no symptom of tetanus remaining. I gave the quinine in diminished doses for a week after. The tumour in the instep broke, discharging a considerable quantity of thin, unheal-

thy pus. I dressed this and the toe with quinine, and put the boy on cod-liver oil and a little brandy three times a day. At this time he is fatter than I ever saw him, and his foot nearly well. I think his toe will also get well without an amputation.

I had just read the case reported by Dr. Pye in the New Orleans Medical Journal, and re-published in your Journal in March. I thought smaller doses of quinine than those he recommended would answer the same purpose; for, the absorbents acting very suddenly and powerfully might produce alarming, if not fatal results, after such large quantities had been administered.

ARTICLE XXVII.

Another White African.

WHITEVILLE, Harris Co., Ga., May 16, 1853.

Dr. Dugas: Dear Sir—I noticed in the Eclectic department of the April No. of your Journal the report of a case of "Change of Color in an adult negro." I propose, briefly, to present you with a case of the same kind, equally as remarkable and much nearer home.

Charlotte, is a woman 34 years old, and living within one mile of me, whose skin is as fair as that of any lady of Caucasian blood, and who was as black at the age of 11 as any African. She says her health has been uniformly good, with the exception of one "spell of bowel complaint," which occurred when she was about 10 years old; sometime after which a white spot appeared on her forehead, which gradually though slowly enlarged. In the mean time other spots appeared on different parts of the face, which also increased in size, until the whole face became perfectly white. The change in the color of the face was completed in about six years, and she says that after her face "turned white," her whole body changed in one week. I saw her frequently during the metamorphosis, and noted its progress, during which time her countenance was so hideous that she was a fright to all the juveniles of the neighborhood.

She is at present strictly a white woman, except her eyes and hair, which are those of the negro—the latter, however, being whitish all around the margin. The skin freckles, and is

easily blistered by the sun. She is a good servant; has ordinary intelligence; has had ten healthy children, all of whom are as black as their father, who is a full-blooded African.

The above are the facts of this "strange freak of nature." Was the change a pathological or a physiological one? I am inclined to the latter opinion, because the subject was in good health, and the skin seemed to be so, during the change. But what strange whim possessed the absorbents to remove the pigmentum nigrum?

These are questions I shall leave to be answered by those who have more taste and talent for speculation than your humble servant,

E. C. HOOD, M. D.

ARTICLE XXVIII.

[We are happy to be able to corroborate the testimony of Dr. Booth in favor of the value of Quinine in the infantile affections of the bowels, usually denominated Cholera Infantum. We have been using it ever since we saw his first suggestions in reference to it, and regard it a very important addition to our resources.]

Quinine in Cholera Infantum. By G. W. BOOTH, M. D., of Carrollsville, Miss.

As the season is near at hand when that scourge of infancy, Cholera Infantum, usually makes its appearance, I will again invite the attention of the profession to the views I entertain of its etiology and treatment as published in some of the medical journals in 1851. In the communication referred to, I stated it as my opinion that it was of malarial origin. There are many reasons that I could give to sustain the correctness of this opinion, but I deem it unnecessary at this time.

My views respecting the origin of the disease influenced me to use the great and approved antiperiodic, Quinine, in its treatment. The success I met with in combating the disease with this article still farther confirmed me in my convictions as to its predisposing cause. I earnestly solicit the profession to give Quinine a fair trial in Cholera Infantum: I feel confident that no one will regret the experiment. I use other articles to

meet particular indications, such as acetate of lead, and calomel, in small doses, for controlling the discharge from the bowels, acting on the liver, &c.

PART II.

Eclectic Department.

Extracts from the Records of the Boston Society for Medical Improvement. By WM. W. MORLAND, M. D., Sec'y.

SEPTEMBER 27, and OCTOBER 11.—Cases of Arrest of Development, Blemishes, etc., in Infants, attributed to Strong Mental Action in the Mother, while Pregnant; Disagreeable Sights; Sudden Shock, &c; Discussion upon the Question whether such Action of the Mind be ever a Cause of such observed Results.

CASE I. *Arrest of Development attributed by the Mother to a Fright during Pregnancy.*—Dr. Storer reported the case. Several months since, a female patient of Dr. S.'s expressed some anxiety respecting her sister, who expected shortly to be confined, and who could not be persuaded that she would not have a deformed child. Three or four weeks after her marriage, she was much affected at seeing a hen injured by a stone thrown by a boy—the stone broke one of its legs, and removed the lower portion. She was exceedingly troubled at the time, and as her pregnancy advanced, continually dwelt upon the subject, insisting that her child when born would be found to be deformed. Her friends at last began to feel some concern as to the result, and hence the reason of Dr. S. being consulted. A few weeks afterwards, Dr. S. heard from his patient that her sister had been confined, and that one foot was wanting; that, immediately after her delivery, the mother asked to see her child; one foot was shown her as being natural, and an effort made to divert her attention from the child; but she could not be pacified until she saw the other limb, continually insisting that she knew it was deformed. Dr. S., being very anxious to see the child, requested that he might know when it should be brought to this city.

Since the last meeting of the Society, he has had an opportunity to examine it. The child, perfectly well formed in other respects, exhibited upon one of the lower extremities, simply a heel and the rudiments of the five toes, at the extremities of which were placed microscopic nails. Dr. S. observed that he was led to refer to this case from the peculiarities of the patient's conduct previous to her confinement. It was not an uncommon circumstance for a mother, *after* the birth of a de-

formed child, to account at once for the defect by something which had occurred during her pregnancy; but he believed it to be very rare for a patient to dwell upon the subject for months *previous* to her confinement, and immediately upon her delivery insist upon seeing her offspring to prove her conviction.

Dr. Bigelow, Sen., remarked that very many of these cases are referable to afterthought of the mother. Women are often anxious in regard to the possible deformities of their offspring, and there are few pregnant women who escape seeing some peculiar and even disagreeable sight or objects during their gestation; any marks observed would, therefore, be coincidental, *by the rule of chances*: in ninety-nine out of a hundred pregnant females, no untoward results would be observed. Dr. B. does not believe it possible that an arrest of development can arise from the action of the imagination of the mother, or from the effect of sudden disagreeable impression on her mind. It is not infrequent that mothers inquire *whether their child be perfect*; at all events, the *frequency* of deformities after any shocking sight is the *test* of the truth of the existence of such *cause* for deformity. Dr. B. believes that if one hundred pregnant women were to be exposed to the action of such supposed causes, or should experience any of the strong and peculiar "longings" of pregnancy, no blemish would be thereby produced in the children.

Dr. Cabot referred to the fact that, during the siege of Antwerp, there were very many *stillborn*, and a large proportion of *deformed* children.

Dr. Durkee asked, if this theory of the influence of mental impressions on the pregnant female does not find support in certain analogies observed among animals? He referred to Old Testament narration (*Genesis*, chap. xxx.), and added that remarkably similar facts may be observed at the present day in the breeding of animals.

Dr. Jackson said that very likely there might not be one in a thousand, exposed as suggested by Dr. Bigelow, in whom unfortunate results would follow; *all* are not *equally susceptible* of such influences—some not *at all so*; the *special* cases, however, are not to be explained or ridiculed away; there is enough of plausibility, if nothing more, in the view he sustains of the matter, to render women cautious of needless exposure to unpleasant sights in the *early months* of pregnancy.

Dr. J. added that, from the cases he had seen, or heard of upon good authority, he was inclined to believe that a malformation in the fœtus may be induced by an external cause operating upon the mind of the mother during her pregnancy, and,

further, that the malformation may bear to the cause some relation or resemblance. This last has always been a popular notion; but is regarded, on the other hand, by the scientific, as perfectly absurd; the presumed cause operating upon the mother at a period of pregnancy when the fœtus must be too far developed, it is said, for any such malformations as have been referred to, to be produced. There is, moreover, no nervous communication between the mother and the fœtus. The number of negative facts is numerous beyond account; of cases, that is, where a sufficient cause, according to the above hypothesis, existed for malformation, and yet none occurred. No possible explanation can be offered for the production of a malformation by the presumed cause; nor will it be possible, until we know much more than we do of the obscure subject of the physiology of the fœtus. The resemblance between the malformation and the cause, it is said, is a mere coincidence; but, upon the common doctrine of chances, the coincidence is too remarkable to be explained away so readily, and, if one case is suggestive, a second adds very great weight, and a third is almost or quite conclusive. M. St. Hilaire (*Anomalies de l'Organisation*) believes that some monstrosities, the anencephalous for instance, owe their being, in some cases, to the operation of a strong moral cause upon the mother; but he does not believe in any resemblance in the particular malformation to the exciting cause, and for the reason above mentioned. It would have been well had he stated at what period of pregnancy the mothers, in his cases, were subjected to the causes that led to their monstrous births. Another remark often made, is, that pregnant women do not predict the malformation, but, when it is discovered, look back for some cause to which they can refer it. This might be questioned; but, allowing that it is so, and even that they had ceased to think of the assigned cause, it does not follow that it had not operated efficiently at the time; the impression was made upon the mind of the mother, and through it upon the fœtus—the one was soon effaced, the other remained. Much would depend upon the character of the exciting cause, and much also, as he had before remarked, upon the *susceptibility* of the individual to the receiving and the retaining of impressions. Dr. J. then reported the following cases:—

1. Several years ago, he saw a young lady, about thirteen years of age, of rather a small and delicate figure, and whose middle finger upon one hand was fully equal in size to that of a man's; the corresponding metacarpal bone being also decidedly enlarged, and even the forearm somewhat so. The finger was itself well formed, as in some other cases of similar malforma-

tion that have been observed here. The hand also was otherwise well formed.

The mother of this young lady, during her pregnancy, was obliged for a long time to dress a felon or whitlow for an old uncle, the finger affected corresponding to the one that is malformed in the case of her daughter; and the operation was always disagreeable to her, as she was a woman of a particularly nervous temperament.

2. In November, 1846, Dr. J. saw an infant with an extensive blood-mark, as it may be called, upon one of the upper extremities. The mother, whom he has attended for several years, and knows to be a woman of very susceptible feelings, gave the following statement: When two and a half to three months pregnant, though not at the time aware of her situation, she had been out of town for the afternoon, when, on her return, as the cars entered the depot, she saw a man who had been recently killed. He was lying upon his face, with his head turned to one side, the sleeve of his coat torn entirely off, the whole upper extremity exposed and more or less bloody, and so twisted, as she supposed from dislocation at the shoulder, that the hand lay upon the upper part of the back. At this sight she nearly fainted, and probably would have entirely, if she had not had two of her children with her. For at least a month afterwards she was decidedly ill; and during the remainder of her pregnancy she could not banish the thought from her mind; she also felt that there would be great danger in regard to the child in utero, though she never expressed her anxiety upon this point to any one. The extent and degree of the discoloration of the skin were not fully recorded by Dr. J. at the time; but, having recently seen the mother, she informs him (for the child has since died) that it extended from the back of the hand and towards the thumb, where it was most marked, upwards along the extremity to the shoulder, and even somewhat upon the neck. It was not equally marked throughout, being in some parts more or less continuous, and in others simply spotted or dotted. Her other children she has always dressed in short sleeves; but this child's arms she covered, on account of the marked deformity; and, as she says, if it had lived, it could never have worn a low-necked dress. The child itself, moreover, was so sensible of the mark that it would occasionally be seen endeavoring to wipe it off. The discolored arm in the child corresponded, as to the side affected, with the one that was injured in the man.

3. Dr. Z. B. Adams related to Dr. J., several years ago, the following case: He attended a woman in labour, and the child was found to have one of the forearms terminating in a conical

stump just above the wrist. During her pregnancy she attended for several weeks upon her brother who had had his hand torn off by machinery, and the forearm afterwards amputated; the injury being upon the same side as the malformation in the child.

4. Case reported by the late Dr. Doane, of this city. The mother of several blind children was holding one of them in her lap, when it put its finger into one of her ear-rings, drew it down forcibly, and tore it through the flesh. She was pregnant at the time; and when her child was born, it had a fissure in the ear corresponding to the laceration in its mother's.

At the next meeting, Dr. Storer said he had met with reports of two cases since the last meeting, which tended to confirm the opinion at that time advanced, that any remarkable sensation sustained by the mother during her early pregnancy, might produce an effect more or less well marked upon the *fœtus* in utero—that this effect need not necessarily partake of the character of the exciting cause—but that, should the mother be so far influenced as to receive a decided shock upon her system, it might be followed by some abnormal condition of her child—arrest in development or some peculiar malformation. The following cases he had observed in the "Proceedings of the Medical Association of the State of Alabama," for December, 1850.

"Dr. R. Lee Fearn related the following very remarkable particulars of a case, where the impressions received by a mother during pregnancy, affected her child in utero. A gentleman, whilst shooting, shot through the metacarpal bone of his index finger. The wound was a bad one, and piece after piece of the bone came away. A few months after the accident here mentioned, and in due season, his wife bore him a child perfectly formed in all respects. When about four months advanced in her second pregnancy, an operation was deemed necessary to remove the last remaining portion of bone in her husband's finger. She witnessed the operation, and was much shocked and alarmed at the sight. When her child was born, it was found to be deficient in this very bone, though in all other particulars it was a well-formed child. The Doctor thought this was by no means the result of chance, but a very conclusive instance of cause and effect.

"Dr. Dossey remarked, that the relation of this case called to his mind a similar instance:—

"Dr. G—— was thrown from his horse and broke his leg, midway between the ankle and knee. His wife was about five months advanced in pregnancy. When the child of which she was pregnant was born, it had on the leg corresponding with

the injured limb of the father, and at precisely the same spot, the appearance of a fracture of the limb, and there was also a decided shattering of the leg."

Dr. Strong thought that the notion so popularly entertained in reference to this matter is too general not to have *some foundation in truth*. It certainly deserves investigation. He added, that the idea of greater liability to these attributed effects, from the causes mentioned, in the *early* part of pregnancy, seemed to him very well founded; perhaps the influencing power, be it the imagination, or what it may, *ceases after a certain period of gestation*; has a *limitation*. Dr. S. also referred to the fact that a *mare* covered by a *quagga* always afterwards, when impregnated by a *stallion*, brought forth *striped colts*.

Dr. Cabot mentioned the statement of M. Donné, of Paris, who declares the above fact, as stated by Dr. Strong, to be constantly observed under the same conditions; he even goes farther, and asserts that one portion of the constitution of one man is propagated, *by another man*, from *the widow of the first to children by the second*.

Dr. Bigelow, Sen., asked to what cause we must attribute the imperfections and arrests of development observed in fruits, flowers, and plants. Have *they* imagination? and does it act with this effect upon them?

Dr. B. declared his entire disbelief in any such effects from the attributed causes.

Dr. Hayward, Sen., asked whether this action or influence (if it be recognized as efficient) is *imaginative* or purely physical? Dr. H. mentioned a mother, who, while pregnant, suddenly saw one of her children, ill with pneumonia, covered with blood by some accident; the child she was carrying, when born, had a *large red stain* or spot upon its face. Here, imagination could hardly be the cause, for the woman had not thought of, or brooded over, the occurrence, nor had she any apprehension that her infant would be marked.

Dr. Homans spoke of an acephalous fœtus, the mother of which during pregnancy had not even apprehended any marring of her child. Dr. H. does not remember, since that case, any other wherein the mothers had *anticipated* any monstrosity, although such did occur. These facts he considered would militate with the imagination doctrine, or the idea of effect from strong mental impression or shock on the fœtus in utero.

Dr. Strong remarked upon the difference between mental and physical laws in their action on the system. Some individuals have, and some have not, the imagination sufficiently sensitive and impressible for the production of such marked

action as has been referred to. The *final question*, however, is whether such results are *ever* produced. If this be proved and conceded, it is sufficient to make the fact a fixed one. One instance thus established is enough, and as good as *ten thousand*. The numerical system is valueless upon this question.

Dr. Bigelow, Sen., would pronounce this principle of different impressibility, &c., to be the foundation of all the charlatantry of the day. The remarks of Drs. Jackson and Strong go to prove only the possibility of these occurrences under the above supposed theory, but do not establish their certainty. There is not sufficient basis, as he views the subject, for making a rule or law in regard to it.

Dr. Strong asked if any one had seen so strongly marked special cases as that of the slitted ear and the enlarged finger narrated by Dr. Jackson?

Dr. Hayward, Sen., mentioned two cases of slitted ear; the slit being in the portion in which ear-rings are inserted. One individual, when seen by Dr. H., was 16 or 17 years of age. The mother did not remember any accident or sight possibly causative of such a result.

Dr. Wm. T. Parker related the following case which occurred in his father's practice in Virginia, many years ago: A lady, three months advanced in pregnancy, saw a pig, driven furiously out of an inclosure by a negro boy, have *its bowels torn out* by the stake of a fence. The lady was greatly shocked, and fainted. Her child, when born, had the entire front of the abdomen *covered only by a thin film, and the intestines were visible through it*. There was also imperforate anus. The child died not long after birth.

Dr. Channing related the case of a lady in Edinburgh, of which he heard while there lately: Her surgeon had been performing the operation for hare-lip, and the lady's attention was drawn to some blood which chanced to remain upon one of his fingers; at her request, he described the operation to her. She was from four to five months gone with child; at birth, *the child was found with hare-lip*. The mother stated that she had been much impressed by the above occurrence and narration.

Dr. C. alluded to the fact that the umbilical cord sometimes performs amputation of fœtal limbs.

Dr. Coale said that the agency of mind upon matter cannot be denied. Sometimes the cause and the effect of nervous impressions are greatly disproportionate the one to the other. The action of these impressions on the stomach and bowels is undoubted. Dr. C. mentioned a patient under his care who, during her pregnancy, saw a deformed man, and was greatly affected and troubled at the occurrence. Previous to her con-

finement, however, the vivid impression subsided, to be renewed during the throes of parturition. She became very apprehensive of possible deformity of the child about to be born, which, however, presented none.

Dr. Bigelow, Jr., thought the *negative*, as well as the *positive* evidence on this subject should all be weighed; otherwise, *fallacious results* are nearly sure. *Strong numerical evidence* and *many cases* seem to him necessary for a decision; isolated cases are insufficient.

At the meeting holden Nov. 8, Dr. Storer reported that he had, during the last week, delivered a woman of an infant with hare-lip; the mother, the day after confinement, told him that she had been very painfully impressed while visiting, during her own pregnancy, a friend who was dying from phthisis, and who, during her visit, had a severe and sudden attack of hæmoptysis, accompanied by distressing dyspnœa. The pregnant lady was greatly shocked, so much so as to faint; and she subsequently thought frequently of the occurrence, fearing its effects upon the child she carried. This infant was born with a hare-lip of double fissure.

Dr. Gould mentioned, as a "set-off" case to the above, the following: On last Monday, he attended a woman in labour, who, at the birth of her child, was very anxious that its palate should be examined, she having, early in her pregnancy, seen a person with very disagreeable countenance from deformity of the palate. She had been very unpleasantly affected by the sight, but, notwithstanding her very strong apprehension, no effects are visible upon her child.

The discussion of the subject was not resumed.—[*American Journ. of Med. Sciences.*]

Letters upon Syphilis. Addressed to the Editor of L'Union Médicale, by P. RICORD. Translated from the French, by D. D. SLADE, M. D.

[Continued from Page 409.]

SIXTEENTH LETTER.

My Dear Friend,—Most decidedly *we cannot please everybody*; and this old adage, so ingeniously presented by La Fontaine, is particularly applicable when medical science is concerned. The monkeys have brought me ill luck; I have not satisfied the experimenters, who have pretended to have inoculated them with syphilis, and I have much less satisfied those who do not believe in this pretended inoculation. However, see how mistaken I am, since I had the naïveté to think

that from these two parties I merited some praise. You will see what was my error.

The young Bavarian colleague who has just inoculated his name with syphilis, has reproached us, myself and others, *of having been hasty in our conclusions upon the non-transmissibility of syphilis to animals*. However, if I count correctly, more than *twenty-four hours* have passed by since Hunter, and the time has been sufficiently long for me to reflect, and that, too, without too much precipitation.

On the other hand, the colleagues whom I respect, and who ordinarily entertain the same ideas with me, have reproached me in almost the same way. They have discovered that I have been a little hasty with the monkeys; they believe—they tell me—that I have yielded to apish tricks. My learned and able colleague of the Hospital du Midi, M. Puche, is yet in a state of perfect incredulity relative to the transmissibility of the syphilis to animals, nor does M. Cullerier, that persevering experimenter, believe the reality of experiments which make so much noise.

What I recounted to you in my last letter, I have seen with my own eyes; I have also told you the attenuating circumstances, which it is impossible to put to silence, however satisfied of the convictions and good faith of M. Auzias Turenne. But after having told you of this fact of the inoculation of the virulent pus from man to the monkey, all that I know of the matter, *I am astonished at the sudden and premature conclusions which our German colleague draws from the fact*; and to speak frankly, he who exacts in others so much maturity of reasoning and reflection, has not himself offered the example. After all, the promptitude of his conclusions can be excused on the ground of the very inoculations to which he has courageously submitted himself, and which he would have been very glad not to have made uselessly.

Our German colleague makes much of this proposition:—*"One single positive experiment has more value than an innumerable quantity of negative results."* Without doubt; but upon one condition, which is that this experiment should be positive, that it should be incontestable, and that it should present all the guarantees of certainty and exactitude, and, more than this, *that it can be repeated*; without all this, it is worth nothing. The Academy of Science knows the value of this proposition constantly brought forward, and by which, periodically, rash and new experimenters pretend to overthrow the laws of physics. This argument has served for all human deceptions. What says the magnetologist who pretends to transport the sense of sight to the nape of the neck or to the epigastrium?

Precisely what our German colleague says—viz: one single positive experiment, &c. What says the homœopathist, who maintains that an atom of *bryonia* diluted in the immensity of the waters of the ocean can cure pneumonia? Precisely the same thing as our German colleague.

In the physical and natural sciences, *one isolated fact is worth nothing if it is not susceptible of being repeated.* Here is what all those think who know what the philosophy of science is. Otherwise this would be the most dangerous and the most perfidious stumbling block to progress, if laborious and patient observation did not come in to prove that it was but a sophism, an error, and often only a boast. My honorable colleague and friend, M. Cullerier, ought himself to tell you what he thinks of the experiments of M. Auzias. As to myself, I have established this, that the virulent pus has been transported from man to the monkey, and from the latter it has been inoculated upon man; nothing more, nothing less. Here is the plain fact; afterwards comes its interpretation.

I said to you in my last letter, "*Might not the monkey have served herein only as a soil for transplantation?*" I believe so, for here is what happens—the puncture of the inoculation which has been made upon the monkey, scarcely irritated, scarcely inflamed, and suppurating very little, although soaked in the virulent pus after it has been made, has a constant tendency to heal up, and this happens with astonishing rapidity. We do not see in the inoculation made upon the monkey that ulcerating, continued, increasing progress which is the character of the chancre upon man, especially the chancre which does not become indurated; we do not find even that period of specific *statu quo*, so tenacious, so long, which nature keeps up in man, and which he has ordinarily so much difficulty to destroy. There is never in the monkey the least phagedenic tendency; nothing which resembles the specific induration in its commencement and in its consequences. A puncture, scarcely any suppuration, a crust, and a cure! Herein are the effects of inoculation upon the monkey—and all this takes place almost as quick as one of his gestures. We see that it is for the chancre a refractory and foreign soil; the virulent seed is there exotic; in vain we take much precaution to sow it well, water it, to place it in a green-house, or under a bell; it dies before having thrown out any roots, and consequently without having given forth any fruits.

M. Auzias explains all this by the great vitality of their circulation; it would be more easy to explain it by their nature so averse to syphilitic virus, upon which I congratulate them. We can even believe that in the pustule which is produced with so

much difficulty, the virulent pus serves there only as an issue-pea which irritates, causes suppuration, but is not combined with the tissues; it is mixed with the pus which is produced, that is all. It would be necessary, in fact, to be able to conclude definitely upon any other result, that the pustules produced upon the monkey were broken, that the ulcerated surfaces were frequently cleansed, in order that we should not suppose that there remained some pus of the chancre mixed up, and that we inoculated afterwards the suppuration furnished by these surfaces. We know what happens in man. We may in vain cleanse the surface of the chancres, apply to them even medicated substances; still the virulent secretion continues to be produced. As long as we shall not have carried out this experimental programme, the sole experiment which has been made will be insufficient to destroy all which has been established by serious men upon numerous and perfectly-established facts. The sole acquisition made to science, and which I am perfectly ready to recognize, is, that we can place and preserve the virulent pus upon the monkey, and afterwards make use of it to inoculate man, as one transplants a plant from one soil to another. That is all that I have seen and established, and the only deduction which I can draw from it.

Until a new order of things, then, our German colleague would be in the same condition as regards his inoculation, as if it had been made with virulent pus preserved in tubes or between two layers of glass.

This induces me to tell you what the pus inoculated upon man produces, the course which inoculation follows, and what it teaches as regards the pathology of chancre. But you inform me that my honorable colleague and friend, M. Cullerier, asks of you permission to speak. I yield to him with pleasure; we shall all gain from it.

SEVENTEENTH LETTER.

My Dear Friend,—I think that I have done justice to the monkeys; for the present, I shall not occupy myself any more with them. If later, it can be proved to me, that they can contract anything but what I have told you, I shall be found always ready to acknowledge it. Until then, I do not see any motives to change my opinion. In waiting, let us return to the poor human species, to whom, at the present day, no one contests the claim to the verole as an inalienable right.

However, before going farther, permit me, after all that I have said to you, and perhaps even by reason of what could be recently said, to establish the following proposition, which appears to me to be impossible to overturn:

The chancre (primary ulcer) at the period of progress or of specific *statu quo*, is the only source of the syphilitic virus (morbid inoculable poison).

I have already told you in what conditions the virulent pus ought to be, in order to act; you know, also, the conditions in which the parts ought to be, in order to undergo the action of it. Let us now study the effects of this action; in other words, the pathogeny of the chancre. This subject is a serious one, but a little dry. I depend upon all your good will, to follow my developments. Please to look for no other interest than that of the question itself.

If we make a puncture under the epidermis, with a lancet charged with virulent pus, this puncture, which ought scarcely to bleed, soon grows red, becomes prominent, and its summit is raised up by the serosity, which soon becomes turbid in order to take on afterwards the characters of pus.

Thus, puncture, redness, papule already surrounded with an areola, vesicle, vesico-pustule, and finally pustule; such is the series, the constant succession of phenomena produced by inoculation. All this follows without interruption, without any arrest, from one hour to the other, from one day to another; it is a pathological riband, which is constantly unrolling in order to arrive at a regular and inevitable term, that is, to the production of a pustule of ecthyma, the most perfect, and of the best possible type.

This pustule is often depressed at its summit, even umbilicated at the point which corresponds to the puncture, and upon which we perceive most generally a little drop of dried blood. If the pustule is not broken, the pus which has formed, dries up, and gives rise to a conical, brown, greenish or blackish crust. This crust tends to increase at its base; for it covers an ulceration, the circumference of which tends itself to increase. In this increase of the ulceration under the crust, the epidermis of the areola which surrounds it and the border, is successively raised up by the suppuration; this latter in its turn dries, in order to form a new disk of crust, while a new areola is formed at its circumference, and so on.

Tell me, without ceremony, if I am sufficiently clear in this description; it is of great importance to me to be well understood.

The red circle (the areola) which borders the crust, is ordinarily tumefied, and encloses it as the rim of a watch encloses the glass—only, as there is here an increasing ulceration, and always new pus produced, and as the circumference of the crust is always less hard than its centre, this crust is not generally very adherent. Sometimes the crust is formed early; at other

times the pustule remains in the purulent state during a time more or less long. This pustule sometimes does not acquire a very great volume; often it has at its commencement only the size of a lentil; at a later period its surface might equal that of a five-cent piece and even that of a franc; but it is not rare to see it acquire dimensions much more considerable.

The pustule offers, then, those transitions which we observe so often in other forms, and which give to it the aspect of rupia, either before the formation of the crust or when the crust is formed. There is only here, as sometimes in rupia, a difference of volume. If we break the pustule the second or third day in those cases of quick evolution; or if we break it at a later period in the ordinary cases; or if the crust is detached, we find beneath an ulceration occupying all the thickness of the skin, perfectly rounded, with the borders cut perpendicularly, as if it had been made with a punch. The borders of this ulceration, slightly separated from the adjacent parts, tumefied, serrated, and turned back, remain surrounded by the red areola which constitutes the margin of it; they are covered by a diphtheritic layer, a special adherent pyogenic membrane. The surface of the ulceration secretes a sanious, sero-sanious pus, often reddish, and charged with organic detritus; this is the virulent inoculable pus. When we cleanse this surface, we find a diphtheritic layer more pronounced than that of the borders, and which is also constituted by a special pyogenic membrane, of a greyish color, of a lardaceous aspect, and which cannot be detached. Moreover, the bottom of the ulceration reposes upon a base more or less thick, more or less engorged, according to the progress which the ulceration is to pursue—a progress especially determined by the character of the *soil* in which the *syphilitic grain* has been sown.

The ulceration which I have just described, and which has followed an increasing progress, may arrest itself at the extent which I have already indicated, or persist a long time—a month, six weeks and more, or continue to increase in order to take on larger dimensions, and to present also important modifications.

In the numerous inoculations which I have made, things have always happened regularly, thus:—An incessant evolution starting from the puncture; constant production of an ecthyma, the ulcerating bottom of which, presents in its turn, above all, the classical and typical characteristics of the chancre; ulceration with a *tendency to increase*, or remaining a special *statu quo*.

You already see, my friend, that the artificial inoculation overthrows all that we have been accustomed to teach and to

repeat to each other for ages past; you see it break the physiologism of Broussais; you also see it reduce to its proper value the doctrine of the *physiologic contagion* of a more recent date. And first, can the theory of incubation sustain itself in presence of what inoculation produces, and of those results which you can repeat every day; for, remark, it is not a unique, exceptional fact that I relate to you, but there are masses of identical facts always giving place to the same phenomena, and of which every body has the proof in their hands.

The electric, expansive mode of Bru; it is no longer possible to believe that the syphilitic virus penetrates the economy like lightning, that it is a shock from the individual infecting, to the individual infected. The chancre, the primitive ulcer, is no more the result of a *shock in return*. We cannot admit, at the present day, unless we are blind, that the virulent pus traverses our tissues by a solution of continuity or otherwise, in order to infect first the entire economy, to hide itself at a distance, in order to return afterwards upon its steps to *hatch* in the *nest* where it had been first placed.

Special grain, the syphilitic virus, grows where it has been sown; *particular ferment*, it is those parts which it immediately touches that enter first into fermentation. All this takes place, as we have already said, more or less quickly, according to the disposition of the soil, according to the fermentable aptitude—but all this takes place strictly, absolutely, in a point at first very circumscribed, which we shall contrive perhaps to limit by and by.

The non-existence of a period of incubation, a fact so evident, so true and so logical; is not yet, however, accepted; the contrary prejudices have been of too long standing not to have the force of law, or to be easily overthrown. Those who, notwithstanding, sustain the incubation, and who believe that the virulence of syphilis is compromised if it does not exist, have made me a primary objection; they say to me, if you obtain instantaneous and uninterrupted effects by artificial inoculation; if you have observed only a local evolution; if you have been struck by an apparent silence of the organism, and if you have perceived nothing which explains a general participation in the syphilitic drama, it is because you operate upon an organism already impregnated, injected; you inoculate patients, and those patients are already inoculated.

This objection, you see, enters into the famous theory of *virulent bottles*. I have already refuted it; I have told you what we ought to think of this opinion as respects wounds, injuries and operations made upon syphilitic subjects. I cannot help returning to it; permit me to refer you to what I have

already stated upon this subject. But I have another answer to make to this objection, besides the experiments practised upon the patients themselves. I shall answer this by the experiments made from sick individuals to healthy ones, and I shall invoke especially the recent inoculations practised upon man upon the occasion of the inoculation of the monkeys. Well, in these cases the results of the inoculation have been identical with those which I have just described to you; that is to say, an immediate action, an uninterrupted evolution, and production of the ecthymatous pustule.

But does artificial inoculation always give rise to this uninterrupted series of phenomena? Are there no circumstances, in which, between the inoculation and the manifestation of the symptoms, there will be a period of rest, of sluggishness, as in the inoculation of the vaccine virus? In the contagion by the ordinary way, does there not always seem to be a time sufficiently long between the action of the cause and the manifestation of the effects?

Yes, without doubt, and these are the cases which can justify and legitimize in some sort the theory of incubation. But when we take the pains to examine these facts with attention, we see that they have been badly appreciated. I shall try to reduce them to their true value, and to bring them back to the laws before established.

I have already said that these cases have never happened to me, in my numerous experiments, always publicly made. This arises evidently from the uniformity of the proceedings which I have employed. My honorable colleague, M. Puche, who has experimented as much as myself, and perhaps still more, has only once or twice seen these accidents manifest themselves, at the second or third day after the puncture. All those who have studied the inoculation of syphilis, know that when it does not succeed immediately, it is because it is negative.

However, we can understand that a too superficial puncture, that the virulent pus placed upon surfaces scarcely denuded, would require a longer time in order to affect the part, and in order that the effects should be produced. Here is what I have observed upon M. Robert de Welz. A first puncture very superficial, which produced no effects the first day, so that there was something which might resemble incubation. But the second puncture, which I made myself upon him, followed the regular course. The partisans of the influence of the general state would answer me, what of that? The first puncture had a slow development, because the organism was not yet impregnated. The effects of the second puncture have been rapid, on the contrary, because then the virus had invaded the

entire economy. That is very well, I shall answer, but here is something which slightly deranges this beautiful theory; it is that M. de Welz had a third puncture made, which being too superficial like the first, has given like that, only tardy results.

Here is the key to incubation, my dear friend. We understand very well, without its help, how in the contagion by the ordinary methods, virulent pus placed upon surfaces more or less denuded, and consequently fitted to receive more or less quickly the virulent action, are affected more or less quickly, and give place to a morbid action more or less rapid. We know, and observation teaches us every day, and the experiments of M. Cullerier demonstrate it in an irrefragable manner, that the virulent pus can remain in contact with healthy surfaces without altering them, and without being altered itself; but we know also that surfaces constantly bathed by the virulent pus, acrid and irritating, excoriating before being specific—we know that these surfaces end by becoming eroded, and by being placed by this pus itself in the conditions necessary to the inoculation taking effect.

This sort of vesication might require a time more or less long to be produced, before the special effects appear, and simulate incubation. For example, some virulent pus is collected in a fold of the vulva, of the vagina, of the prepuce, in the interior of a follicle; it is not till a longer or shorter period after the pus shall have been thus placed, that passing through the successive action that I have just shown, it arrives at the effects of incubation. There is nothing herein which is *plausible*; it is physical and material; it is what the observation *de visu* demonstrates every day to the eyes which know how to see. How many patients there are who think themselves at first only affected by a balano-posthitis, and in whom we see chancres produce themselves, in a longer or shorter time. Add to this the carelessness of patients, the absence of all observation of what concerns them, a circumstance so common in practice, and which causes them to take for *incubation* the time which has passed between the exposure to the cause, and its apparent manifestations. Under these circumstances, you will see for the chancre, as for the bleonorrhagia, the explanation of these pretended incubations of an elasticity of duration so considerable, that they vary between hours, weeks, and even months.

You see that I enter more and more into the substance of these important and grave syphilographic questions. In my next letter I shall treat of the different forms which the chancre can assume.

May your good will, and that of your honored readers, still accompany me. This is for me the most valuable encouragement.—[*Boston Med. and Surg. Journal.*

General Pathology and Treatment of Dyspepsia. By Prof.
J. H. BENNETT.

By dyspepsia (from *δυσπεπτω*, to digest with difficulty,) is generally understood, all those functional derangements of the stomach which are primary in their origin, that is, not dependent upon, or symptomatic of, inflammation or other disease in the economy. Such a disordered condition is exceedingly common, and often constitutes the despair of the physician, arising, as it frequently does, from causes which are often obscure, or, if discovered, are beyond his control. This will become apparent by considering, in the first place, those circumstances which require to be united to secure a healthy digestion. These are—1st. A proper quantity and quality of the ingesta. 2d. Sufficient mastication and insalivation. 3d. Active contractility in the muscular coat of the stomach. 4th. Proper quantity and quality of the gastric, biliary, and pancreatic fluids. 5th. A consecutive and harmonious action of the intestinal canal. Dyspepsia, or indigestion, may be produced by any cause which occasions derangement of one or more of these conditions; and hence why so many different circumstances may produce somewhat similar symptoms, and why so many different remedies have been found effectual in various cases. Notwithstanding that you will frequently meet with instances which baffle all preconceived rules, there can be no doubt that a careful attention to the essential physiological conditions above enumerated will, in the great majority of cases, conduct you to a successful rational treatment. Thus—

1. Of all the causes of dyspepsia, excesses in eating and drinking are the most common. An over-distended stomach, or too rich a meal, not unfrequently induces a feeling of weight or fullness in the epigastrium, nausea and eructation of acid, bilious or gaseous matters, with a loaded tongue, headache, and other general symptoms. This is acute dyspepsia, or the *embarras gastrique* of the French. Occasionally, there is more or less vomiting of bilious matter, when the attack is vulgarly called a *bilious seizure*. If called into such a case, immediately on its occurrence, and before the ingesta have left the stomach, as determined by the sense of load at the epigastrium, an emetic should be given; but if vomiting be present, it should be assisted by warm diluents. As soon as the stomach is quieted, or, if you have been called in at a late period, when the ingesta have passed into the intestines, a purgative pill should be administered, consisting of four grains of calomel, with four of compound extract of colocynth, followed in a few hours by a purgative draught of salts and senna. If necessary, an emetic

may also be given. The purging, with a day or two's confinement to farinaceous food, will generally get rid of such an attack; but their frequent repetition leads to the chronic form of dyspepsia, when careful regulation of the diet, with exercise, must constitute the chief treatment. Hence the advantage of what is called 'change of air,' and much of the benefit which is derived from watering-places. Chronic dyspepsia, however, is far more commonly caused by excess of spirituous and vinous drinks, than by eating, when abandonment of the evil habit is a *sine qua non* in the treatment. Tea-drinkers are very liable to the disease, and its frequency among female servants is probably owing to this cause.

2. It may frequently be noticed, that those who have acquired the habit of eating rapidly are more or less dyspeptic. I knew a journeyman printer who was much tormented with indigestion, but who was cured by changing his residence. The cause of this was for some time a mystery, but on again changing his house the disease returned, although no apparent cause could be discovered. I ascertained, however, that it depended not on the locality *per se*, but on its distance from the printing-house. When far off, he eat his dinner with his family rapidly having only just time enough to walk home and back within the hour. When he lived near, the time otherwise spent in walking was occupied in eating, or in cheerful converse with his wife and family. Since I made this observation, it has often occurred to me that the distant residence of artisans from their place of employment may be the occasional cause of the dyspeptic symptoms they frequently possess. With regard to the exact object of the saliva in the process of digestion, whether it be to convert the farinaceous compounds of the food into glucose, or, by its viscosity, to mix up air with the portions swallowed, is not positively determined; but its necessity for the digestion of man is shown by cases where the under lip has been lost by accident or disease, and where salivary fistulæ have formed, in which dyspepsia is generally present, and in which the disordered digestion has been cured by operations that, by restoring the parts to their normal condition, prevent the escape of saliva. Again, persons habituated to the dirty habit of spitting, are for the most part dyspeptic; and it has been asserted that the pale countenances of the inhabitants of the United States, and the leanness of their persons, are owing to this cause. In all cases where dyspepsia can be traced to this source, the treatment must be obvious.

3. The contractile movements of the stomach, which, by kneading the ingesta, and keeping them in constant motion, secures an intimate admixture with the gastric juice, and the

rapid transference to the duodenum of such portions of it as are transformed into chyme, are evidently of immense importance to a proper performance of digestion. The experiments of physiologists have shown that digestion in gastric juice out of the stomach is much slower than in it, and that section of the pneumogastric nerves, by arresting the contractile movements, only permit the circumference of the mass in contact with the secreting surface to be digested. These facts at once explain the well known influence of mental emotions upon the stomach. Contentment and hope are as favorable, as dissatisfaction and despondency are opposed to, good digestion. Nothing is more common than dyspepsia among literary men who overtask the mental faculties; among young persons of very excitable minds; and among individuals of a melancholy temperament, hypochondriacs, etc., etc. It is in such cases that cheerful society, active and appropriate occupations, change of scene, removal from mercantile or literary employments, different trains of thought, and so on, are beneficial. Hence also many of the good effects of travel, visiting watering places, etc., etc.

4. Our knowledge with regard to the offices performed by the gastric, biliary, and pancreatic juices in digestion has of late years been much advanced. Thus, the gastric juice more especially operates on the albuminous, and the pancreatic juice on the fatty compounds of the food. The function of the bile is perhaps more obscure, although it probably acts as a means of precipitating or separating some of the excretory matters from chyme, and so facilitates assimilation of the nutritive portions. Digestion may be deranged by all those causes which too much increase or diminish the secretion of these three fluids. Thus excess of acidity in the stomach is one of the most common causes of dyspepsia, producing that form of it which accompanies scrofulous and tubercular diseases. It may be in such excess as to neutralize the alkaline action of the pancreatic juice, and render it difficult or impossible to emulsionize fatty matters. In such cases, alkalies, with bitter tonics and the direct introduction of animal oils in excess, are indicated. On the other hand, the gastric juice may be diminished in quantity, as frequently occurs in persons who suddenly overtask the powers of the stomach at feasts, or in old persons with feeble digestion. The sense of load after eating is generally indicative of slow digestion from this cause. In acute cases, a stimulant rouses the stomach to increased action, and hence the moderate use of drams and generous wines after dinner is occasionally useful. In old persons, the sense of load and feebleness is best removed by giving up tea, and drinking at night a little weak brandy and water. In chronic cases, acids are indi-

cated, especially muriatic acid. The tr. ferri co. of the pharmacopœia, is a useful preparation in chlorotic females. We have no distinct means, as far as I am aware, of rousing the pancreas into action, and yet many cases are on record in which fatty matters have passed undigested through the alimentary canal in consequence of obstruction to the pancreatic duct. In such cases, and all those in which fatty matters are difficult to digest, alkalies, especially the liquor potassæ, with vegetable tonics, are indicated. When the bile is deficient, constipation and dyspepsia are usual results, and are to be relieved by gentle mercurial purgatives, with extract of taraxacum, and by remedies such as rhubarb, and especially the compound rhubarb pill, which, by acting on the duodenum, also favour the flow of bile into the upper part of the alimentary canal. Dr. Clay, of Manchester, has recommended in such cases the administration of ox-gall, a remedy, which, although not extensively given, is evidently rational, and calculated by its purgative action to be highly serviceable. Excess of bile, on the other hand, ought to be treated by drastic purgatives, diuretics and diaphoretics, according to circumstances, to cause excess of excretion. Exercise should also be insisted on to call the lungs into action, and thus relieve the liver in its office of separating hydrocarbon.

5. A derangement of the consecutive and harmonious action of the alimentary canal is another frequent cause of dyspepsia; for it is as necessary that those portions of the food which are not assimilable should be removed out of the economy, as that the nutritive materials should be absorbed. Hence, whatever impedes the contractility of the intestinal canal, whatever alters the structure of its mucous membrane, or whatever mechanically obstructs its caliber, may always be observed to induce dyspeptic symptoms. The removal of these various conditions, whether by stimulating the nervous centres, by appropriate diet, or by purgatives and astringents, as they may be required, need not be more particularly dwelt upon.

In many cases of dyspepsia, two or more of these classes of causes may be combined, so as to render the indications for treatment complex and apparently contradictory. In other cases, one or more causes may exist, although from the indications presented they cannot be determined, when our treatment must always be more or less vague and unsatisfactory. Lastly, there are a few instances where dyspepsia can only be explained by *idosyncrasy*, in which we find this or that particular article of diet to derange the digestive functions, and in which avoidance of the offending cause is the only plan that is attended with success.

In addition to the different kinds of dyspepsia to which I

have directed your attention, it is practically important to keep in remembrance the leading symptoms which may be present, and the remedies by which they may be removed. These are anorexia, acid eructations, sense of load in the stomach, cardialgia, vomiting, flatulence, palpitations of the heart, and cephalalgia. Some persons talk of a stomach cough, which, however, is more commonly dependent on irritations in the œsophagus or pharynx, which have hitherto been overlooked. I have already alluded to the mode of treating most of these symptoms. Palpitations of the heart often occasion alarm in young dyspeptic persons, and in addition to remedies directed towards the stomach, change of scene, removing attention from the affected organ, and varied reading should be enjoined. The sense of load in the stomach is most frequently removed, as we have previously said, by acids, whilst acid eructations and cardialgia are best relieved by alkalies and bitter tonics. Vomiting and flatulence are often very troublesome symptoms, and the varied remedies which may be employed in a case of chronic vomiting may be gathered from the following history:—

CASE. *Dyspepsia—Vomiting of Fermented matter, containing Sarcina*.—Thomas Spence, æt. 53, a weaver. Admitted September 6, 1852. He states that, for fourteen or fifteen years past, he has been subject to occasional vomiting, which generally occurred on Sundays, owing, he supposes, to want of exercise at his usual employment. On these days he scarcely ever took his meals from fear of the almost certain vomiting which would follow. For two or three years past he has been liable to frequent heart-burn, water-brash, and acid eructations, but was able to continue at his usual employment till about six months ago. Since then, he has been gradually losing his appetite, and his strength has become much prostrated. He has never vomited blood or any dark-colored matter, and has never passed any such by stool. On admission, tongue clean; no difficulty in deglutition; appetite capricious, but always best in the morning and early part of the day. Shortly after taking food, he begins to have uneasy sensations in the epigastrium, sickness, and a sense of weight at the stomach. When these symptoms appear, the abdomen generally begins to swell, and in about an hour to an hour and a half, the food is frequently vomited. The rejected matters consist generally of the half-digested food, with a thick, dirty, frothy scum on the surface, resembling yeast. He has also frequent pyrosis, acid eructations, and flatulence, the latter sometimes so great as to occasion a sensation of choking, especially after vomiting. These symptoms are worse after some kinds of food than others: oatmeal, especially in the form of porridge, produces them in

the severest form; broths, vegetables, or any kind of slops, do not agree with him; animal food suits him best, but when even this is taken for any length of time, the symptoms soon reappear. The abdomen at present is much swollen, very tense, and tympanitic on percussion, with considerable tenderness over the epigastrium. The bowels are generally constipated; the stools usually of a dark color and hard consistence. He has occasionally slight pain and difficulty in voiding his urine, which is slightly phosphatic. Other functions are normal.

On taking charge of this patient on the 1st of November, I found him vomiting from time to time large quantities of fluid mixed with undigested matters, on which there gathered, after a short time, a thick brownish scum, exactly resembling yeast. On examining this scum with the microscope, it was ascertained to contain a large number of *sarcinæ ventriculi*, mingled with starch corpuscles, more or less broken down, and granular matter. From the ward-books, I learned that this treatment had consisted in the successive administration of—1. The local application of leeches; 2. Of the sulphite of soda, in scruple doses, with two grains of aromatic powder three times a day; 3. Of half a grain of protochloride of mercury at night; 4. Of a scruple of the sulphite of soda every three hours, which was subsequently increased to half a drachm; 5. Of creosote mixture; 6. Of a naphtha mixture; 7. Of bismuth and aromatic powders; and 8. Of pills of calomel and opium. These different kinds of treatment, some of which, especially that of the sulphite of soda, had been continued for several weeks without intermission, seemed to have produced no good effect.—November 11. During the last four days, he has vomited every night, four hours after dinner, that is, about 6 P. M. The ejected matter presents the same yeast-like character formerly described; but the *sarcinæ*, though still abundant, are not so numerous. He complains of a great sense of distension, and a feeling of 'working' or 'bubbling' in the stomach shortly before vomiting. *R.* Acid. hydrocyan. dil. *M.* xviii; Syrup. aurant. \mathfrak{z} j; Aquæ \mathfrak{z} ii; *M.*, half an oz. three times a day.—November 20. The hydrocyanic acid checked the vomiting till last night, when it returned with more violence than ever.—November 24. Vomiting still continues regularly every day. Omittantur mist. acid. hydrocyan. *R.* Liquor potas. \mathfrak{z} ss; Aquæ \mathfrak{z} vss. Two tablepoonsful to be taken every four hours.—December 2. Alkaline mixture again checked the vomiting, which, however, returned last night to a slight degree. Appli- cet vesicat. 4 x 5 Epigastrio.—December 8. Vomiting has once more returned daily since last report. *R.* Tinct. ferri muriat. \mathfrak{z} j. Sumat 3ss *ter in die ex aqua*.—December 16. The

vomiting has been again checked, but once more returned in a slight degree at 1 o'clock this morning. The matter ejected exhibits very little of the usual frothy scum, but consists of a brown liquid like coffee, with a few shreds of undigested food. It is of intensely acid reaction, and contains only a few sarcinæ. The dose of the acid tincture has been reduced to M. xv. The diet during this period has been principally animal, porridge and vegetables invariably increasing his complaint. To-day he left the hospital to visit his friends in the country, expressing himself as greatly relieved.

Commentary.—The kind of chronic vomiting and dyspepsia which is above described has been long known in Scotland, and was described by Cullen as a form of pyrosis. It has been supposed to be associated with the habit of largely consuming oatmeal as a principal part of the diet, although its real pathology was unknown. In 1843, Mr. Goodsir discovered in the ejected matter from the stomach, in a case of this kind, organized forms, which from their resembling a woolpack, he denominated *sarcina*. He considered that they were of a vegetable nature, and by multiplying fissiparously, communicated to the contents of the stomach the appearance of yeast, which is also known to be dependent on the developement and growth of vegetable structures. This occurrence in the stomach of course explains their frequent presence in the feces, although, whether they ever are developed in the intestines is unknown. On one occasion, however, I have seen them in the urine, which occurred in the case of a gentleman under the care of Dr. Mackay, of this city. The sarcinæ vesicæ were in that case uniformly smaller in size than the sarcinæ ventriculi. They have also been discovered in an abscess of the lung by Virchow. The exact mode of formation and origin of these structures are unknown; but little doubt can exist that their presence is the real cause of the chronic vomiting and other symptoms of the individuals affected, and that the cure will depend on such means as are capable of insuring their destruction and preventing their return. It must be obvious, however, that the same means which destroy or check vegetable growth on the surface of the body (see Favus.) are not applicable to the mucous lining of the stomach. Besides, we are ignorant whether these parasites grow in an exudation poured out on the mucous membrane, or are developed only in a fluid. Again, it is very possible that once introduced from without, the conditions necessary for their developement may be dependent on some kinds of ingesta, a view which derives support from the facts observed in the case before us, namely that they were always increased by farinaceous kinds of food. On all these points, however, we are

as yet ignorant, and our efforts at cure hitherto have not so much been directed to cutting off the sources of growth, as to destroying it after it has proceeded to a certain extent. With this view it has been imagined that the sulphite of soda would destroy them, by causing, on its union with the gastric juice, the extrication of sulphurous acid which is so destructive to vegetable life. This remedy has consequently been given, and, it is said, with success; but in the present case it was of no benefit. Subsequently, a variety of medicines were given, several of which succeeded in checking the vomiting for a time. Indeed it was remarked that the mere circumstance of changing the medicine was sufficient to stop the vomiting for several days, when it returned and continued as before. Of all the numerous remedies tried, the tr. ferri muriatis seems to have done most good.—[*Clinical Lectures in Monthly Journal of Medical Science.*

Apparatus for Premature Infants.

Dr. Channing, in an account he is publishing of his visits to various European hospitals, describes an apparatus he saw in use at the Maternite of St. Petersburg, the use of which might be advantageously extended. "It was a species of cradle, without rockers, into which infants are placed when prematurely born, or when imperfectly developed, and for whom a steadier and higher temperature is required than that of the atmosphere. It is made of brass, and is everywhere double; a space being left between the two plates comprising it, with openings, into which warm water may be poured, and others for drawing it off when cool. A soft bed, and a properly arranged canopy, when needed, complete the apparatus."—[*Boston Medical Journal.*

Tartarized Sulphate of Quinine. By Dr. GALAMINI.

Dr. Galamini speaks in strong terms of praise of the febrifuge power of sulphate of quinine when combined with equal parts of tartaric acid—a much smaller quantity of the alkaloid so administered sufficing. During an epidemic of ague, it was given in 43 cases, in 31 of which it speedily effected a cure. In 21 of these, half a scruple sufficed, while in 10 others, it required more continued use. In most of the cases, there was hyperæmia of the brain or bronchial membrane, enlarged spleen, or gastro-biliary derangement, requiring the preliminary employment of bleeding or purgatives. Of the 12 other cases, 5 had relapses; in 3, no effect was produced; and in 4, the above-named irritative symptoms returned.—[*Bull. delle Sc. Med. Brit. and For. Med. Chir. Rev.*

On the Influence of Posture in the Treatment of Epilepsy.

By Dr. MARSHALL HALL.

We have only to raise one hand and arm high above the head, and allow the other to hang down, for a minute or two, and then bring the hands together and compare the syncopal condition of the former with the apoplectic condition of the latter, to form an idea of the influence of posture in the treatment of diseases consisting of affections of the circulation, especially that of the head.

I believe ordinary syncope may pass into fatal sinking if the raised posture be continued.

I believe that simple apoplexy may become deeper and deeper, simply from the opposite course of retaining the patient in the recumbent position.

Sleep, which is a sub-apoplexy, may pass into epilepsy or apoplexy, solely from the fact of a recumbent position. As a preventive of epilepsy and apoplexy during sleep, it is of the utmost moment that the patient should habitually repose with the head and shoulders much raised. For this purpose both bed and mattress should be raised by means of a bed-chair, or triangular cushion, and the patient be prevented from gliding down in the bed by means of a firm bolster, four inches in diameter, placed under the sheet, under the front of the ischia. The trunk should be raised to an angle of 45 or 50 degrees.

In this manner the encephalon will be less oppressed with blood, the sleep will be lighter, this position to epilepsy or apoplexy will be diminished.

This should be the patient's habit during the rest of life.

There are two other circumstances in which attention to posture is most important.

The *first* is the condition of the patient after certain fits of epilepsy, the respiration being impeded by *rattles* in the throat. The posture should be much raised; but, besides this, it should not be such that the saliva may *fall* into the fauces. The stupor and insensibility prevent the patient from swallowing. The saliva, therefore, if a just position be not adopted, accumulates and falls into the fauces, and a throat-rattle and dyspnœa, painful to witness, and dangerous to life, are the consequence. The posture of the patient should be such as to allow the saliva to flow out of the corner of the mouth. In one case such a change of posture relieved the patient immediately.

The *second* case requiring extreme attention to the posture of the patient is that of *Syncopal Epilepsy*, or that form of epilepsy in which there is ghastly pallor of the countenance and other signs of syncopal affection. The patient should be placed

with the head *low*. If this be not done, the syncope may be speedily fatal, an event which actually occurred in an interesting case a few days only ago.

The patient was no other than Ann Ross, on whom Mr. Anderson had performed the operation of tracheotomy. Her fits had changed from those of the *epilepsia laryngea* to the abortive form. The reader may remember that the patient's age was thirty-six; that her case was hereditary, her father having been epileptic; and inveterate, her fits having recurred during twenty-four years; and that she herself was thin and pallid. She was seized with syncopal epilepsy; was laid on the bed, and expected to recover as formerly; was left; and was at length found to have expired. A low position and proper attention might have saved the poor creature's life.

I need scarcely observe, that what I have said of epilepsy applies to many other diseases. It is the *principle* of position which I wish to enforce; a principle, the importance of which I believe to be still greater and still more extensive in application than is generally imagined.—[*London Lancet*.

Of Blood-letting in Pneumonia.

The memoir of Dietl on this subject, published several years ago, excited much attention at the time, and we think it will not be unprofitable to transcribe some of the facts and conclusions contained in a recent report by the same author, inserted in the weekly medical journal of Vienna (*Wein. Med. Voschr*).

This memoir comprises all the cases of primitive and essential pneumonia treated at the great hospital of Vienna from 1847 to 1850, without excepting those aggravated by age, regimen, or analogous circumstances. Cases of secondary pneumonia supervening upon acute or chronic diseases, such as typhus, Bright's disease, etc., are excluded. No patient has been bled in pneumonia in this hospital since 1844; the practice is limited to draughts of gum-water, or ptisans containing opium or an expectorant.

In three years, 750 cases of pneumonia were treated on the expectant plan, viz: 412 men, 338 women. Of these the pneumonia was complicated with acute disease in 140 cases, and with chronic disease in 249. Hyperæmia of the liver and acute intestinal catarrh were the most frequent complications; meningitis was never observed; gangrene of the lung only twice, and pulmonary abscess in a single case. Of 750 patients treated without blood-letting, 681 recovered, viz., 384 men, 297 women; 69 died: 28 men, 41 women—8 died in the stage of red hepatization; 56 in that of infiltration; 5 in that of purulent

infiltration. No fatal case was exempt from complication. The mean duration of the disease was 20 or 21 days. In 515 cases the dyspnœa was very great; it is incontestible that the abstraction of blood diminishes the oppression, but the relief is only temporary. The author admits, however, that when the pneumonia is abandoned to the expectant plan, the embarrassment of the respiration is almost intolerable. This is, he adds, an inconvenience for which the rapid convalescence more than compensates.—[*Archives Gén. Virginia Med. and Sur. Jour.*

Examination of the Human Milk in Legal Medicine.

Mr. Mercer Adam called the attention of the Society to a new and important use of the microscope in legal medicine. He remarked that there were few cases in Medical jurisprudence more difficult to decide than whether, after a few weeks had elapsed, parturition had occurred recently or at a remote period. In such cases of doubt, where delivery is circumstantially believed to have recently occurred, but where all the physical signs may, with equal propriety, be reckoned evidences of this having been at a remote period, he believed that the detection of colostrum corpuscles in the milk would at once decide the question, and, almost with certainty, prove the delivery to have been recent. In illustration of this, he cited the following case, which had recently come under his notice. The body of a newly-born child, much decomposed, was found in a morass in the south of Scotland. It was impossible to decide *secundum artem* whether it had been born alive; but it appeared to have been dead for four or five weeks. Proceedings were taken to discover the mother, and suspicion fell on a young woman who was *supposed* to have been secretly delivered about four or five weeks previously,—that is, about the same date as the infant was thought to have been exposed. On being arrested on the charge of concealed pregnancy, she said she had had a child a year and a half before, which she had nursed till within three months of her apprehension, and firmly denied having recently delivered. The two medical men, who were judicially appointed to examine her, came to different decisions, so equivocal were all the signs, as to the period which had elapsed since her delivery. A microscopic examination of her milk was suggested, and it was found to abound in colostrie globules. This decided the *questo vexata*, and showed parturition to have lately occurred. The girl, on being told that imposture no longer availed, confessed having given birth to a still-born child, thereby confirming the accuracy of the revelations of the microscope. Mr. Adam considered, that in such

cases, the microscope was likely to be as serviceable to the medical jurist as it was in the detection of blood globules, spermatozoa, etc.—[*Western Lancet*.

Obstinate Ulcer of the Leg.—New mode of inducing Cicatrization.

It must have occurred to most of our readers to see cases of old standing ulcers on the leg which it has been easy to induce to cicatrise up to a certain point, but extremely difficult to get completely healed. Thus a sore of large dimensions may, under treatment, contract in the most favorable manner possible until it is reduced to the size of a shilling or a half-penny, when it suddenly becomes most obstinately stationary. In an example of this kind, in which the ulcer had never been healed for twenty years, Mr. Gay has recently adopted, with complete success, the somewhat novel procedure which we are about to describe.

A seaman, named Edward Farrell, aged 36, was admitted, on January 10, 1853, with an irregular sore on the front and inner aspect of his right leg, about the size of the palm of the hand. He stated, that it was twenty years since the ulcer first formed; that he had, in the meantime, been an inmate of nine different hospitals, and subjected, without success, to a great variety of treatment; that he had suffered so much inconvenience from pain, loss of time, etc.; that, regarding the disease as incurable, he now applied in order to have the leg amputated. Mr. Gay advised him to defer resort to such a severe mode of cure, until milder measures had again been tried; and ordered him to be confined to bed, with a lotion, composed of lot. plumbi and laudanum, applied to the diseased part. As his nights were rendered very restless by pain, a pill consisting of pil. sap. cum opio. gr. v., was exhibited nightly four times in succession, after which it ceased to be necessary.

Under this treatment, the ulcer rapidly improved, and, by the middle of February, had become lessened to about the size of a half crown. Cicatrization had thus far been accomplished, in part by the dragging in of the surrounding integument, but chiefly by the formation of new skin. During the next three weeks, no progress was made; the sore remained in precisely the same state; it was, however, painless, and the man appeared to be in very good health. He stated, that it was at this point that the healing process had, on previous occasions, been arrested. Mr. Gay called attention to the circumstance that the healthy skin on each side of the cicatrix was in a state of great tension, and apparently unable further to yield to the

contracting power of the scar. To relieve this condition, he determined to try the effect of an incision through that part of the skin where the tension existed in the greatest degree.

On March 10th this was accordingly done. A longitudinal incision, about three inches long, and deep enough to include the fascia, was made on the inner side of the leg, two inches from the border of the cicatrix.

It was noticed, after the lapse of a few days, that, as if to confirm the propriety of the measure, the new wound gaped most at the part exactly opposite to the unhealed sore, and that the latter had already diminished in size.

April 1.—(Three weeks after the performance of the incision.) The original ulcer is now quite closed by a very sound-looking scar, and of the wound made by the knife there remains unhealed only a mere line of very healthy granulations, which will probably be skinned over in the course of a few days. It is remarkable that the healing of the latter has been partly by the formation of new cuticle, and partly by the approximation of its edges. A stage of contraction will soon no doubt ensue in this cicatrix, but as the traction exercised by it in so doing will be transferred to the back of the leg, where the skin is loose, it may be fairly hoped that the scar of the ulcer will escape injury. Mr. Gay observes, that it is only by thus altering the direction of traction, and transferring it to more healthy and moveable parts, that any advantage can be expected from measures similar to the above. If the conditions under which the artificial sore is placed were precisely similar to those of the original one, it would then share with the latter in the improbability of the accomplishment of cicatrization. But they are not. The incision is made at some distance from the diseased surface, and in healthy parts, yet unstretched, and easily permitting of considerable yielding.—[*London Med. Times.*

Iodine as a preventive of Mammary Abscess. By H. C. STEWART, M. D., of Salisbury, Somerset Co., Pa.

This communication, I presume, will fall under the notice of no physician unacquainted with what is usually termed mammary abscess, both as regards the condition of the breast and the best known means of obviating that distressing condition, to which the "lying-in woman" is so often subjected.

Perhaps no organ of the body is capable of producing a greater amount of suffering to the patient, and vexation to the physician, than the female breast. Situated upon a prominent part of the body—delicately constructed—influencing and being influenced at times, by other organs, it is adapted to the per-

formance of an important function, the disturbance of which must necessarily produce a disagreeable and dangerous result, often requiring the best efforts of the physician to counteract.

Seeing, then, that these things are so, we have been lead to inquire, is there no remedy? or must our patients, after having undergone the agony of parturition, still suffer on, simply because their breasts have not been properly and sufficiently relieved of milk as fast as secreted?

If mammary abscess cannot be *prevented*, it is not because *remedies* have not been *proposed* for it; for amongst all the 'ills that flesh is heir to,' there is none, perhaps, for which such a multitudinous variety of *cures* has been tried. This is probably the best evidence of the difficulty of preventing such an occurrence.

The first indication that suggests itself to the mind of the physician, is to remove the tension by withdrawing the milk. But this cannot always be done: for in how many cases do we find a complete obstruction of the ducts; others, again, where there are no nipples, consequently no outlet for the milk. Have we no remedy here, or must we let the gland inflame, and then bleed and apply leeches and 'poultices to favor suppuration,' and when the abscess forms, open it with a lancet, and run the risk of forming a milk fistula, then apply adhesive strips, and if all this fail—*let it alone*.

In the early part of my practice, I was called to attend a lady, the mother of five children, none of whom she had ever suckled, owing to inversion of the nipples, and consequent obstruction of the ducts. So thorough was this obstruction, that the best efforts of the physicians, on former occasions, had totally failed to relieve the breasts of a particle of milk; consequently the woman had suffered on every occasion from mammary abscess.

In giving me a history of the treatment at different times, she said that at one time she came near losing both breasts; when the physician, (dead at the time of this conversation,) as a last remedy, applied something which, from the description given me, I believed to be iodine. Knowing the efficacy of that article in all glandular affections, I resolved to try it as soon as the breasts showed any signs of inflammation. On the third day, finding them large, heavy and intensely painful, I made an application to the breasts of iodine ointment spread upon linen, which gave almost immediate relief. After a few applications, I found the breasts "perfectly flaccid, completely cool, and admitting of the freest palpation and handling, without the woman making any complaint." From the favorable result in the above case, I was induced to try it in two similar cases, with

the same success, and so far as I know it has never failed in the hands of any of my medical friends to whom I have recommended it; but not a few there are who can bear testimony to its virtues.

With these few suggestions, I respectfully submit it to the profession, hoping that it may not disappoint their expectations.
[*Medical Examiner.*

A new mode of Reducing Dislocation of the Thumb. By JOHN DOE, M. D., of Cabot, Vermont.

"In this dislocation, the phalangeal end of the metacarpus projects into the palmar surface of the hand, forcing itself between and through the flexor muscles of the thumb, which form a loop around the head of the bone. Extension made upon the thumb makes this loop more tense; and, as the metacarpal end of the first phalanx is broad and considerably flattened on its palmar aspect, it must be apparent at once that the difficulty of reduction is directly as the amount of extension. There is good reason to believe that extension would never succeed in these cases without rotation. The principal indication in treatment here, is to relax the flexors forming the loop, so that the end of the phalanx can be pushed forward into the loop, and by carrying the muscles forward with it disengage the head of the metacarpus. This can be done in the following manner:—

Having previously warmed the hand, if cold, in warm water, the surgeon should seat himself by the patient, facing in the same direction, and upon the same side with the injured thumb, and place the hand upon his knee. Tip back the thumb upon the dorsum of the metacarpus to more than a right angle, or so as to form a slightly acute angle with the latter bone; place both index fingers against the ball of the thumb, and the ends of both thumbs against the dorsum of the disarticulated end of the phalanx; now by pushing forward forcibly, yet steadily against the phalangeal bone with both thumbs. Reduction will generally be effected on the first trial, and almost instantly."—[*Western Lancet.*

Erysipelas.

Collodion appears to be now preferred by many, over every other local application, but in some cases its contraction causes a painful degree of pressure upon the inflamed parts. To obviate this, it has been advised to mix two parts castor oil to thirty of collodion, which gives it the requisite softness and

elasticity. The local applications used in this disease are numerous, and each different one that has been suggested has its advocates. Probably the good effects of all, are mainly due to a common quality—that of shielding the surface from the contact of the atmosphere. The collodion answers this purpose very effectually, but solutions of gutta percha and shellac scarcely less so. A coating of flour, or powdered starch, as in the treatment of burns, affords sufficient protection in many cases, which is increased by first washing the skin with a solution of glycerine. The white of an egg is scarcely inferior to any of these. Of the stimulating applications we have found Lugol's Solution of Iodine the most useful. To prevent the spreading of the disease, it should be extended for a small space upon the sound skin. The constitutional treatment of erysipelas is not less varied than the local, depending mainly upon the variable character of the disease in its different stages, and at different times and places. It is treated both as a sthenic and an asthenic disease. In this country, and particularly in the South, the former character is more likely to prevail, requiring depletion and sedatives; while in Europe, and particularly in the large cities, the disease is said to assume, in general, the asthenic character, indicating the use of tonic remedies. The Bells of Edinburgh recommended the chalybeate treatment, and they are sustained by Ranking, who succeeded with the muriated tincture of iron, in doses of 36 drops, after wine, ammonia, quinine, &c., had failed. He states it as his invariable practice in this disease, to lose no time in sustaining the powers of the patient by wine, beef-tea, and ammonia; the local applications being nitrate of silver and flour. In this region it is a rare thing, we apprehend, to administer tonics in the early stage of the disease.—[*Ibid.*]

Observations on the use of Potash in the Treatment of Scurvy; with Cases. By WILLIAM A. HAMMOND, M. D., Assistant Surg. U. S. Army.

Hitherto, in the treatment of scurvy, little reliance has been placed by physicians in medicines, strictly so called. Fresh vegetables, lime-juice, &c., have been regarded as indispensable to the cure of this disease; and in situations where these articles could not be obtained, the unfortunate patient has generally lingered out a miserable existence till relieved from his sufferings by death.

The researches of animal chemistry have, at length, however, thrown some light upon the pathology of scurvy; and if future observers confirm the results derived from the following

cases, it will not be the least boon which that science has conferred upon the practice of medicine.

New Mexico, where the following cases occurred, presents to the physician a wide field for the observation of scurvy. During eight months of the year, it is exceedingly prevalent among the troops stationed in this territory, and from the scant vegetation of the country, it is impossible to obtain those vegetables generally esteemed most beneficial in the treatment of the disease under consideration. Perhaps, however, I saw less of scurvy than any medical officer stationed in New Mexico. There did not *originate* among the troops serving at the post with me more than four or five cases, out of twelve admitted into the hospital, and those were generally of a mild nature. This immunity from the disease I ascribe to the fact, that the small stream which flows past the post (the water of which was used by the troops) contains potash in quite an appreciable quantity. From one of the springs situated among the mountains, which supply the main stream, I obtained very satisfactory evidence of the existence of potash in considerable abundance, a large precipitate of the bi-tartrate being thrown down by a solution of tartaric acid.

A small portion of some salt of potash (say the bi-tartrate, as the most agreeable), issued to the troops as a component part of the ration, would, I am confident, entirely prevent the occurrence of this affection among them.

The cases here presented are selected from about a dozen others, as being those in which the disease was farther advanced before being placed under treatment.

CASE I.—William Henry, a private in company "K," 2d U. S. dragoons, admitted in hospital at Cebolleta, New Mexico, January 2, 1850, with scorbutus. Has been on detached service to Santa Fé, and while there became affected. Countenance sallow and bloated, breath extremely fetid, and gums exceedingly tender, bleeding profusely upon the slightest touch. Legs and thighs covered with the characteristic livid spots; stiffness of the knee and ankle-joints, and slight œdema of the feet. Ordered, potass. carb. grs. v., ter in die, and sol. aluminis, for mouth.

January 3. Much improved; continued medicines, 4th, 5th, 6th, and 7th; continued to improve; ordered medicines as before.

8th. Gums hard and firm; maculæ on legs and thighs entirely gone; complexion clear, and general health restored. Omit medicine.

9th. Discharged hospital, cured.

CASE II.—Augustus Miller, private 2d dragoons. Admitted into hospital May 26, 1850. Has been affected with scurvy for three weeks, at Albuquerque, New Mexico, and has been under treatment for it. Does not know what was given him. Transferred with his company to this post May 25, 1850. Says he feels a little better since his arrival here. Has some pain in right leg, at seat of a fracture which occurred some years since. Maculæ on legs and thighs very abundant, and two or three large extravasations of blood about the ankle joints of each leg. Bowels regular. Ordered potass. carb. grs. v, ter in die.

28th. Spots on lower extremities disappearing; skin over fracture livid in appearance, and slight motion can be perceived at the fractured part. Continued medicine as above.

29th. Improving. Pain in leg much less. Motion of fractured parts slightly perceptible. Extravasation fading. Gums firm, and healthy in appearance. Committed some indiscretion in his diet to-day, in consequence of which he became affected with dysentery, which required very active treatment. Potass. carb. was omitted on the 31st, and on June 1st all signs of scurvy had disappeared. He is much debilitated from the effect of the dysentery, which still continues.

June 7. Convalescent. Continued to gain strength, and on the 12th was discharged from hospital, and returned to duty.

CASE III.—Rafael Garcia, a Mexican, has been affected with scurvy for a month or more at Berlin, New Mexico, where he has been on a visit to his friends. First noticed the disease at that place. Returned to this village June 5, 1850. I first saw him on the 7th. Gums exceedingly tender and spongy; teeth very loose, and sore to the touch. Has several large extravasations of blood on his legs, and myriads of the characteristic spots. On the anterior surface of the left leg is an ulcer three inches in diameter, of a pale, indolent appearance; knees stiff, flexed on the thigh, and very painful on any attempt to extend them. Ordered potass. bi-tart. $\bar{3}j$, ter in die. This medicine was continued for two weeks, at the end of which time all signs of scurvy had disappeared, with the exception of the stiffness of the knees. By friction and careful passive motion for a week, the free use of these joints was completely restored. The ulcer of the leg healed perfectly without the use of any local application but ung. simp.

These cases are, I conceive, sufficient to prove the benefit to be derived from the use of the salts of potash in the treatment of scurvy. Others could be adduced; but as they are general-

ly similar to the above in all essential particulars, I have not thought it necessary to introduce them here. In regard to the most eligible combination of potash, I am inclined to think the bitartrate preferable. It is more generally relished by patients, and, independently of this advantage, I think its purgative and diuretic properties beneficial. Both this and the carbonate, however, becoming exhausted at the post, I administered several other salts of the same article, both separately and with citric acid, without, however, perceiving that the latter article at all accelerated the cure.

No vegetables were given to the patients, for the simple reason that they could not be procured. They certainly would have proved powerful adjuncts to the potash in the treatment of the disease; but from the arid nature of the soil of the country, it is impossible to obtain them, even when in season, in sufficient quantities for the use of the sick alone.

In situations where the water has not a chemical constitution resembling that of Cebolleta, I should think it expedient to increase the quantity of potash given at each dose considerably over that indicated by the above cases. I did not, however, find it necessary to administer to any over two drachms of the bicarbonate thrice daily, and this quantity was given but to one patient.

In all, twelve cases of scurvy were treated with the salts of potash. Of these, four originated at the post, the remainder in different parts of the territory. All recovered perfectly, and in no case did lameness or other deformity remain. The minimum period of treatment was three days, the maximum twenty-one days, the average nine days and six-tenths.

In regard to the *modus operandi* of potash in scurvy, I think the hypothesis of Dr. Garrod, of London (to whom I am indebted for the idea of its curative properties), correct. He regards the disease as depending upon a deficiency of potash in the blood, and considers it essential to the perfect nutrition of the muscular fibre.* The urine of each patient whose case is here related, as also that of all in whom it was examined, exhibited, before treatment, a total absence of potash, and I noticed that, as this substance appeared in the urine, amendment commenced.

The action of this remedy must be seen, in order to be properly appreciated. The return of strength to the body, cheerfulness to the mind, and the speedy disappearance of all symptoms of the disease which ensue so quickly upon its administration, liken its influence more to that of a charm than aught else.

* See American Journal of the Medical Sciences, July, 1848, pp. 200-203.

The importance of the discovery of the curative properties of potash in the treatment of scurvy cannot be too highly estimated. In our own country, especially, will it prove a most valuable acquisition. Scurvy has been the scourge of the numerous parties of overland emigrants to Oregon and California; more, probably, having been affected among them by this disease than all others combined, and numbers have, in consequence, left their bones upon the plains. From the facts previously mentioned in this article, I am satisfied that not only as a *remedy* is potash valuable in scurvy, but that as a *prophylactic*, also, its qualities entitle it to a high rank among that class of agents. On the score of economy, a better one could not be obtained.

The attention of physicians is invited to this subject. It is only by numerous experiments that the efficacy of this article can be established on a firm basis, and if these are made, they will, I think, have as much reason to be satisfied with its action as myself.—[*American Jour. of the Med. Sciences.*]

Enlarged Spleen—Suppuration—Evacuation of the Pus through the abdominal walls—Recovery. By M.M. PALLER, M. D.

The following case was read before the St. Louis Medical Society, on the 18th of October last.

A girl aged twenty, resided during the month of September, in a little town on the Illinois river, where she had frequent attacks of Intermittent Fever. On the 18th of October, I saw her, when she was free from the Intermittent, but had an enlarged spleen. It extended into the epigastric and umbilical regions as far as the median line, and inferiorly into the left iliac region. I ordered for her a combination of laxatives and tonics. A few days afterwards, she complained of acute pain over the tumor—for this, a blister was prescribed. It failing to give relief, another was put on—the pain remained unabated—so great was it, that she was deprived of sleep at night.

On the 6th November, the integuments over the tumor were red and a little œdematous—there was also obscure fluctuation. Dr. Pope who saw the case with me, suggested the application of poultices until fluctuation was more manifest. On the 9th, he passed an abscess lancet into the prominent part of the swelling, and about a pint of sanguineo-purulent matter was evacuated. It continued to discharge freely for twenty-four hours, and in the course of a week, the opening healed up. The circumference of the enlarged spleen was not diminished, but at the place where the abscess was, there is a deep cup-like depression whose diameter is about four inches.

Suppuration of the spleen is a rare termination, yet, there have been some examples of it. Such a termination is spoken of by Cruveilhier and Andral—the quantity of pus which is collected may vary from a few ounces to many pounds. Dr. Abercrombie cites a case, where the spleen contained thirty pounds of matter. In another case, eight pounds of matter were drawn off by tapping. ‘The patient died the next day, and the spleen was found still to contain seven pounds of matter, and to form a sore eighteen inches long and twelve inches in diameter.’ In a case which fell under his own observation, inspection after death, showed that the ‘spleen was enlarged, and in the centre of it, there was an irregular cavity, containing several ounces of pus.

The matter sometimes makes its way through the diaphragm and left lung. Dr. Tweedie records a case of splenic abscess during fever, in which it had nearly perforated the contiguous part of the diaphragm. It sometimes finds its way through the stomach, as in the case reported by M. Coze. The patient had vomiting of blood, mixed with purulent matter. Post-mortem examination showed, that the spleen adhered intimately to the stomach, and formed a bag full of purulent matter, and clots of blood. The parietes of it were in general about six lines in thickness; and it communicated by a free opening with the cavity of the stomach, at the place of adhesion. Sometimes the pus is discharged through the colon, as in a case mentioned by M. Jacquinelle—or, through the abdominal walls, as reported by Drs. Grotanelli and Raikem, and in the case now brought before the Society, adhesions in these cases taking place between the spleen and the parts through which the pus is discharged.—[*St. Louis Med. and Surg. Journal.*

On Matico in Diarrhœa. By M. MODONI.

M. Modoni states that he has employed this substance in about 120 cases, and usually with good and speedy effect, in various species of intestinal discharges, and especially in atonic diarrhœa. It is given at the Bologna Hospital in doses of from 18 grs. to ʒj. per diem, no inconvenience, save a little nausea or diarrhœa, ever being caused, and this being capable of removal, either by diminishing the dose or temporarily suspending the remedy. In the most favorable cases, an impression is made upon the disease by three or four doses; and it is quite relieved in from three to six days. The medicine should, however, be continued awhile longer in order to prevent relapse. In those diarrhœas which owing to the existence of visceral changes, or in the presence of a general dyscrasis, the return

of the diarrhœa is inevitable, the matico is still an invaluable remedy, owing to the rapidity of its astringent action, which enables us to suspend the exhausting discharges, and obtain time for the employment of any other means which the nature of the case may indicate.—[*Brit. and For. Med. Chir. Review*, from *Bulletino delle Scienze Med.*

On Pagliari's Hæmostatic. By M. SEDILLOT.

M. Pagliari, a pharmacien at Rome, professes to have discovered a styptic liquor of great power; and several of the officers of the French army have testified to its efficacy. M. Sedillot has also, on several occasions, brought forward cases in corroboration; and in the present paper he adduces additional ones, in some of which considerable vessels, although not those of the first class, furnished the blood. He says that it has been objected, that compression is employed by means of bandages and charpie; but this is merely to prevent the coagula which form being removed from the mouths of the vessel; and it has only to be continued for twenty-four or forty-eight hours. So little plastic is human blood, that compression alone, unaided by styptics, would have to be so prolonged and forcible, that it would risk the formation of ulcers or gangrene in the parts to which it was applied.

M. Pagliari has now revealed the composition which is as follows: Eight ounces of tincture of benzoin, one pound of alum, and ten pounds of water are boiled together for six hours in a glazed earthen vessel, the vaporised water being constantly replaced by hot water, so as not to interrupt the ebullition, and the resinous mass kept stirred round. The fluid is then filtered, and kept in stoppered bottles. It is limped, slightly styptic in taste, aromatic in odor, and the color of champagne. M. Hepp, of Strasburg, has substituted white resin for the benzoin. Every drop of this fluid poured into a glass containing human blood, produces an instantaneous magma; and by increasing the proportion of the styptic to the quantity of the blood, a dense, homogeneous, blackish mass results.

Many are the circumstances in which the surgeon may not be able to have resource to the ligature, as in the case of friable arteries, secondary hæmorrhage from deep-seated, painful or inflamed wounds, the impossibility of seizing the artery, or where the hæmorrhage results from numerous arterioles, which are too small or retracted, or from veins and capillary vessels. In all cases, in fact, where compression is now usually employed, without much benefit being expected to result from it, and often, indeed, proving useless or dangerous, this fluid seems indicated.—[*Ib.* from *Bull. de Ther.*

On Irritable Bladder in Children. By CHRISTOPHER FLEMING, M. D.

Irritable bladder occurs much more frequently in young children than would at first sight appear, and this, where the irritability is not the consequence either of inflammation or of organic disease, although occasionally attributable to some abnormal defect. The mother or nurse of the child so affected states, that the child is constantly applying the hand to the organs engaged; that it appears to suffer pain during micturition; that the act is frequent; that it is urgent, but when the urine has passed off the child appears relieved; that often, if the urine falls on the floor or clothes, it rapidly becomes muddy and whitish, and it is even stated by some, that it is so at the moment of being passed; that when the child sits down for such purpose, it has an inclination to remain longer than is requisite, and, in some cases, that there is a disposition to prolapsus of the rectum, from the forcing and straining attendant, and very frequently a discharge of bloody mucus from the rectum takes place; that these symptoms have continued for some time, notwithstanding the exhibition of medicines to regulate the bowels and produce other ordinary effects; that the child is losing strength and wasting in flesh; that the appetite is most precarious, and that there is a great desire for drink; that the quantity of urine passed is very variable, sometimes deficient; that its quality is equally changeable, at times being pale, at others deep in color, and again clear and often muddy, and with copious sediment.

If accurate inquiry is now instituted, it will be found, that many such children are born of gouty parents, or of persons much subject to dyspepsia, and that they are children whose diet and habits of life are irregular; and in the humble walks of life that they are, in addition, irregularly clad and irregularly cleansed: such are by no means of unfrequent occurrence. The quantitative and the qualitative analysis of the urine satisfactorily explains the symptoms; and attention to the physical condition of this secretion, to its chemical constitution, and to the appearance of the deposits—particularly the deposits of rest—assisted by the microscopic characters of the latter, point out the curative indications which are suitable to each variety of case. It is beyond all manner of doubt, that, as in adult age, many practical hints can be taken from attention to the general constitution of the urine in the surgical diseases of these organs, and that in the child the normal essential constituents of this important fluid may be increased or diminished, and that abnormal ingredients may be superadded. We have here the

lithic, the oxalic, and the phosphatic diatheses, and each has its special influence. In fact, with few exceptions,—and the presence of sugar in the urine of children is one worthy of note,—there is no derangement of the urine found in the adult which I have not also found in the child, in its most exaggerated form, both as regards the disproportion between its normal constituents, and the introduction of abnormal substances. I feel perfectly satisfied that attention to these details, as subsidiary means, will be found of value in the diagnosis and treatment of many of the diseases of children, and especially in those cases of cerebral complication which so repeatedly puzzle the practitioner, and where the quantity of this secretion, on the one hand, is materially diminished, or, on the other, increased.

With a view to the practical study of the morbid condition of the urine, as auxiliary to the diagnosis and treatment of diseases of the urinary organs in the child, I find it difficult to condense my remarks so as to avoid the introduction of any irrelevant matter, and at the same time to escape the charge of an attempt to undervalue those more minute particulars to which, justly, much importance is attached. In my lectures on these subjects, I have been in the habit of directing attention to the color and smell of the particular specimen under examination, to its chemical reaction, and to its density; and I have always attached very great importance to the *deposit rest, as to shade and outline*, and to the transparency or otherwise of the supernatant fluid. From an analysis of these several leading features of urine I have derived the greatest advantage, and even, in the absence of any microscopic examination, have been enabled to decide in very many cases, with sufficient certainty for practical purposes, on the peculiar nature of the deposit. Of course in some, particularly the “non-crystalline organic deposit,” the assistance of the microscope is often indispensable; but in children they do not constitute the majority of the cases met with. As attendant on the “irritable bladder,” I would say that, according to the classification of urinary deposits by Golding Bird, those of uric acid, and the urate of ammonia, and of oxalate of lime, are particularly frequent in occurrence; and that, next in order of frequency are conjointly or separately with these “the non-crystalline organised products,” such as blood, pus, occasionally mucus, but very often indeed vibriones. It would be too great an occupation of space and of time to enumerate the many cases I have witnessed, as illustrative of these statements; they are of almost daily occurrence. I do not deny that phosphatic deposits are to be met with, but these deposits do not occur, under ordinary circumstances, as a substantive deposit in the urine of children.

The prisms of the neutral triple phosphate are to be seen conjointly with the crystalline deposits above specified, just as in adults. but it is very rare indeed to meet with them as solitary deposits, although so frequent in advanced life; and it is equally rare to find them combined with that physical and chemical condition of urine almost necessarily present under such circumstances. Indeed I find it difficult to bring to my recollection—unless under the most aggravated form of vesical and renal disease, complicated with phosphatic calculus in the child—that excessive secretion from the mucous membrane of the bladder which takes so very prominent a part in the formation of such deposit in advanced life. Amongst the numerous cases of urinary disease I have witnessed in the child, such is excessively rare, and it is equally rare as a symptom of calculus in the child. There was in one case a source of deception, which was by the merest chance unfolded to me, and which, perhaps, may be noted as confirmatory of Sir Benjamin Brodie's opinion as to the special source of those phosphatic salts in the urine.

In May, 1852, a boy, aged three years, was brought to my study by his mother, in great alarm from the suffering the child had endured for some weeks in passing urine. There was frequency and urgency, and so much forcing and straining as to produce distressing prolapsus ani. The urine was largely loaded with lithates, and contained a remarkably tenacious mucous deposit, deeply colored with blood, and adherent to the glass. I found in it numerous blood discs and large crystals of the triple phosphate. Symptoms not improving, I felt justified in sounding the child, which I did with a silver catheter, and whilst the urine was escaping, a severe paroxysm, resembling a fit of the stone, occurred, during which a considerable quantity of gelatinous mucus escaped from the rectum. I collected the urine drawn off through the catheter in one test-glass, and the discharge from the rectum in the second. The urine was acid, the latter alkaline; the former was loaded with lithates interspersed with some crystals of oxalate of lime, the latter, in addition to mucus, blood globules, and epithelial scales, was studded with large, distinct, triple, phosphatic prisms. I merely mention these details incidentally, as interesting and not unimportant phenomena, and particularly in connection with the statement of Sir Benjamin Brodie already alluded to. They were to me then novel, and I took the opportunity of showing the microscopic appearances and the specimens to my colleague, Dr. Hutton. I have since been enabled to confirm them.

I have said that the lithic acid and lithate of ammonia depos-

its, and also that of the oxalate of lime, are the most frequently met with in children, and they will be found to be productive of most decidedly distressing urinary symptoms. I have often found these deposits present conjointly; very often the oxalate of lime and the urate of ammonia, the latter cloaking the former, unless carefully looked for. I have found the red sand, as the lithic acid is sometimes termed, in the child, but it is far more frequent to find the colorless, or nearly colorless, crystals of lithic acid, and all are met with of every variety of shape and form, and they are to be seen in the children of the poor as well as of the rich; and really it does not appear that diet very materially influences their presence or their character. I have a boy, aged about seven, now in hospital, with suspected calculus in the bladder, and in him numerous crystals of pale lithic acid exist in combination with oxalate of lime; whilst in another ward in the same hospital, there is a boy, somewhat about the same age, a patient of Dr. Hutton, in whom the red sand is visible to the naked eye in the test-glass, floating through the urine, and under the microscope the deep orange crystals are to be seen distinct, and in large aggregated masses. It is in this class of deposits, and in that of the oxalate of lime, that the surgeon requires to be more circumspect, as the physical and chemical characters of the urine are often not remarkable. The color is in such cases often pale; the density very low, so low as 1007 to 1010; the deposit a mere tomentous, semitransparent cloud, but one which will not escape detection by the practised eye. The suffering experienced in some cases of this nature is really very great; and if the child happens to have a long narrow prepuce, or an abnormal opening in the urethra, he may be put to unnecessary torture from inattention to the morbid state of the urine. I have known such cases: one, a fine child, the only son of a fond father, who lived freely, and thought the child could not do better; the other, a boy aged three years, with hypospadias, from which the child had not previously experienced any visible inconvenience. In each the usual traces of symptoms of irritable bladder existed; in each, the ordinary clear condition of urine diverted attention from the examination of its actual state; and in each, its normal restoration caused the subsidence of all annoyance. I do not dwell on the lithate of ammonia deposit; its characteristic appearance is too obvious to require any comment. I shall merely add, in conclusion, as regards it and the other crystalline deposits noted, that when from their continued presence they produce local or general irritation, they demand the watchful care of the practitioner to put rigidly in force those dietetic and therapeutic means which are laid down in systematic

treatises on the subject; and if the symptoms do not yield to such treatment, he should search for some local cause either in the bladder or in some portion of the organs implicated; and he should also bear in mind, that such character of urine is the most likely to lead to the formation of stone in the bladder, as proved by its composition in the child, and by the fact of its being the most common attendant on its presence when found there; and that hence two practical lessons should not be lost sight of, namely, to alter, as quickly as can be effected, this morbid condition of urine, and to suspect the presence of calculus, should it be obstinate.—[*Dublin Quarterly Journal*.

Excision of the Inferior Maxillary Bone for Caries. By
W. G. BULLOCH, M. D., of Savannah, Geo.

Towards the end of July, 1852, John Turner, an Irishman, entered the Savannah Hospital, and applied to me for relief under the following circumstances.

The right side of the lower jaw, from the symphysis to the neighborhood of the articulation, was diseased, and so extensively enlarged from infiltration of the soft parts as to produce great swelling and deformity of the face, and to impede much the movements of mastication. It also rendered him an object of disgust to himself and others, in consequence of the sanious and excessively fetid discharge which flowed abundantly from several fistulous orifices opening internally in the mouth, and externally on the cheek. A probe introduced into these fistulæ, discovered the bone not only denuded, but so perforated and broken up, that the instrument could readily be passed into the mouth in various directions.

He was rather obtuse in intellect, and therefore I could not obtain any very satisfactory information from him of the nature or history of the disease, except that he stated, to use his own language, "he got it hurted some year or two ago."

From the nature of the case, it was evident that entire removal of the diseased portion of bone was the only remedy, and several of my professional friends who saw the case with me, were decided in their opinion as to the necessity of such an operation, other modes of treatment offering no hope or prospect of cure in our opinion.

An operation being determined upon, with the assistance of Drs. Kollock, Howard, and Warner, after administering a mixture of chloroform and ether, and bringing the patient completely under the anæsthetic influence of those agents, I proceeded to perform the operation in the following manner:

My first incision was made a line or two to the left of the

middle of the lower lip, by transfixing it with the knife directed obliquely upwards and backwards, then reversing the cutting edge of the knife, and continuing the incision down to the lower margin of the jaw. From the termination of that incision I boldly drew the scalpel along the entire inferior margin of the bone as well as it could be defined in the swollen state of the parts, to the angle of the jaw, turning up behind that point, and extending another incision at right angles or so with that, to a point nearly opposite the articulation. The formidable flap made by these incisions was next dissected up, and reflected upon the upper part of the face, so as to expose fully the diseased portion of bone. The jaw was then sawed through with the chain saw immediately to the left side of the symphysis. Seizing the end thus sawed through, the section of bone was then carefully separated from its internal soft attachments, by drawing the scalpel along its internal surface with the edge close upon the bone. Considerable difficulty was experienced in detaching the soft parts about the angle of the jaw, and the advantage to be derived in using it as a lever was lost by its giving way and separating from the ramus and processes above. These had to be seized singly with a forceps, and separated by drawing them out and relieving them from their attachments above with a knife passed carefully under the arch of the malar bone. After excising portions of the diseased soft parts connected with the bone, tying the vessels cut, of which there was but one of importance, viz., the facial, and suppressing the hemorrhage, otherwise inconsiderable in this case, the flap was brought down, and the edges of the wound were accurately adjusted by means of a few hair-lip pins and adhesive plaster, and thus kept in complete apposition. Water dressing was applied, and such other treatment adopted as circumstances required to facilitate union and healing of the wound.

He is now, at the time of writing this description, a month after the operation, going about, cheerful, and almost entirely well—union so completely established as to leave little external deformity, and exhibiting but slight traces of so extensive an operation.—[*American Jour. of the Med. Sciences.*

Senile Gangrene.

Not many years ago, one of the most painfully discouraging subjects of surgical study or practice was that mortification which is so apt to attack the toes of old people, and has been called the *gangrena senilis*. The insidious commencement of its attack; the agonizing distress which attends its progress; the inefficiency of measures employed for its remedy, and the

inevitably fatal result to which it led, rendered this disease indeed one of the *opprobria* of surgery. According to the idea then entertained, nothing could be done, except keeping up the patient's strength by an ample allowance of wine and brandy, dulling his sensibility with opium, and preparing his friends for the issue. But now the case is very different. The morbid action being attributed to excessive irritability, dependent upon weakness proceeding from an imperfect supply of blood, instead of attempting to supply the deficient vigour by stimulants that can do no good, and must hurry on the disease, the treatment is directed with a view to lessening the tendency to over-action by the use of soothing means. The patient is confined to bed, debarred from animal food in every form, with the exception of milk, which, with water, constitutes his only drink, supplied with doses of morphia in proportion to his pain and restlessness, and has the part affected covered with a linseed poultice. Under this system, the distressing symptoms gradually disappear, the slough ceases to extend, and after its separation the sore cicatrizes soundly, no difficulty or bad effects being subsequently experienced in returning to the ordinary habits of diet and exercise. The student, therefore, no longer turns over this page of his principles with a blush for the imperfection of surgery; and the practitioner, instead of being called upon to undertake a hopeless task, has frequently the satisfaction of conducting his patient safely through what might formerly have well been called the valley of the shadow of death.—[*Monthly Journal of Med. Science.*

Coagulation of Blood in Arteries by means of a Solution of Perchloride of Iron. Aneurism of the Suborbital Artery cured by this means.

M. Pravaz, of Lyons, has instituted some experiments with a view to obtaining instantaneous coagulation of blood in arteries. He makes use of perchloride of iron at the maximum of concentration, a few drops of which are to be injected into the vessel in which it is desirable to obtain coagulation. This injection is to be made with a very fine trocar, of either gold or platinum, the diameter of which is scarcely greater than that of a needle; this is to be introduced very obliquely, through the parietes of the artery, with a wriggling motion. To this trocar is adjusted a small syringe, the piston of which works with a rack and pinion motion, so that the injection proceeds without impulse, drop by drop, and in such a way the quantity of liquid injected can be accurately measured. Every turn of the pinion allows of the escape of two drops of the liquid.

Whilst this injection is used, the circulation is momentarily arrested in the vessel by means of pressure above and below. A few drops suffice (three or four in a sheep, six or eight in the horse) to form a solid and resisting clot.

Up to this time, the experiments had been only made on animals, but M. Raoul Deslongchamps has just sent before the Surgical Society of Paris. a case of aneurism of the suborbital artery treated with success by injection of perchloride of iron. It was a tumour situated in the suborbital region, affording pretty strong pulsations, isochronous with the heart's action. This tumour, for the cure of which compression had been used without effect, disappeared completely by means of the injection, as recommended by M. Pravaz, after presenting some inflammatory symptoms, which easily yielded to antiphlogistic treatment.—[*Dublin Med. Press*, from *Gaz. Méd. de Toulouse* and *Presse Méd. Belge*.

Anæsthetic Properties of the Lycoperdon Proteus—Common Puff-Ball.

The number of the Medical Times and Gazette, for June 11, just received, contains an abstract of a paper read before the Medical Society of London, on the anæsthetic properties of the *Lycoperdon proteus*. The author's attention had been directed to the fact, that the smoke of the common puff-ball was used in the country for stupefying bees, and the idea struck him, that it would be worth while to ascertain if the same agent would produce narcotism in higher classes of animals. Several weeks since, he commenced a series of experiments with the fumes of the fungus, and had continued them to the present time. He found it possible to produce the most perfect anæsthesia with the fumes. His experiments had been made on dogs, cats, and rabbits, and had been witnessed by Drs. Wills, Crisp, Cormack, Snow, and several others. He had administered the narcotic fumes in the impure state, and in a clarified state obtained by passing them through a solution of caustic potass. When an animal was exposed to a large quantity of the narcotic vapour, the narcotism came on very speedily, and the insensibility was most decided, but recovery soon took place. Dr. Willis and Mr. Richardson had removed a large tumour from the abdomen of a dog that had been placed under the influence of the narcotic. No sign of pain was shown during the operation, and the animal did well afterwards. The fumes were obtained by burning the fungus. When a moderate quantity was inhaled slowly, the narcotism came on and passed off slowly, the animal exhibiting all the symptoms of

intoxication, with convulsions, and sometimes vomiting. Several animals had been intentionally destroyed by the narcotic. It destroyed life slowly; a dog would often inhale the fumes for twenty minutes or half an hour, after being completely narcotized, previous to expiring. The heart's beat in all cases survived the respirations. The lungs after death were pale; there was no sign of congestion in any organ; the blood retained its red colour, but did not coagulate quickly; cadaveric rigidity set in in two or three hours. During recovery from a protracted narcotism, an animal would sometimes be quite conscious, but insensible to pain. Mr. Richardson had himself inhaled the clarified fumes of the fungus; they produced in him symptoms of intoxication and drowsiness, but he did not breathe them long enough to become completely narcotized. Mr. Richardson was able to afford but little information as to the nature of the narcotic agent contained in the fumes. Many of the fungi possessed narcotic properties, and had been supposed to possess an alkaloid resembling morphia; but the subject had never been thoroughly investigated. He should only say, concerning the narcotic principle contained in the puff-ball—1st. That it was of a most volatile nature; 2dly. That it was not absorbed by alcohol, water, or strong alkaline solution; 3dly. That if the fungus was burned in oxygen gas, the narcotic principles still remained in the fumes, and produced its effect, if free oxygen was breathed with it. The fungus had been given internally to two animals without effect. In Italy, it was fried and eaten as food. In conclusion, Mr. Richardson said, that he had been anxious only to show that a volatile narcotic principle, capable of causing anæsthesia by inhalation, did exist in one of the fungi; it remained to be seen whether other fungi possessed a similar principle, and whether from a fungus an anæsthetic could be obtained that might be used in practice, with as little trouble to the operator and with less danger to the patient than ether or chloroform.

Dr. Snow corroborated Mr. Richardson's observations, having witnessed several of his experiments. There could be no doubt that the fungus did possess a very volatile narcotic principle, capable of causing insensibility to pain. As yet, however, the narcotic was not so practicable as chloroform. The subject deserved and required farther research.—[*Amer. Jour. of the Med. Sciences.*]

Hot Water and Soap in Ptyalism.

A great variety of remedies have, from time to time, been employed in the treatment of ptyalism; every practitioner

having his own favorite remedy. Tar water, solution of creasote, lead water, sumach root tea, sage tea and honey, alum, spts. turpentine, &c., have each acquired more or less reputation in the hands of different practitioners; but we have never been satisfied with any of these remedies, though we have repeatedly prescribed them. Very recently, having to treat a severe case of accidental ptyalism, we prescribed a *hot* solution of soap. The patient was suffering with severe pain of the gums and copious salivary discharge—a few drams of spirits of soap was added to one pint of *hot* water, and the patient directed to take it into the mouth, as hot as he could bear, and retain it until the surplus heat was exhausted, and repeat for an hour, allowing an interval of half an hour for rest. At the end of twelve hours, we had the gratification to find the patient almost entirely relieved of the pain—the swelling and redness of the gums and soft parts about the mouth rapidly diminished, and in a few days, by the persevering use of the hot water, the patient was free of all uneasiness about the mouth.

The value of hot water was suggested from having observed the good effects of hot tar water in a similar case. The patient, a delicate, nervous female, was directed to use warm tar water occasionally, but finding that the hotter the water, the greater relief was afforded, she continued using it as hot as the mouth could bear it. We had noticed too, the effects of the prolonged immersion of the hands of washwomen in warm *soap-suds*, corrugating and puckering the skin of the hands and fingers to such a degree that the blood seemed almost expelled from the vessels of the part.

The first effect of hot water in mercurial sore mouth, seems to be relief from the painful distension of the soft parts, and secondly, an anemic condition of the blood-vessels from contraction or collapse of the capillaries. The stronger preparations of soap are powerfully astringent—the kind used in preparing the spts. *sapo*. was the castile—it may be that *turpentine* soap is preferable.—[*Southern Jour. of Med. and Phys. Sci.*

Treatment of Sprains by "Firing." By JAMES DICKINSON, Esq.

The advantages of "Firing," in many forms of rheumatic and neuralgic affections, have been pointed out by Corrigan, Day, and others (vide "Abstract," vol. iii., p. 199). Its use in sprains of the back, seems to be one from which the most striking benefit may be anticipated, as is seen in the following remarks:

Sprained backs are cases which give the surgeon much trouble and annoyance, appearing in many instances to resist every

remedy. Many cases have come under my notice, and finding that blisters, cupping, stimulating liniments, &c., failed, I tried "Firing," and the results have been most successful; patients who for many weeks have evinced the greatest agony, have, after the first or second application, been perfectly cured. The plan to be adopted is as follows:—Heat a metal button, the shank of which is fixed into a wooden handle, to such a temperature as can be borne with slight pain; pass it lightly over the affected part, without inducing vesication, which is unnecessary. The pain produced is severe, but is transient. In long-standing cases, two or three applications are required; in recent ones, one will be found sufficient.*—[*Prov. Med. and Surg. Jour.*

Miscellany.

Medical Jurisprudence. By ALFRED TAYLOR, M.D., F.R.S. Third American from the fourth London edition. Edited with Notes and Additions, by EDWARD HARTSHORNE, M. D., one of the Surgeons of Wills' Hospital, etc. Philadelphia: Blanchard & Lea. 1853. 1 vol. 8 vo., pp. 621.

This work of Dr. Taylor has been for some time before the profession, and the favorable estimate placed upon it is evinced by the number of editions through which it has passed. We do not hesitate to express the opinion that this is one of the best works upon the subject, if not the very best which has yet appeared. This edition contains many important additions.

"Under POISONING, the additions include a notice of the new enactments affecting medical evidence, and of the recent decisions regarding the administration of poison,—some new facts and cases illustrative of the action of arsenic, opium, strychnia, the salts of lead, antimony and other poisons,—as well as improvements in the application of chemical tests for the detection of poisons:—under WOUNDS, the changes produced by the new statutes,—cases of peculiar and severe suicidal wounds,—struggling and power of exertion in wounds of the heart,—processes for the detection of blood stains, and their distinction from vegetable dyes,—the examination of weapons and projectiles,—additional facts regarding burns on the living and dead bodies: under CHILD MURDER, medical evidence derivable from an examination of the umbilical cord, with references to many new cases. Additions have been made under ABORTION, on the action of oil of savin:—under LEGITIMACY, on the duration of pregnancy,—proofs from the

* This remedy sometimes affords relief in cephalgia, and other forms of neuralgia.—EDT. BUFFALO MED. JOURNAL.

development of offspring,—plural and twin births:—under RAPE, fallacies attending evidence from the examination of females: under the various forms of death from ASPHYXIA,—additional facts regarding the examination of the bodies of persons who have died by drowning, hanging, strangulation, and suffocation,—facts and cases illustrating the means of distinguishing homicidal from suicidal strangulation.—under INSANITY, some additions on the application of Restraint.—decisions in recent cases, showing the liability of medical practitioners,—the testimonial capacity of the Insane, with remarks and cases illustrative of Homicidal Mania and the Plea of Insanity.”

Elements of Chemistry. For the use of Colleges, Academies and Schools. By M. V. REGNAULT. Illustrated by nearly 700 wood cuts. Translated from the French, by T. FOREST BETTON, M.D., M. A. N. S., Fellow of the College of Physicians of Philada., etc., and edited with notes, by JAMES C. BOOTH, Melter and Refiner U. S. Mint, and WILLIAM L. FABER, Metallurgist and Mining Engineer. Second edition. In 2 volumes.

This work has been “got up” with great care and labor, and contains a valuable embodiment of chemical knowledge in its present state of advancement. Its seven hundred wood-cuts are faithful representations, and admirably executed. Much of the apparatus employed in the performance of the most important chemical experiments, and in conducting the various processes of metallurgy and pharmacy, are here strikingly illustrated. The two latter subjects have occupied much of the attention of the author. The chapter on crystallography alone is enriched by 122 beautiful diagrams, drawn conspicuously in white lines upon a black back-ground, and exhibiting consecutively the principal crystalline forms, classified under six separate “Systems of Crystallization,” each system based upon the number and direction of the axes.

An elaborate exposition of the facts and phenomena of Organic Chemistry, also occupy more than 400 pages of the work. The typography and entire finish of these volumes do credit to the enterprising publishers, and on the whole, although too voluminous for an ordinary text book, “Regnault’s Elements of Chemistry” must be regarded as quite an acquisition to the library of the Chemist and Metallurgist, and especially valuable as a work of reference.

A. M****.

Singular nucleus for a Calculus.—Dr. Wm. G. Wheeler, of Massachusetts, reports in the American Journal of Medical Sciences, a case of Lithotomy performed upon a female, in which “a stone was

extracted weighing over two ounces and three quarters, and there was found passing obliquely *through its centre a large wire hair-pin, measuring over three inches and a half in length.*" . . . "Since the operation, the patient has stated that the pin *was introduced through the urethra about six years ago.*" . . . "The foreign body caused some pain and uneasiness soon after its passage within the bladder, but no severe symptoms were manifested until about two years after its introduction, since which time they have gradually increased in severity." The patient has recovered.

Extraordinary case of Saccharine Diabetes.—Dr. Fricke, of Baltimore, reports, in the Amer. Journal of Medical Sciences, a case of Saccharine Diabetes, that on one day passed eight hundred and ninety six ounces of urine, equivalent to fifty six pints, or seven gallons; and the amount of sugar contained in this enormous quantity of fluid, was estimated at four pounds ten ounces.—[*Western Lancet.*]

Saccharated Medicinal Powders.—Some of the most useful of our pharmaceutical preparations are those known as tinctures, which hold in solution many of the most active principles of vegetable substances. Yet as alcohol, either pure or more or less diluted with water, constitutes the greater part of their composition, the frequent administration of this substance is, occasionally, very much open to serious objections, both on the part of the physician and his patients. For this reason Dr. Becker recommends the employment of saccharated medicinal powders as substitutes for tinctures, whenever the latter may be considered objectionable. He directs equal proportions of the tinctures of hellebore, cinchona, hyoscyamus, or of other vegetables, as the case may be, and sugar, to be well mixed together, and then evaporated so as to drive off the alcohol, and then to administer the residue instead of the tincture. To this residue he gives the name of *helleborus saccharatus, hyoscyamus saccharatus, cinchona saccharata, &c., &c.,* according to the drug made use of.

This mode of preparation of medical substances has evidently attracted the attention of some of the medical authorities of our own country, for in reference to this subject the editor of one of the medical journals makes the following practical observation: "Supposing the unimpaired medical properties of the tinctures can be thus fixed in these powders (which is problematical), this mode of administration would prove a great boon to physician and patient. Not only is alcohol obviously mischievous in many cases wherein the active principles of which it is the vehicle, are indicated; but in others in which such contra indication is not so apparent, it has often proved a means of inducing a habit of dram drinking, which prevails even among respectable females to a far greater extent than is usually supposed.

[*Annals of Pharmacy.*]

Mixtures or other Medicinal Preparations, containing Gum-resins.—The gum-resins, such as myrrh, ammoniacum, assafoetida and others, may be successfully suspended in mixtures and other preparations by means of the following easy and simple process. On adding six or eight drops of pure sweet almond oil to a very small quantity of the gum-resin employed, it forms into a mass by trituration in a mortar; and when the oil and gum resin have become thoroughly incorporated together, the result is a smooth paste, to which the vehicle, in which it is intended the gum-resin shall be administered, is to be added very gradually. By this means an emulsion is prepared in a very speedy manner. This method of preparing emulsions with the gum resins admits of the mixture being heated, if necessary, without causing the least coagulation; whereas emulsions prepared by means of the yolk of egg will not allow of the application of heat, without more or less coagulating; an advantage of which both the pharmacist and the physician will on all occasions gladly avail themselves.—[*Ibid.*]

Child poisoned by an over-dose of Laudanum—Restored by Galvanism.—The March number (1853) of the London Lancet contains the report by Dr. Kirk, of a case of poisoning by laudanum, in which galvanism was successfully applied after all other means had failed. When the child, only one month old, was first seen, it had no pulse, hands and feet cold, features shrunk and deadly pale; aroused with difficulty, and soon relapsing into a state of complete stupor; pupils contracted; respiration slow and scarcely perceptible; suspended at intervals.

The child being unable to swallow, Mr. Kirk applied one of the conductors of a galvanic battery over the epigastrium, and taking the other conductor in his right hand, he passed one of the fingers of his left hand into the mouth of the child, between the teeth and gums, as far back towards the fauces as it could be conveniently carried (the battery acting as feebly as possible.) In the meantime the breathing became regular, the action of the heart was steady and more vigorous, and all the symptoms of approaching death disappeared. As often as the galvanic influence was withdrawn, all the unfavorable symptoms returned; but they again disappeared on the renewal of the agent. At the end of about four hours, Mr. K. succeeded completely in restoring the child, when a mild emetic was administered, followed by a dose of oil, and the child rescued from its perilous condition.—*New Orleans Journ.*

Chlorate of Potash in Croup.—The following facts, as given by Dr. Sankey, corroborate the views laid down by Dr. Budd in his paper on Croup: A child aged 13 months, with croup and bronchitis, was treated with blisters, salines, ipecac, etc., without benefit, when he was put upon the use of *chlorate of potash*; and although the child was much reduced, the circulation languid, the blood not properly oxygenized, yet in a short time the croup and bronchitis disappeared, and the child was restored to its usual good health.

Another child, æt. three years, with croup, had been treated with leeches, calomel and ipecac, without much benefit; he was put upon the *chlorate of potash*, and soon his breathing became easy, and his countenance lost its anxious and livid hue. It recovered as in the first case. The chlorate of potash is supposed to be in part decomposed, in these cases, and thus a large portion of its oxygen is given to the blood, for the absence of which the child perishes in attacks of croup.—[*Ibid*, from *Braithwaite*.

A Formula for a Castor Oil Electuary. By SEPTIMUS PIESSE.—Many persons' stomachs revolt at taking castor oil in an undisguised form. To overcome this repugnance, it has been the practice to administer it in the shape of an emulsion, which involves a large increase in the bulk of the dose to be taken, as well as the employment of a considerable quantity of gum or the yolk of an egg, to form the emulsion. To disguise the castor oil, to give it in a condensed form, and to diminish, as much as possible, the quantity of the excipient, the following formula has been devised:

Take of castor oil,	3 ounces.
White soft soap,	1 drachm.
Simple syrup,	1 drachm.
Oil of cinnamon,	6 drops.

Rub the soap with the simple syrup in a mortar, and then add gradually the castor oil, with constant trituration, until it is thoroughly incorporated with the above ingredients. Finally, mix with the electuary, thus formed, the oil of cinnamon, or any other essential oil that may be preferred. By these means, a gelatinous electuary will be formed, which is rather palatable than otherwise, and nearly equals, bulk for bulk, castor oil in strength. The quantity of potash present in a dose of this electuary is only a homœopathic dose, and consequently, not likely to produce a bad result in any case, even when its use should be contra-indicated.—[*Annals of Pharmacy*.

Butter as a substitute for Cod Liver Oil.—The Union Médicale, for May 3, quotes the following from the Répertoire de Pharmacie:

Cod-liver oil is an aliment which restores and reconstitutes the tissues; in a word, it is an analeptic medicine, by the aid of which the disorganizing action of tubercle is combated. The only inconvenience attending its use is, that it is sometimes difficult of digestion. In this case, M. Trousseau substitutes with advantage for it, the following compound:—

Fresh butter	3iv.
Iodide of potassium	gr. $\frac{3}{4}$.
Bromide of potassium,	gr. iij.
Common salt	3ss.

This butter is eaten during the day on very thin slices of bread.—[*Association Med. Journ.*

Raw Meat in the Diarrhœa of Infants. By M. TROUSSEAU.—At the time of weaning, and for the first year following, children suffer from diarrhœa which is with difficulty overcome by pharmaceutical preparations. The evacuations are frequently accompanied with anæmia which is not relieved by the ferro-manganesian preparations of our pharmacopœias. M. Trousseau administers with success, in these cases, raw meat finely hashed and slightly salted. In the case of a child three years old, he gave from four to eight ounces of it every morning. The raw meat retains in its fibres a certain quantity of blood; this organic liquid contains iron and manganese, but in combinations much more natural and of easier digestion than the pharmaceutical preparations. Raw meat is preferable to cooked, and beef blood is much better than raw meat. "We will add," says the *Presse Médicale Belge*, that it is a vulgar custom at Brussels and many other of our cities, in certain affections, to drink the warm blood of the beef, on an empty stomach, each morning gradually increasing the quantity. We have seen hæmoptysic persons, wasted by frequent hemorrhages, recover from the loss of blood in a short time, by drinking half a pint of blood each morning at the slaughter house. No known pharmaceutical article can equal this ferro-manganesian preparation, compounded by nature, and ready to be digested and assimilated. But it is necessary to drink the blood the moment the beef is killed, as it loses its properties partially by cooling, from the time the globules and serum begin to separate. Children take the blood willingly, though many adults will object to it. Unfortunately, however, it is always difficult to obtain the blood before its coagulation.—[*Journal de Chimie Médicale.* *Virginia Med Jour.*

American Medical Society of Paris.—We translate the following from the *Gazette des Hopitaux*, of March 24th.

"We learn that the American Medical Society continues to receive a large number of new members. Twenty five or thirty members, and a number of visitors, are present at the weekly *re-unions*, which gives a lively interest to the original dissertations and the debates to which they ordinarily give rise. Dr. W. E. Johnson (of Ohio) has recently been elected President of the Society.—[*Western Lancet.*

Extraordinary Accident.—A man was lately admitted into the Portsmouth, Portsea, and Gosport Hospital, under the following singular circumstances: He was trying to extract a cork from a large stone beer-bottle with his teeth, when it was suddenly driven into his gullet by the force of the carbonic acid which had been generated in the bottle. Medical assistance was immediately obtained, but unavailingly, and the man was taken to the hospital, where œsophagotomy was at once practised, and the cork, which measured about three inches and a half in circumference was extracted.—[*St. Louis Med. and Surg. Journal.*

SOUTHERN MEDICAL AND SURGICAL JOURNAL.

Vol. 9.]

NEW SERIES.—SEPTEMBER, 1853.

[No. 9.]

PART FIRST.

Original Communications.

ARTICLE XXIX.

Uva Ursi, a substitute for Ergot. By E. G. HARRIS, M. D.,
of Fayette, Alabama.

I wish to call the attention of the medical profession to *Uva Ursi*, as a substitute for *Ergot*, in producing uterine contraction. Whether it will produce all the effects, or answer all the indications of that drug, I am not able to say; but I do know, that it will produce considerable uterine contraction when given during labor. Since December last, I have given it in five cases, in all of which it acted more efficiently than *ergot*. In three of these cases the pains had ceased entirely, from exhaustion of nervous energy. A strong decoction of *uva ursi* was given every ten minutes, and in thirty minutes the pains had increased considerably, and in from one to one and a half hours, the delivery was effected and the placenta expelled, the uterus contracting well, and no untoward symptoms taking place. In the other two cases the pains had not ceased, but were fast doing so. I gave them bountifully to two of my *new parturients*—one of them was delivered in fifty minutes, the placenta following in less than five; the other in an hour and twenty minutes, the placenta in ten. In all these cases the head occupied the superior strait at the time I commenced giv-

ing the medicine, in consequence, no doubt, of inefficient uterine contraction. The os uteri was in good condition in all the cases. Directly after the exhibition of the *uva ursi*, the pains would become strong and propulsive, lasting about the ordinary length of time; then going completely off, leaving the patient free from pain, except that produced by pressure upon the soft parts: the placenta soon followed; the uterus contracted well, and no hemorrhage more than ordinary. There was little or no tonic contraction until after the expulsion of the placenta, when it was complete.

I am aware these five cases are not sufficient to establish its reputation as a therapeutic agent in labor, yet the result of the past encourages me to give it a further trial; and should its use in the future prove as successful in the hands of my professional brethren as it has in mine, I shall feel amply rewarded for any trouble I may have been at in bringing it to their notice. I was first induced to use it by being called to a case where the pains had ceased; and having no ergot, and knowing the effect it had on the kidneys and bladder, I gave it as above described. I prefer it to ergot, for the following reasons:

1st. Because there is no danger in it; you may give it *ad libitum*. It is well known that ergot often produces nausea and vomiting, and sometimes slow weak pulse, cold extremities, dilatation of the pupils, &c.

2d. Although it increases the propulsive efforts of the uterus, yet it does not produce that tonic contraction which is so painful to the mother, and at times so hazardous to the life of the child, until after the delivery is affected: this is generally far otherwise with ergot. More than once have I seen a young and healthy mother give birth to a well developed but dead infant—not from the poison being absorbed by the mother, and through the circulation destroying the child, but *alone* by the powerful tonic contraction of the uterus compressing the umbilical cord, arresting the circulation, &c. Often have I seen the placenta retained for hours, and in two cases for a day and night after it had been entirely detached from the uterus, by the firm and unyielding grasp of that organ—all brought about by the free administration of ergot

My plan for giving it is as follows:

R. Uva ursi, a good article, 2 oz.

Boiling water, . . . 1 pint.

Pour the water on the leaves in a pitcher or bowl, stir it till it becomes cool enough to drink, and give one-fourth as hot as it can be drank every ten or fifteen minutes, until it has the desired effect. Attention should always be paid to the condition of the os uteri, dimensions of the pelvis, &c.

ARTICLE XXX.

A Case of Hepatic Abscess—Communicated to the Editor, by
H. R. CASEY, M. D., of Columbia Co., Ga.

APPLING. Aug. 1st, 1853.

Dear Doctor—I have thought a description of a rather unusual case, which occurred in my practice about a year since, would be read by you with interest. If I had taken notes of the case at the time, I would have given it to you in a form which would have entitled it to a place in your Journal; but as I failed to do so, the account I can now give of it will necessarily be imperfect (from the lapse of time). I will give you the particulars, as my memory serves me.

I was called to see Mrs. J., aged 24, whom, upon examination, I found in bed with a face very much flushed and in a profuse perspiration; high fever; pulse 120, full and bounding. On enquiry, I learned that she had been nursing a case of typhoid fever. She seemed much frightened, thinking that she had contracted that much to be dreaded fever. I soon, however, quieted her fear on that score, telling her that she had no symptom of typhoid, but a fever of an exactly opposite type. I bled her freely, gave a mercurial cathartic, and left 15 grains quinine, to be administered should the fever subside. Visited her the next day, when the true character of her disease began to shew itself. Pulse 125; tongue heavily coated; skin hot and dry, and slightly tinged with yellow. She now complained of pain in her right side. On examination, I found considerable swelling, the intercostal spaces indicating pressure from within. Ordered leeches over the region of the liver—calomel and Dover's, powders in small and separate doses, to be followed with Seidlitz powders; warm cataplasms to succeed the leeches.

Did not see her the next day, from press of business. The following day was summoned to see her in haste. Found her complaining of increased pain and soreness—the distension much greater. There was now evidently a change in the character of her fever: the bright hectic now mantled her cheek, while the blood which had before coursed rapidly along the vessels, now took on a slow and measured step. It was now apparent that the hepatic inflammation had passed into the suppurative stage, and that an abscess was there formed. The liver was very much enlarged, but as yet no pointing. I applied a large blister over the swelling, put her upon a generous diet, and gave her the muriated tinct. of iron, and withheld the lancet for further developments.

Some twenty-four hours thereafter I was again called in haste to see her, when she told me that the pain had left her side, and had located itself in her hip. On examination, I found a spot larger than the palm of my hand on the right hip, fiery red. Not knowing how to account for this, but supposing it to be of an erysipelatous character, I scarrified it, and ordered it to be covered with althea poultices. Continue the tr. ferri.

In the course of the next day I again saw her: found her hip much swelled and fluctuation evident. Continued the warm application, and on the following morning I plunged my lancet in the tumor, and gave exit at once to at least a pint of matter. It was pus, of a creamy appearance and consistence, with not even a tinge of yellow, and exceedingly offensive. The amount of matter, and the recent date of the hip affection, determined its origin in the liver; but why the pointing should have been down here, instead of opposite the liver, I could not answer. This being my first case of hepatic abscess, I was not well “*posted up*”—I judge they are not of frequent occurrence in this country—not remembering an instance where an autopsy has revealed a cicatrix of the liver as the result of hepatic abscess. As further confirmatory of the fact that the matter came from the liver, when the patient sat up, the matter would gravitate in a bolder stream, and by pressing over the liver and passing the pressure downwards towards the orifice, the matter would make its exit freely. In the progress of this, another pointing was observed opposite the liver, and with my lancet I gave exit to about a teacupful of matter.

Louis states that he has never known of the occurrence of a cicatrix of the liver the result of a cured hepatic abscess. I am certain, if ever the body of this patient becomes a subject of the dissecting knife, a *large* cicatrix of the liver will be among the autopsical appearances.

I called to-day to see the lady, to question her as to the facts of the case. She states that the matter was discharging three months or more, and that she thinks as much as three gallons of it passed off in the time. This, of course, is too large an estimate. She states that she is in the enjoyment of good health, but has occasionally pains in her right side, when she stoops down or bends to the opposite side, doubtless from adhesions of the liver. She thinks her "entrails have grown to her side," and states that she was "physicked" some years since for liver disease.

Your's respectfully,

H. R. CASEY.

PART II.

Eclectic Department.

Letters upon Syphilis. Addressed to the Editor of L'Union Médicale, by P. RICORD. Translated from the French, by W. P. LATTIMORE, M. D.

EIGHTEENTH LETTER.

[Continued from Page 478.]

My Dear Friend,—Positive inoculations always pursue the course I indicated in my last letter.

When the inoculation fails, the puncture sometimes becomes slightly irritated, but it immediately subsides.

Still, without taking away from inoculation any of its certainty, it is necessary to know that syphilis, like variola, like vaccinia, has *false pustules*. Their existence, if the examination be superficial, may lead to error. My learned colleague, M. Puche, now acknowledges, with a good faith which does him honor, that he was thus deceived by *false pustules* when he made inoculations with the muco-pus furnished by balanoposthitis. Consequently he does not now accord the same value as formerly to the facts contained in the *Memoir* which he published on this subject; he has better studied these facts, and

their signification to him has changed. You understand, my dear friend, that I would not commit the impropriety of speaking thus, had I not the formal authorization of M. Puche for so doing. My critics, then, who made much of the inoculations of the muco-pus of non-ulcerous balano-posthitis, who have used this as a weapon against my doctrines, who would prove by it that chancre does not alone furnish inoculable pus, and that inoculable blennorrhagia may not be accompanied by ulceration—these critics, then, I say, can no longer use this argument without the new verification which its author believes to be indispensable.

These *false pustules* are but slightly developed; most usually they are only simple bullæ, beneath which we find a superficial vesication of the skin. Here there is not that complete perforation of the derma, *en emporte-pièce*, which is observed in the true inoculation. In some very rare cases, a more profound inflammation may supervene, and produce something analagous to furuncle; but the march is always, and even in these cases, very rapid; the duration ephemeral, from three to five or six days at most; and the cure supervenes also very quickly, without the intervention of any treatment.

However it may be, I have said, and I still say, that when the inoculation does succeed, the chancre invariably begins by a pustule; this is incontestable—something which can be reproduced at will.

However, the syphilographers who have ranked with the primitive accidents of syphilis so many accidents which do not belong there, ought, really, to place among them this *ecthyma* developed under the conditions already mentioned.

It is true that our learned confrère, M. Cazenave, says that *ecthyma* may sometimes be primitive. He even cites in his *Traité des Syphilides* a very fine example of primitive *ecthyma* of the lip, the direct and immediate result of contagion. But what M. Cazenave says of this case, so frequent and common in my experience, proves to me precisely that neither Bielt nor he knew the true nature, the veritable essence, of this accident. Read over this passage from M. Cazenave, and you will be convinced that, in this particular case, he does not consider the *ecthyma* as being simply one period of the chancre. According to him the *ecthyma* which he calls *primitive* is always a *syphilide*, that is to say, the product of a general constitutional infection—in a word, what I call a *secondary symptom*.

But, in order to prove that *ecthyma* is always the result of a previous general infection, although it may be the only isolated accident by which syphilis commences; in order to succeed in confounding the chancre of the *ecthymatous debut*, the veritable

primitive, *contagious, inoculable* ecthyma, with the secondary, constitutional ecthyma, M. Cazenave, after saying so plainly that this accident might be the first and only result of the contagion which, "aside from the influence of the virus, needs for its development particular conditions," conditions which, in short, are those demanded for the inoculation of primitive accidents—M. Cazenave, I say, wishing, against his own conviction, to bring ecthyma among the syphilides, gives as examples of primitive pustular syphilides, two observations where this accident was perfectly secondary, and regularly preceded by a primitive accident on the fingers.

This error is very common with persons who are unacquainted with all the varieties of chancre. Did not this occur to one of our unfortunate confrères to whom M. Cazenave alludes? Was he not thought to have experienced a constitutional infection *d'emblée*, and as having offered an example of primitive pustular eruptions? And yet this poor confrère had had a chancre upon one of the fingers of his right hand; chancre followed by an epitrochlean adenitis, and afterwards, in the regular order, by secondary accidents. All this was verified, both by my learned friend M. Nelaton and by myself. True, a person not much skilled in venereal diseases, although he has written a great deal about them, was cognizant of the ulceration of the finger, but pretended that it was only an *anatomical tubercle*, which had simply given passage to the virus, without itself being inoculated. I am very much afraid that this person's brain gave passage to this beautiful story without being itself inoculated, during the transit, with even a slight resemblance to common sense.

I have not yet done with primitive ecthyma. You, who read everything, sometimes from a sense of duty, often from taste, and always with profit to those who in turn read you—you must have been surprised at seeing in a *manual* of syphilitic diseases (the learned author of which we both highly esteem) that this author admitted the possibility of this production of a pustule from artificial inoculation, but not otherwise. In fact, M. Gibert resolutely denies that chancre not artificially inoculated can commence by a pustule; he affirms that this period of chancre has been admitted through error in diagnosis. I think you already perceive on which side lies the error. "If you admit," I will say to M. Gibert, "that a pustule may be produced by the point of a lancet, acknowledge that no great effort of imagination is necessary to find, in the processes of ordinary contagion, something which may act in the same manner, a nail, a hair, etc., without taking into account other circumstances, of which, in your character of

syphilopath, you must receive the lascivious and disgraceful confessions."

See, my dear friend, how even the most distinguished observers are liable to err! Assuredly, both M. Cazenave and M. Gibert know as well as I what *ecthyma* is; and yet how does it happen that they always obstinately refer it to a general state, and deny its existence as the product of a chancre? Why? Because theory too frequently throws a deceptive veil between the observer and the matter of observation; because, as another observer has just told us, ten years passed in a venereal hospital do not suffice to make one comprehend what passes there; because, alas! there are eyes which are always looking, but which never see.

I ask your pardon, my dear friend, for dwelling so long upon the pustular form of chancre. I have done so because I think it is at length time to get rid of that *parrotage* which always gives the same invariable characters to the primitive accident, as though it were changeless and eternal in form. Than this doctrine nothing is more false, more contrary to every day's observation. The primitive accident, on the contrary, presents numerous varieties, both at its commencement, during its march, and subsequently. Allow me here to bring forward what observation and experience have taught me.

Most usually, chancre begins by an ulceration either superficial or profound. The primitive ulcer does not always destroy the whole thickness of a mucous membrane or of the skin. Thus, on the semi-mucous membrane of the glans and prepuce, the ulceration may be so superficial as to give rise to belief in an ulcerating balano-posthitis, and to justify certain successful inoculations.

The ulcer *d'emblée*, then, is produced when the virulent pus is deposited either upon a surface recently denuded, or upon a bleeding wound, or, which is more difficult, and consequently rarer, upon a suppurating wound. We also see, sometimes—and this has been disputed by those who habitually dispute everything—chancre commence under the form of an abscess. Thus, inoculated leech-bites frequently present, it is true, an *ecthymatous* form; but it also happens that the virulent pus inoculates the bottom of the puncture without contagioning the borders; these may then unite and inclose the virus which inoculated the bottom; and this bottom then gives rise to a little virulent abscess of the subcutaneous cellular tissue, which, on bursting or on being opened, presents a chancreous *foyer*. The infiltration of the virulent pus into the subcutaneous or sub-mucous cellular tissue gives rise to the same phenomenon.

All this is from the every-day practice and observation of my hospital service. I am well aware that, in this so simple theory of the abscesses as the first period of chancre, some have sought an argument in support of the existence of the bubo *d'emblée*, an existence which I do not admit, and which seems at variance with my doctrine. But I will hereafter return to these buboes *d'emblée*, and in such a way as, I trust, will content my opponents. However it may be with these different beginnings of the chancre, their varieties have no influence upon the ulterior form of the ulcerations.

This point has its importance; it bears upon the question of the unity or the plurality of the syphilitic virus, a question still obscure enough, or rather obscured by the vagueness and want of precision of the facts. So far as I am concerned, here is what I can say in regard to it:

When the inoculation is made on the patient himself, the debut of the chancre being always the same, the ulceration following the inoculation takes, finally, the form and offers the same varieties as the first accident which furnished the inoculable pus. Thus, if the pus was taken from a phagedenic chancre, the ulceration will take on the phagedenic character; if from an indurated chancre, the ulceration will indurate, etc. This is what my own experience has taught me. But, in the inoculations made from diseased to healthy individuals, have matters always happened thus? We cannot tell; for in the inoculations thus made by other experimenters, no note has been taken either of the form of the accident from which the pus was taken, nor of the form of the resulting accident. They have been contented with saying chancre on one hand, chancre on the other, without any detailed description; so that, in fact, these inoculations cannot be of much assistance in elucidating the question.

In common observation, we find that one form in one individual may produce a different form in another. But as we are never rigorously sure of the source of the infection, the results may be contested by supposing that the individual who has a different form may have contracted it from another source than the one he accuses. The results of the last inoculations just made from diseased to healthy individuals, are balanced, and serve neither pro nor con. In the observation of M. de Welz, the pus was furnished by a non-indurated chancre, and his chancres were not indurated, which may depend upon a want of aptitude in him. In the fact of the inoculation upon the interne of the *Hôpital du Midi*, the chancre indurated; and yet the pus with which he was inoculated, must have come from a non-indurated primitive ulcer, owing to the conditions

of previous constitutional syphilis under which the patient labored.

You perceive, my dear friend, that this question of plurality of virus, so clearly laid down by some English physicians, is far from being resolved. Up to this time we are entitled to believe only in the existence of a single virus. It appears rational to admit that chancre, commencing always in the same manner and under conditions which may be determined in advance, depends upon a single cause, the ulterior effects of which are determined by conditions existing in the individual in whom it is developed.

In fact, the numerous varieties presented by the primitive ulcer at the period of progress, which manifest themselves more or less quickly, and which may be summed up thus—

Simple chancres; Inflammatory chancres, with a markedly gangrenous tendency; Phagedenic chancres; Indurated chancres—seem to find the reason of their existence in causes secondary and external to the specific cause. I am not now delivering a course of lectures; I am not writing a book of special pathology, and consequently cannot enter into too long details. But, in order to justify my proposition, allow me to recall some of the coinciding causes which give to the chancre this or that physiognomy, this or that appearance, this or that march.

For example, observation demonstrates the results of the abuse of alcoholic drinks, especially in warm weather. The simplest chancres, under their influence, rapidly become inflammatory, and inflammation in certain regions, as the genital organs, in a cellular tissue which easily becomes edematous, very soon reaches gangrene. The action of alcohol in these cases, with which the English have furnished us such splendid examples, is so marked that these ulcers might be called aenophagedenic.

For the other varieties of phagedenic chancres, pultaceous, diphtheritic, serpiginous, etc., their cause may often be found in certain hygienic conditions, unhealthy dwellings, bad nourishment, want of cleanliness, in the rash employment and in the abuse of rancid mercurial ointment for dressings, in certain diathetic conditions, tubercles, scrofulas, herpetic vice, scurvy, and often in the different conditions which favor the production of hospital gangrene. Add to these, as we shall hereafter see, the influence of an anterior syphilitic diathesis.

However, the most interesting conditions to know, those which almost of themselves constitute the pox, are those which preside over the *induration of chancre*.

But the *indurated chancre* being one of the important points

of the doctrine which I uphold, and which these letters are called upon to defend, you will allow me to make of it the subject of my next letter.

NINETEENTH LETTER.

My Dear Friend,—If I have been well understood in my last letter, you have seen that I admitted the unity of the syphilitic virus, although the fact has not yet been incontestably demonstrated; that I did not, like some syphilographers, seek the explanation of its varied effects in its greater or less activity, or in its different degrees of acrimony. These effects, on the contrary, I attributed to certain conditions in the individual subjected to its action; so that, in spite of several cases of Bell, and of analogous cases still occasionally met with in practice, in which there is only a simple coincidence, we can draw no inference from the form and gravity of the primitive accident of an individual, as to the form and gravity of the disease of the person by whom it was communicated. Finally, we can no longer say, as we formerly said, to a patient: "If your disease is of a grave form, it is because the person who communicated it to you was seriously affected; inasmuch as the contrary is very often observed.

This law, with respect to the unity of the virus, being laid down, I will now direct your attention, as I promised in my last letter, to the most important variety of chancre—namely, *indurated chancre*.

The induration of chancre—a condition which certain primitive ulcers assume—was not unknown to writers of former times. Some authors even pretend that traces of the doctrine may be found in Galen—a circumstance which does not surprise me the least in the world, inasmuch as I believe in the antiquity of the pox. It is certain, that after the great epidemic of the fifteenth century, some of the first syphilographers of the time described this remarkable symptom. This fact did not escape the attention especially of Jean de Vigo, who has other titles to our esteem than that based on the invention of his famous plaster.

Nevertheless, you know that to Hunter is awarded the honor of having first described indurated chancre. This symptom has even received the name of the great physiologist. The *Hunterian* chancre, in fact, is nothing else than indurated chancre. And yet Hunter scarcely touches on this subject. You remember what he says in relation to it: "Chancre has usually a thickened base; and although the common inflammation extends much beyond it, still the specific inflammation is limited to

this base." But, as you see, Hunter does not make this thickened base a constant condition; and he was right, for the greatest number of primitive ulcers do not present this peculiarity. Nor does he make it the condition of the constitutional infection, an important and inexplicable omission in a man of Hunter's sagacity, instinct and divination.

The syphilographers who came after him, even Bell, with his illustration of a *split pea*, did not appreciate the whole value of the induration.

Since the time of Bell, most syphilographers have paid no attention to this symptom. M. Lagneau, in his treatise, appears to attach no importance to it. But I must do M. Lagneau the justice, to state that, with Bell and others, he recognized the fact, that chancre might have a pustular period. But aside from this circumstance, you will be struck, like myself, with the confusion which pervades his descriptions of the chancres which he calls *primitive*, and those which he calls *secondary*. In no respect can he be said to have correct views, relative to the induration of chancre.

M. Cazenave, "whose work is all alike, and who cannot be considered in earnest"—expressions of courtesy which he has but recently used in regard to myself, and which I return, so that I may keep nothing belonging to him,—has a method of appreciating primitive accidents which is truly incredible. Does he acknowledge any other primitive accident than the *infecting* act? According to him, in fact, other accidents must be either *primitive secondary* or *secondary primitive*. Escape from this dilemma, if you can, notwithstanding all the wit with which you daily regale us. At all events, the induration of chancre—the capital phenomenon in the disease—does not appear to exist on the *other side of the river*, as Lisfranc observed.

And yet, who can now misconceive the importance of this phenomenon? In view of all that I have done to elucidate this subject, in view of the judicious observations of the learned Professor Thiry, of Brussels—of those of my pupil and friend, M. Diday, of Lyons—of those of M. Marchal (de Calvi)—of those of my learned friend and too kind partisan, M. Venot, of Bordeaux—of those of MM. Acton and Méric, of London—of those of my learned colleagues MM. Puché and Cullerier; and finally, in view of the observations of my hospital patients themselves, whose education is such as to leave few chances for inattentive physicians to commit error,—I am justified in concluding, that they who do not recognize the value of this phenomenon, have eyes which do not see.

Therefore, as this induration, which may *line* as well as

surround chancres, merits the utmost attention of the practitioner, allow me to study it carefully.

All chancres do not become indurated ; at present, only a small number become so ; and if my doctrines are true, this number will constantly diminish.

But what is the specific condition, ulterior to the insertion of the virus, which causes the chancre to indurate ?

This is one of the most interesting problems which the study of syphilis can present, and it is also one exceedingly difficult of solution. Nevertheless, I believe I have found one of the unknown quantities.

When we interrogate the age of the patient, relative to the cause of the induration, we receive no reply.

The sex, the temperament, the hygienic habits of the patient, are interrogated with no better result.

Anterior or concomitant diseases, under which the patient has suffered, do not enlighten us any more than the specific medication to which he has been subjected.

Thus far, then, we have been forced to content ourselves with the common explanation, which, you know, refers everything to aptitudes and idiosyncrasies.

In fact, it is found that the first chancre developed in certain individuals does not become indurated, while a second one does ; and that those contracted subsequently do not indurate.

What now is the cause of this mysterious phenomenon ?

One reason for this difference, which has thus far escaped notice, we shall seek in the general and constant laws of virulent diseases ; in the striking analogies which exist between variola, vaccinia, and the pox.

We are now in the true path.

Vaccination, for example, may fail for the first time ; this failure will be due to some want of aptitude of which we are ignorant ; but, if it succeeds, the unsuccessfulness of subsequent vaccinations is explained. The effect of the diathesis produced by the first vaccination is not yet worn out ; and a certain period, which modern observation is tending to determine with accuracy, must elapse before the organism again acquires aptitude for a vaccinal impregnation.

Very well ! We have thus arrived at a capital fact in syphilogeny, a fact which long experience has demonstrated—a fact, which has been also observed by two persons, whom it is always a pleasure to cite, MM. Puché and Diday. The fact is this :

As a *general rule*, A PATIENT WHO HAS ONCE HAD AN INDURATED CHANCRE WILL NEVER HAVE ANOTHER.

With respect to vaccinia and variola, it is probable that this

law must present exceptions ; I will add that it is even desirable that these exceptions should exist inasmuch as they show that the syphilitic diathesis may be destroyed.

But, one thing is very certain : these exceptions are far more rare with respect to syphilis ; for MM. Puché, Diday and myself are still in search of indubitable proofs of their existence.

This circumstance is due, my dear friend, to the fact, that, when there is indurated chancre, there is of necessity *constitutional pox*.

With the induration, the syphilitic *disposition*, as Hunter called it, is acquired ; the syphilitic *temperament*, as I have elsewhere stated, becomes established ; and finally, a specific *diathesis*, which gives rise to ulterior manifestations, is developed.

Neither the disposition, the temperament, not the diathesis can double or triple itself, any more than the analogous disposition in vaccinia can thus double or triple itself.

The indurated chancre is to the pox what the *true* variolic pustule is to the variola ; what the *true* vaccinal pustule is to the vaccinia.

The *non-indurated* chancre is the pseudo pustule ; it is a false vaccinia.

Here, my dear friend, you have an admirable law ; a law which brings the pox under the general laws of virulent affections ; a law which guides us in the study of syphilis, as the variolic and vaccinal inoculations guide us in the study of variola ; a law which satisfies the mind, and gives it a sure resting place after a painful and tedious voyage amid deceptive hypotheses and contradictory theories ; a law, which arithmetic—the first rule of which was so much outraged by one of your former correspondents—will serve to establish, if, to obtain the real sum, similar values be added together.

But I am not charged, at present, with the special education of your honored correspondent, the provincial student ; with the duty of teaching him to distinguish the difference which exists between a diathesis and the manifestations of this diathesis ; between the diathesis, properly so called, and the resulting cachexia ; to all of which matters I shall undoubtedly have occasion to return, and with respect to which I am afraid the mind of the poor student is in much trouble.

For the present let him be aware—he will excuse this magisterial style of speaking—that the diathesis, acquired by the infected patient, prevents the induration of another chancre which he may contract ; and that this immunity from a new general infection must also be hereditarily transmitted. By means of this fact we are able to understand the remark which was made a little while ago : This transmitted disposition may well have

an influence in the diminution of indurated chancres, and therefore in the diminution of constitutional poxes. Variola and vaccinia also present, in this respect, a curious field for study. This idea, which originated in my school, has been carefully studied in a remarkable thesis, maintained by a distinguished pupil of Val-de-Grace, whose name I cannot just now call to mind.

Therefore, the non-induration of chancres contracted at different periods, subsequent to the developement of an indurated chancre, is a proof which can easily be verified by statistics, of the *unicité*—a neologism for the introduction of which I am not to blame—of the syphilitic diathesis; a fact implicitly admitted by Hunter, when he said that the formation of the syphilitic disposition could be prevented, but that this diathesis could not be destroyed when once established; a fact which M. Cazenave did not suspect he had proclaimed in accordance with our views when he wrote in his *Treatise on Syphilis*: “We are not aware that the syphilitic temperament has ever been destroyed.” M. Cazenave would certainly not admit, as a sound principle in physiology, the assertion that there exists a double sanguine, or a double bilious temperament, any more than he would admit the assumption of the existence of a double glanders, a double variola, and a triple hydrophobia, to be a sound principle in pathology. The *non bis in idem* is thus, so to speak, a pathological law. I trust I shall be able thoroughly to elucidate this question, in all its bearings, while studying the evolution of constitutional accidents.

These points of doctrine, relative to the etiology of the induration, being thus established, let us now study this phenomenon with respect to the period of its appearance, and with respect to its seat, its peculiar symptoms, its nature, and its progress, that we may finally arrive at a true exposition of its consequences.

This important question will be the subject of my next letter.

[*New York Med. Times.*

Spasmodic Asthma.

The first number of the *Glasgow Medical Journal*, contains an interesting paper on this subject, by Dr. Eben Watson.

It appears from Dr. Strang's statistics (Report on the Glasgow Mortality Bill for 1851, p. 46,) that in the year 1851, 212 persons died of asthma in Glasgow; and in 1852 (Report on the Glasgow Mortality Bill for 1852 p. 28,) rather fewer, viz., 202. Now by the same tables, we also find that the total deaths from all causes, among persons above fifteen years of

age, amounted in 1851, to 4543, and, in 1852, to 4853; and seeing that asthma very rarely attacks persons below fifteen years of age, it follows that these two numbers afford the means of ascertaining the ratio between the general amount of mortality, and that accruing from asthma. Regarding therefore, the adult population alone, viz., persons above fifteen years of age, one death was caused by asthma, in 1851, for not more than 20.4 by all other diseases put together; and, in 1852, one death was caused by asthma for 23 by all other diseases. Or, to take another view of it, of all deaths happening to persons above fifteen years of age, 4.6 per cent., in 1851, and 4.1 per cent in 1852, arose from asthma.

The name of spasmodic asthma was originally founded on the mere supposition of a spasm in the air-passages, occurring so as to cause the sudden paroxysms of dyspnœa, to which the patient is liable; and now that the structure and functions of the bronchial tubes have been thoroughly investigated and made familiar to every one, we do not *suppose*, but we *know*, that such a spasm really occurs; so that in this instance modern science has confirmed ancient hypothesis. There are only two portions of the air-tubes where spasm can at all take place, so as to cause dyspnœa. These two portions are at the rimaglottidis, and at the extremities of the bronchial tubes, where, instead of cartilaginous rings, there exist muscular fibres. In all other parts of the bronchi, the rings of cartilage in their outer walls prevent anything like complete closure.

Laennec observed that during the asthmatic paroxysm there was great diminution, or even complete absence, of the respiratory murmur; a fact which is explained by the small tubes being obliterated by the spasm, so that the air cannot pass into and distend the air-vesicles. When the spasm begins to relax, the patient inspires slowly and with difficulty; a vibratory sound, accompanying the inspiration, is heard by the bystanders and much more loudly through a stethoscope placed over the thyroid cartilage. It is caused by the vibration of the glottis, still partially stretched over the entrance to the windpipe. Sufficient importance has not been attached to the spasm of the glottis in asthmatic cases; it is the glottidean contraction which chiefly hinders the patient from overcoming that of the much weaker fibres of Reisseissen, in the smaller bronchial tubes. As soon as the muscles of the glottis relax, and not till then, does, the respiratory murmur become re-established.

Observation thus teaches us that the superior constriction is the last to give way; and Dr. Watson believes that, in early cases of asthma, it is the first to occur. There are two circumstances which prove this satisfactorily to his mind: first, the

fact that many cases of purely laryngeal disease end in spasmodic asthma; and second, that there are cases, though perhaps not very common, in which the affection is confined to the glottis.

In a paper on Chronic Laryngitis, published in the *Dublin Quarterly Journal of Medical Science*, in November, 1850, Dr. Watson stated it as his opinion, that inflammation of the larynx, especially if ulcers have formed, constitutes a not infrequent cause of bronchial asthma, and supported that opinion by the relation of a case, occurring in a lady, who had had acute laryngitis producing ulceration, and passing into the chronic state. Afterwards, not only the usual symptoms of the laryngeal disease remained, which were persistent, but a new affection was excited, viz., spasmodic asthma, in a most severe degree. The bronchial tubes ultimately became altered by the violence of the morbid agency that had attacked them. It was not to be expected that, at this late stage of the disease, any treatment could produce a perfect recovery: but it is satisfactory to be able to state that, after the cure of the laryngeal ulcers by the topical application of solution of caustic, the lady had no such severe asthmatic paroxysms as those from which she formerly suffered.

The occurrence of a kind of asthma confined to the glottis will be sufficiently illustrated by the following case:—

Case.—A young lady consulted Dr. Watson, about two years ago, for sudden attacks of breathlessness. She had no cough of any consequence, and in the intervals of the attacks she breathed freely enough; but as she seldom enjoyed a night's rest, her general health was somewhat disordered. Her pulse was quiet and natural, and there was no evidence of heart-disease; but her complexion was slightly florid, and her lips were of rather a bluish tinge. When he saw her, there was none of the bronchitis which generally attends asthma, and her age forbade the supposition of its being the ordinary kind of that disease. The respiratory sounds in the larynx were loud and harsh, and the length of the inspiratory sound was exaggerated.

The fits of dyspnoea were worst at night and in the morning. When the disease was mild, she could, by keeping very quiet and still during the evening, avoid the breathlessness for the early part of the night, and thus she got sleep for a time; but soon after midnight she was sure to awake with frightful dyspnoea, and was obliged either to rise from bed, or, at all events, to spend in a sitting posture the rest of the time usually allotted to sleep. Before she came to Dr. Watson, however, she was always attacked in a similar violent manner in the evening, so

that it was only after being completely worn out that she obtained a short repose, from which she was again roused by extreme breathlessness. There was no approach to hysteria.

No other treatment was used but the regular application of a solution of caustic (Ëi to Ñi) to the affected part, at first every day, and afterwards every second day. About six weeks of this treatment sufficed to remove the symptoms, and the lady remained quite well until the following winter, when she caught a slight cold and became affected in a similar way; but she applied to Dr. Watson sooner than on the former occasion, and half the time of the same treatment again produced a cure. During the autumn, she again had another attack of her disease; but this time it was so light, and treated so early, that it did not resist the topical application above a week. Since then she has been entirely free of the spasms, notwithstanding the very changeable and trying weather of the past winter.

Dr. Watson then concludes, 1st, that local causes of irritation in the larynx may produce spasmodic contractions, not only of the glottis, but also of the lesser bronchial tubes; and 2d, that spasmodic affections of the glottis may occur periodically for a length of time, without involving the small bronchial tubes in any great or important contraction. These conclusions, if correct, prove that asthma commences in the upper and not in the lower parts of the air-tubes; and that in the rational treatment of that disease, the remedies most likely to benefit the patient are such as may be applied to the laryngeal lining and to the glottis itself. But it must be remembered that, in many of these cases, universal bronchitis exists along with the spasmodic affection of the upper and lower tubes: this may arise either from causes capable of exciting both diseases, or the bronchitis may have existed previously to the occurrence of an asthmatic paroxysm. The former is then probably the exciting cause of the latter; and he admits that it is difficult nay, perhaps impossible, to ascertain with accuracy in this class of cases, whether the spasmodic affection was first excited in the small tubes, or at the top of the larynx. It is enough for practical purposes, however, to know that the latter region is always affected in such cases at the same time as the inferior bronchi, and with even greater intensity; and, moreover, that it is the spasm of the glottis which chiefly maintains that of the bronchi, by preventing their expansion during the forcible inspirations of the patient.

The ordinary treatment by bleeding, general or local, by emetics, antispasmodics, opiates, and mercurials internally, with blisters, and various other counter-irritants externally, has seldom been followed by even a partial success in these cases.

It is established, both by clinical observation and by Dr. Williams's experiments,* that bleeding carried to any length can never diminish the tendency to spasmodic contraction in the air-tubes; but during a bad fit of asthma, such a measure may be absolutely necessary to relieve congestions, arising secondarily, either in the brain or in the lungs themselves. Again, though emetics cannot save the patient from a renewal of the spasm, they may assist in overcoming that which exists, as well as in clearing away the mucus which clogs up the smaller tubes; and antispasmodics may assist in prolonging their good effects for a short time. In some cases, where there is much bronchitis, blisters have a good and more lasting effect, but they do not exercise much influence over the spasmodic asthma. In like manner, a slight mercurialization often benefits the bronchitis of the more sthenic variety, as indicated by the expectoration containing plastic matter, mixed with mucous globules; but it can have no effect on the paroxysmal disease. Opium only lulls for a time—an effect by no means to be lightly esteemed; but when the paroxysm becomes severe, it utterly fails.

There is here, therefore, an evident blank in therapeutics. There is no agent hitherto proposed which is capable of removing or greatly diminishing the morbid contractility of the air-tubes. And Dr. Watson thinks that a solution of caustic applied to the interior of the larynx supplies this defect. In proof of its having this exhausting effect on the irritability of the glottis, and ultimately on that of the air-tubes, he refers to the results of its use in whooping-cough, a disease which is so analogous to spasmodic asthma in its pathology, that it is almost enough to show the efficacy of a remedy in the treatment of one of these diseases, to prove its suitability for the other. Now, in proof that the topical treatment of whooping-cough is most efficacious and successful, it is enough to state that, combining the cases treated by him since he first proposed the plan in 1849, with those treated by M. Joubert, of Cherion, and published in the *Bulletin de Thérapeutique*, for January, 1852, we have as follows:—

	Cases.
A speedy cure (in ten to fourteen days) resulted in	78
Shortening of disease (three or four weeks' duration)	39
No change was effected in	8
Total number treated	125

* Report read by Dr. C. J. B. Williams, at the meeting of the British Association in Glasgow. See his work on *Diseases of the Chest*, p. 320.

There was not one death among all the cases treated, and, taking their percentage, we have—

65.4	.	.	were cured within a fortnight.
31.2	.	.	were cured in three or four weeks.
6.4	.	.	resisted the treatment.

100.0

He feels assured that no similar statement could be made regarding the results of any other method of treating whooping-cough.

He cannot, as yet, speak of great numbers of cases of spasmodic asthma treated in this way; but he has been very successful with the topical method in some cases that had previously been treated without much benefit in the ordinary manner. Of this he gives two instances.

Heart-disease is a frequent concomitant of asthma, and in such cases it is often supposed that the former is the cause of the latter disease; but this is by no means the constant relation of the two morbid states, for the disturbance to the pulmonary circulation, occasioned by frequent asthmatic paroxysms, is quite as likely to produce the heart-disease as the reverse. It is however, more important to call attention to the fact of the great difference between simple spasmodic asthma and that which coexists with heart disease. The pathology of the former has already been explained as an affection wholly confined to the bronchial tubes. But in cardiac asthma, this is Dr. Watson ventures to say, never the case. In that disease, the substance of the lung is always more or less altered; generally, the air-cells have become much distended, their walls atrophied, and even in some places ruptured; and it is this vesicular emphysema, not spasmodic contraction of any part of the bronchi, which produces the urgent thirst for air so distressingly experienced by these patients. There could be no good object served by introducing solution of caustic into the larynx in such cases.

There are, besides the topical application to the larynx, two other remedial measures which Dr. Watson has for some time employed in cases of spasmodic asthma, but regarding which he is not able to speak with precision. The one is electricity, applied in a gentle current, as much as possible along the course of the larynx and bronchi. In his experiments on the lower animals, Dr. C. J. B. Williams found that such a current destroyed the contractility of the tubes,* and in several instances Dr. Watson has thought that it co-operated with other means, in diminishing the frequency and severity of the asthmatic paroxysms. This, however, might be the effect, not only of its

* See Report, formerly referred to.

local, but of its general action as a tonic on the nervous system. The other agent referred to is strychnia, which he has used in repeated small doses of one-twentieth or one-sixteenth part of a grain, and he believes with good effect in some cases. Dr. Williams found that when animals had been poisoned by this substance, the air-tubes did not exhibit contractility, and he thought that they were retained in a tonic spasm by the operation of the poison. This very probably was the case; but of course the use of strychnia in medicinal doses produces totally different effects on the human system, and the benefit accruing therefrom must have another explanation. Dr. Watson believes that this medicine, in the doses mentioned above, is a powerful equalizer of nervous action, and therefore a good means of diverting that action from concentrating in any particular organ, such as the bronchi in spasmodic asthma.

In conclusion Dr. Watson recapitulates the chief propositions sought to be established.

1. Very many cases of bronchial asthma have their origin in laryngeal disease; that some remain for a variable period as a spasmodic affection of the glottidean muscles, and that in all cases of the disease in question, although the bronchi have long been affected, the chief contraction still occurs in the larynx.

2. If this contraction at the glottis be in any way overcome, that of the smaller bronchii either simultaneously or speedily relaxes.

3. The usual remedies employed in spasmodic asthma are either directed against the complications of the disease, and not against its proximate cause, or have been found in practice incapable of accomplishing its removal. The latter are therefore useless, and the former unfit to fulfil the indications referred to above.

4. This indication may be answered more or less perfectly in different cases, by the application of a solution of caustic of moderate strength (gr. xv. or ʒi to ʒi) to the glottis, which is the organ chiefly affected.

5. Cardiac asthma, as it is called, does not usually depend proximately on simple spasmodic contraction of the bronchial tubes, but rather on vesicular emphysema. Cases of this kind are therefore unfit for topical treatment.

6. Electricity passed in general currents, as much as possible along the bronchial tubes, may be found to diminish their contractility; and repeated small doses of strychnia may co-operate with the other means of treatment, probably by withdrawing the nervous energy to other parts at a distance from the affected air-tubes.—[*Assoc. Med. Jour.* *American Jour. Med. Science.*

Pathology of Inflammatory Gangrene. By J. H. BENNETT,
M. D., Prof., &c.

Occasionally, a very large amount of blood plasma is thrown out, constituting a violent inflammation; a greater or less number of capillaries are also ruptured, and blood-corpuscles are more or less mixed with the *liquor sanguinis* exuded. The exudation thus formed compresses the part so as to obstruct the bloodvessels, and prevent the continuance of any circulation in it. Under these circumstances, instead of forming a blastema for the production of new structures, it undergoes chemical changes, which induce in it decomposition, and the part is said to be mortified, or to be affected with moist gangrene. This change commences first in the blood extravasated, which becomes of a purple colour more or less deep; the corpuscles break down and become disintegrated; their hematozine dissolves and colours the serum; and, should the exudation have coagulated, it forms brown, rust-coloured, purple, or blackish masses. An acid matter is now formed, which, acting on the neighboring tissues, produces fetid gases that are abundantly given off from the affected part. Sulphuretted hydrogen is evolved, which causes the blackish sloughs usually observed in such cases, and discolours silver probes and the preparations of lead. After a time, the elementary tissues surrounding or involved in the exudation become more or less affected. The transverse striæ in the fasciculi of voluntary muscles become first pale, and are then obliterated. Cellular tissue, fat, and other soft substances lose their connection, and fall into an undefined granular mass. The tendons and fibrous tissue retain their characteristic structure for a long time after the other soft parts have been reduced to a softened pulp. The bones resist the action longest, but at length become rough, soft, and commencing externally, are more and more broken down, and reduced to the same pulpy consistence and granular structure as the surrounding parts.

As the tissues thus become broken down and fluid, they are discharged from the system in the form of an ichorous matter, which, examined microscopically, presents numerous granules, imperfect or broken-down cells, blood-corpuscles, and fragments of filamentous tissue or the other structures involved. If the morbid action be seated in the subcutaneous tissue, the skin soon becomes involved, and an opening is formed, which rapidly increases, and gives vent to the discharge. In a similar manner, gangrene of internal organs, by destroying the intermediate parts, at length enables the discharge to reach the surface, or to find its way into the excretory passages, such as

the bronchi, the intestinal canal, the meatus auditorius, &c. In this manner, life may be endangered by the destruction of organs necessary for its continuance, by the exhaustion resulting from the discharge, and sometimes by the absorption of the ichorous matter, which, on entering the circulation, acts as a poison to the economy.

It may be asked, whether inflammation and mortification are similar processes? whether the latter is only a greater intensity of the former? or whether, when mortification follows inflammation, it is dependent on other circumstances, such as a peculiar state of the atmosphere favouring the decomposition of the exudation poured out? In order to answer any of these questions, we must distinguish between mortification arising from a variety of circumstances, and an inflammatory gangrene, properly so called, the which is undoubtedly the rarest of all the terminations of inflammation. We frequently see mortification produced by the application of chemical or mechanical agents, which directly destroy the tissues. It also arises from severe and complicated injuries, in which arteries leading to the portions of structure affected have been divided or crushed. In old persons, it follows obstruction in the bloodvessels, or is dependent on circumstances not yet ascertained. In none of these cases is it caused by inflammation. But when stasis of the capillaries is produced to a considerable extent, followed by the exudation of a large quantity of blood-plasma, which instead of passing into organization, undergoes the changes previously described, then an inflammatory mortification, properly so called, is produced. We see this take place after burns, a long exposure to frost, and in certain cases of erysipelas. Here the amount of exudation is considerable, the pressure caused by its extreme, the obstruction to the circulation in the neighboring parts correspondingly great, and these, as well as the exudation itself, die. In this sense, therefore, it may be said to depend on the severity of the inflammation. This, however, is not the case in the sense of those who consider the adhesive, suppurative, and gangrenous inflammations as different stages of one process. Suppuration, as we now know, has no connection with adhesion; it is opposed to it; nor is it in any way related to mortification, which must be considered as a primary alteration of the exudation. The vitality is lost, and instead of passing into organization, it at once becomes subject to the chemical laws of dead matter, and undergoes putrefaction.

Now, in order that organic substances should enter rapidly into putrefaction, it is necessary that they find sufficient oxygen and water for all their carbon to be transformed into carbonic acid, all their hydrogen into water, and all their azote into am-

monia. When these conditions are not completely fulfilled, transition or intermediate substances are formed. When there is not sufficient oxygen, for instance, an excess of carbon is produced in the debris, and hence the black colour observed in mortified parts. There is also often developed a species of contagion, which causes parts undergoing decomposition to excite it in neighboring ones (*eremacausis* of Liebig.) This does not take place in dry gangrene. Thus, a gangrenous stomatitis (*Cancrum oris*) destroys, in a short time, a large portion of the soft parts of the lips and face; *noma* destroys the genitals of young female children. This appears to depend upon the quantity of destructive fluid or mixture generated in the process. A dry gangrenous foot on the other hand, often requires several weeks before it has produced sufficient decomposition to be detached, and reached all the tissues to the bone.

But there are sometimes external causes which seem to produce mortification, independent of the amount of exudation, or the rapidity with which it is thrown out. During the summer of 1836, I watched with great care the progress of a sloughing gangrene, prevalent, not only in the infirmary of Edinburgh, but throughout the city generally. All kinds of sores and wounds were affected by it, even those of a specific nature, such as chancres, &c. Neither youth nor age was exempted from it. It affected not only those who were debilitated from disease by intemperance or by diet, but those also in the most robust health. Thus, a servant-girl, aged 16, who had never suffered from illness, and of a robust constitution fell down upon some glass bottles, and slightly cut her left thumb. A week after, she entered the infirmary with an ulcer the size of a shilling, filled with a brownish-black slough, discharging a fetid and sanguinous fluid. In this, as well as other cases which occurred, it became impossible to attribute the gangrene to the violence of the injury, the amount of exudation, a state of cachexia, or indeed to any circumstances connected with the individual. It could not arise from contagion, as it originated simultaneously in different parts of the city in individuals who had no communication with each other, was not confined to the infirmary, and the system of dressing wounds there precludes the possibility of this explanation. We, are, therefore, compelled to ascribe the cause to something without.

Most writers have noticed the connection between a certain state of the atmosphere and the prevalence of hospital gangrene and of dysentery. Its more frequent occurrence in summer and autumn—that is, at a period of the year when increased temperature favours the decomposition of animal matter. The good effects which result from change of air, when every kind of

treatment fails, still farther point out its origin from changes occurring in the atmosphere. These probably depend upon some electrical state not yet explained, which powerfully influences the chemical combinations of the diseased part, and prevents cell growth. At least, such is what we may reasonably suppose from all the facts with which we are acquainted on this head. It is similar to blight among vegetables, the potato disease, and so on.—[*Amer. Jour. of Med. Sciences.*]

Analysis of Blood. A new Method of separating the Blood-Globules from the Coagulum. By BENJAMIN S. SHAW, M. D. of Boston.

Hitherto, a quantitative analysis of the blood has been very difficult when once this fluid has been coagulated, the principal difficulty being the perfect separation of the globules. Before coagulation, this can be pretty easily done by whipping out the fibrine, or by agitating the blood in a flask in which is suspended a piece of metal, around which the fibrine collects, and then filtering the serum which still retains the globules. But it is not always possible for the chemist to be present at the venesection for this purpose. The method I pursue is very simple, and is not, I think, generally known to chemists.

The blood when drawn, should be immediately poured into two vessels of the same size. I use two light glass tubes, weighing, with their ground-glass stoppers, each about an ounce, and capable of containing each an ounce of blood. The tubes should be completely filled, the stoppers introduced and allowed to rest twenty-four hours, so that the coagulation may be complete. One of these portions of the blood is used for the estimation of the globules, fibrine, and albumen, the other for the salts and the water. The great difficulty then is in separating the globules from the fibrine without rupturing them, so that they may afterwards be retained on a filter. To effect this, I have tried experiments with different kinds of linen and cotton cloths, of different degrees of fineness, and find that there is a kind of linen, rather fine than coarse, and yet the threads of which are not too closely woven, being about sixty to the inch, through which, when it has been well washed and soaked, or better still softened by wear, the globules can be pressed from the clot without injury, the fibrine being perfectly retained. A saturated solution of sulphate of soda should be prepared in an open vessel, the linen cut in a circular shape of sufficient size to hold the clot, and moistened with the solution before its introduction. The clot inclosed in the linen should then be immersed in the solution, and gradually pressed with the fingers till all the glo-

bules are removed, and the fingers and linen then washed with the solution to remove what few globules may adhere to them. The fibrine remaining on the cloth should then be washed, dried and weighed.

The globules, when examined with the microscope, will be found unruptured, and of natural appearance. These should then be filtered by a small filter previously weighed,) the filter moistened with the sulphate of soda solution, and only a small quantity of the globules be poured at a time upon the filter. The globules should then be washed upon the filter with a fresh solution of sulphate of soda, dried, and the filter spread out on the surface of a water-bath heated to 212° F. to coagulate the globules. The heat in the centre of the bath is not sufficient, and the filter should be well spread upon the bottom of the bath, immediately over the boiling water, as nothing less than a heat of 212° F. will suffice. When the globules are coagulated, the filter should be replaced in the funnel, and the globules carefully washed with distilled water, so as to remove all traces of the sulphate of soda, albumen, &c. This can be easily done without losing a single globule, if the globules have been fully coagulated. The filter is to be dried and weighed in a corked tube. The difference between this weight and the previous weight of the same filter, in the same tube, will give the exact weight of the globules.

The rest of the analysis is much like the common method. The solution of the sulphate of soda, after the separation of the globules, should be coagulated by heat with a few drops of acid, the coagulum washed with water, alcohol, and ether, dried and weighed for albumen.

The second portion of the blood, reserved for the estimation of the water and the salts, should be weighed, dried in a sand-bath, and again weighed, the loss being water. The residue should be burned in an open capsule carefully, for large quantities of carburetted hydrogen are generated, which puff up the mass, and render it liable to be lost over the sides of the vessel. When the gas ceases to escape, the calcination should be continued in a platinum crucible; but even here the perfect elimination of the carbon is very difficult.

A physician, furnished with two of these glass-stopped tubes or even with two common test tubes well corked, can fill them, and hand them to the chemist, without any necessity for previously defibrinating the blood. And by this method, also, the fibrine is perfectly separated, which is not always the case when the blood is beaten. The other ingredients of the blood, as the fatty matters, urea, seroline, &c., can also be estimated. Although this is rarely desired by the physician.—[*Ibid.*

Case of the Beneficial Effects of Strychnia in impaired Spinal Energy. By Dr. MARSHALL HALL, F.R.S., &c.

In remarking upon this case, Dr. Hall says:

Such cases occur from causes of nervous exhaustion, such as excessive study, muscular effort, sexual indulgence, &c.; and in such cases strychnia has appeared to me the appropriate and useful remedy.

This agent acts distinctly on the spinal marrow. In excess it induces spasmodic affection. It is therefore contra-indicated in cases of *irritation* of this nervous centre and of spasm. Its appropriate use is in spinal *exhaustion*. It constitutes one of our best tonics, improving the general health, and conducing to the recovery of strength and flesh.

I have given it in minute doses thrice a day, in the midst of meals, for many months. The following is the formula which I have adopted:—℞. Strychniæ acetatis, gr. i.; acidi acetosi, ℥xx.; alcoholis, ʒij.; aquæ distillatæ, ʒvi. M.

Of this, ten drops, containing one-fiftieth part of a grain, may be given thrice a day; but I have generally begun with five, and gone on to fifteen.

In two cases only have I known it to disagree. It seemed to affect the head. In many the patient has improved in looks, as in general health and strength, without experiencing any thing but good from it.

I am giving the strychnia a cautious trial in the epilepsy attended by pallor, thinness, and nervous exhaustion; in the paraplegia the result of sexual excesses, and in which neither pain nor spasm has occurred; and in the paralysis agitans.—*Lancet*, Nov. 27, 1852, from *Braithwaite's Retrospect*.

In another paper, Dr. Hall says:

I have been recently engaged in some experiments on the effects of strychnia and their remedies. I can only give a brief notice of them at the present moment; on a future occasion I will give the experiments themselves with their interesting details.

The effects of the acetate of strychnia show themselves under *two* forms or degrees, according to the dose of the poison in relation to the powers of the animal: these are—(1) the *milder*, and (2) the *severer*.

If a dog be placed under the milder form of strychnism, it passes into a condition of extreme spinal excitability. If, when in this state, it be continually excited, like the frog under a similar influence, it certainly dies; if, on the contrary, it be placed in a position of absolute quiet, it as certainly recovers—

facts which suggest our principle of the treatment of tetanus and of hydrophobia.

If the animal be placed under the severer form of strychnism, a different series of phenomena occurs. In the violence of the paroxysm, extreme laryngismus, extreme efforts at respiration, apoplexy, asphyxia, death, occur—unless one measure be adopted; that measure is—*tracheotomy*!

Let alone, the animal would infallibly *die*—of laryngismus; tracheotomy being performed, he lives!

But the patient affected with hydrophobia—and *all* hitherto so affected *have* died—have died of laryngismus. Now of laryngismus he *would not* die, if efficient tracheotomy were performed; would he, then, die?

I must add, in connexion with this last remark, that a dog, saved from the effects of laryngismus by tracheotomy, did afterwards die of exhaustion.

I repeat that all patients afflicted with hydrophobia have died hitherto; that all have died of laryngismus; that of laryngismus they need not die, and will not die, if tracheotomy be performed!—that is, no patient need die from the cause from which all hydrophobic patients have died hitherto.

If tracheotomy be performed, will the hydrophobic patient die? This question cannot be answered without an appeal to experiment. As the animal affected with the severer form of strychnism was saved from the first effects of the poison, yet died afterwards of exhaustion, the hydrophobic patient may die of ulterior effects of the poison. Even then, the terrors of this most terrific of diseases—the fits of strangulation and of suffocation—will be averted.

From the experiments to which I have adverted two practical inferences are deducible:—

1. Let the tetanic patient be preserved from all external excitement absolutely.

2. Let the hydrophobic patient, whilst equally preserved from excitement, be submitted to efficient tracheotomy.—*Lancet*, Feb. 5, 1853, from *Ibid*.

Chloroform in Obstruction of the Bowels from Spasms. By J. D. CAIN, M. D.

Every physician meets, in the course of his practice, with cases of obstruction of the intestines, which has come gradually or suddenly, generally from some cause of irritation existing in them. The obstruction in these cases consists of a spasmodic contraction of a portion, or of portions of the intestines, generally the small. The plan I formerly pursued was,

to cease all attempts at forcing a passage by means of cathartics, if one or two brisk cathartics failed, and to resort to opium freely, enemata of warm water, melted lard or butter, sweet oil, etc., the warm bath, fomentations of the abdomen, and other means of producing relaxation. For more than two years I have used chloroform, as a more powerful agent than opium, and its preparations, and as more certain in relaxing the muscular system. The chloroform administered in greater or less inhalation, soon produces a greater or less degree of resolution, and taking advantage of the relaxation thus effected, I give enemata, either stimulating, mucilaginous, or oily, which in a short time bring away fæcal matter. The inhalation may be repeated as often as in the judgment of the physician the case demands.

Chloroform possesses the immense advantage over opium, of relieving effectually and promptly the pain, and in not leaving the bowels in a constricted state, the sedative effect soon passing off.

Seven cases have thus been treated by me with highly satisfactory results. In one case only have I experienced any difficulty in inducing the requisite degree of relaxation of the bowels. The subject of this case was very slightly susceptible to its influence; but the pain was completely relieved by frequent inhalations, and the obstruction gradually overcome.

[*Charleston Med. Journal.*]

Inhalation of Chloroform in Pneumonia.

The late journals of Germany publish more than 200 cases of pneumonia treated by inhalations of chloroform. Far from being contra-indicated in pulmonary phlegmasia as had been thought up to the present time, chloroform on the contrary would seem, according to these facts, to modify favourably the inflammatory process of the lung. From among the observations published, out of 193 cases treated by Drs. Wachner, Baumgartner, and Schmit, only 9 died. Of 23 cases reported by Dr. Wawentrapp, of Frankfort, 19 were treated exclusively by chloroform, and only 1 died. Every two or three hours the patient is made to inhale the vapour from fifty drops of chloroform, during ten or fifteen minutes, so as never to let the effects reach to a loss of consciousness. All the patients were of adult age, and the disease upon an average had reached the fifth day. In every case it was observed that the chloroform had a diaphoretic effect, which was sometimes produced by the first inhalation, and never failed to manifest itself on the third or fourth day. It gradually diminished the local pain,

and caused it to disappear; it calmed the thoracic anxiety, brought back the respiration to its normal type, always appeased the cough, facilitated the expectoration in rendering it less abundant; and, lastly, it reduced the febrile reactions and induced refreshing sleep three or four days after the inhalations were commenced.—[*Gaz. des Hop.*, from *Trans. Med. Jour.*

Upon the Composition of the Semen in old men. By Dr. A. DUPLAY.

Bérard, Burdach, Muller, Longet, state, that spermatozoa are absent in the seminal secretion of the aged; Wagner alone asserts, that, in men of seventy to eighty years, these bodies are found in the vesiculæ seminales, if not in the testicle. Facts tend to prove that the fecundating power is retained up to the age of 100. The author investigated the semen in fifty-one subjects, after death caused by various diseases. In its physical properties the semen was sometimes of yellow color, as in the adult; generally of a paler straw hue, or almost colorless, milky or creamy. In twelve cases it was thick; but, in the greater number, it was serous, though mixed with the usual mucous secretion from the vesiculæ seminales. In most cases the vesiculæ seminales were full of fluid; in one only were they atrophied. The semen in the vasa deferentia was always darker-colored than that in the vesiculæ.

In thirty-seven cases the author remarked the presence of spermatozoa; in fourteen he failed in detecting them. In twenty-seven cases they were such as were usually seen in the healthy adult; in the rest the tail was shorter, and the head sometimes separated. Once he saw a number of crystals, whose nature he could not investigate. Considerable variety existed in their proportion.

In twenty-six cases, spermatozoa were found along the whole tract of the spermatic passages; three times only in the vas deferens; once only in the fluid of the vesiculæ seminales; seven times in the vesicula seminalis of the left side only. In the latter cases there existed mostly degenerations of the testicle, or obliteration of some part of the seminal duct. Secretion of semen continued, however, in cases where there was considerable atrophy of the testicle; the highest weight of which was 11·98 grammes; the lowest 4·50 grammes. The tissue of the testicle was always normal; the epididymis contained occasionally cysts. In four cases there was hydrocele. In the fourteen cases where there were no spermatozoa, the subjects had passed the age of seventy; but there were no

special changes discoverable in the glands. The secretion of semen, concludes Dr. Duplay, continues in old men, but in a less degree; their semen contains spermatozoa even when the person is beyond the age for fecundation. The cause, therefore, of want of power to effect this end cannot be explained upon the idea of the deficiency of spermatozoa.—[*Med. Times*, from *Archives Gén.*

Relationship of Glanders to Acute Diffuse Inflammation.

[The following remarks occur in the report of a case of glanders by J. T. Banks, M.D., M.R.I.A., King's Professor of the Practice of Physic, and Physician to the Whitworth and Hardwicke Hospitals; and we copy them as well worthy the attention of those who may be engaged in medico-philosophical investigations:]

“There are cases presenting all the phenomena of well-marked glanders, in which we fail, after the most searching investigation, to discover that the individuals so affected have ever come within the sphere of the poison. In illustration of this I may refer to the case of a policeman, who died in the Hardwicke Fever Hospital some time since. Of the many examples of glanders which I have seen, I never witnessed what appeared to me a more malignant and rapidly fatal form of the malady, pustules were scattered over the body, and surrounded by the *white areola*, a remarkable phenomenon, the presence of which, Dr. Hutton, I believe, first pointed out in the eruption of glanders. In this instance I noticed that the white areola became more distinctly visible after death than it had been before. The inability to trace the disease to communication with either horse or man laboring under glanders, led me to practise inoculation with the matter taken from one of the pustules, in a horse procured for the purpose. The result was the animal's becoming affected with acute glanders, and dying on the tenth day; the morbid appearances in each being absolutely identical. If, in default of evidence, we admit that the disease in the policeman was not the consequence of the absorption of the poison of glanders, then we have unequivocal grounds for stating that a poison generated in the human body is adequate to the production of a disease in the horse *not distinguishable* from glanders.

“My friend, Dr. Frazer, has published in the ‘Dublin Medical Press’ a most interesting paper, in which his object is to prove the identity of glanders and diffuse inflammation, he reports cases of diffuse inflammation, which he observed while acting as my clinical clerk in the Whitworth and Hard-

wicke Hospitals, in which there existed the eruption with the white areola, considered to be pathognomonic of true glanders. Dr. Frazer, after stating the points of analogy between the cases of diffuse inflammation adduced by him and undoubted cases of glanders, asks:—‘Are they not identical diseases?’ The question is still an open one. Would the matter taken from one of these cases have produced glanders? Perhaps it would. This is an inquiry of much interest, and one which I shall prosecute when an opportunity presents itself.”—[*Dublin Quarterly Journ. Ranking’s Abstract.*

Treatment of Rheumatism by Quinia and Veratria.

The treatment of acute Rheumatism by Quinine, commenced by M. Briquet, and to a certain extent discontinued, on account of the doubtful character of the termination of one or two cases in which large quantities were administered, has been very much revived of late, and the impression respecting it gains favor daily. I have seen M. B. give it with good effect quite often. M. Valleix employs and recommends it at La Pitié, and I hope to be able to send a paper on the subject by a gentleman who attends his *Services* and *Lectures*. One gramme (15 grs.) is administered once or twice a day, and the patient kept partly quininized. The gravity of the usual symptoms daily decreases, and it seems not to be at all hostile to cardiac complications. M. Trousseau, in commenting upon this and expressing his favorable impression, alluded also to the expenses attending the use of this agent, accompanying it at the same time with the relation of a remarkable instance of recovery from acute articular Rheumatism, which was then in his wards. It was true, he added, that it was as yet an isolated case, but the repetition of the means used might give equally favorable results. It followed the employment of Veratrine after the method recommended by M. Daniel (?). The patient, a woman under middle age, suffered intense pain in the articulations, high fever with endocarditis, exhibiting itself by a derangement of the first sound at the base of the heart—(sigmoid valves of aorta)—with a souffle prolonged into the large vessels, and accompanied with an alteration in the left ventricle also. In sixty-two hours she was absolutely cured, leaving not the slightest trace of febrile excitement, the presence of a slight souffle alone remains. One half of a milligramme (about 1-10th of a grain?) was given in the form of pill, each day increasing the quantity, and, when the pains were relieved, continuing it in the same dose every second day. Its influence on the circulation in this case was prodigious, to use M. Trousseau’s expression; the pulse fell to

42, and when I heard of the patient two days after, it was still at this reduced rate. Veratrine, we are aware, is one of the active principles of colchicum, and it is, therefore, not surprising that it should possess some power in acute rheumatism, more particularly in those dependent upon the arthritic diathesis, gout, etc.—[*Charleston Med. Jour.*]

Medical Properties of Ox Gall. Read before the Montgomery County Medical Society.

The broad and catholic grounds which we, as physicians, occupy, stimulates us to push our researches in every direction, in order to extend our area of available medical knowledge. In no way can our efforts be spent to better advantage, than that of trying to enlarge our list of efficient remedies. We are bound to use all means, whether moral or physical, which the indications of science or the test of experience point out as the most successful in the removal of disease. Of the physical means, we, as regular physicians, have the privilege of selecting any thing which the material world affords. We may use a substance of any form, whether aeriform, fluid, or solid, or from whatever kingdom of nature it may be derived, whether animal, vegetable, or mineral.

The article that we have selected, and concerning which we propose to make a few suggestions, is taken from the animal kingdom, and belongs undoubtedly to that class of substances termed *secretion*. Ox bile, which has been brought very prominently before the profession, by Dr. Charles Clay, of Manchester, and other eminent British physicians, some few years since, has not been, as we think, so fully tested in America, with regard to its medicinal properties, as its merits deserve. It was brought into use in the first place, from a suggestion that it might act as a substitute for the human bile, in the case of patients who seem to suffer from a deficiency, or perverted condition of that important secretion. In a country like ours, where perhaps three-fourths of all the disease with which we have to contend, is derived either directly or indirectly from a morbid condition of the liver, it would be well to pay the most strict attention to every remedial agent which may in the least promise to be serviceable in these cases. We do not expect that this article would meet successfully many of the symptoms growing out of the *retention of the bilious matter in the circulation*; but that it will remove that distressing condition of things caused by its *absence from the alimentary canal*, will scarcely admit of a doubt. In order to accomplish the first named object, it would necessarily have to eliminate the bilious

matter from the blood, or restore the healthy action of the hepatic system. This last, however, might be done by its acting as a healthy stimulus upon the mucous membrane of the duodenum, thus (according to a well known principle in medicine) indirectly exciting the liver through the medium of its excretory duct. For this reason we are of opinion that inspissated ox bile could be used in jaundice and other like bilious affections with benefit.

Much might be said concerning the office of the secretion of the liver in the system of man, but the destined limits of this paper will not permit. Suffice it to say that its absence from the alimentary canal is invariably connected with symptoms which render life a burden. From the character of these symptoms, if from no other circumstances, we are naturally led to infer that the bile, in the animal economy, acts more particularly as an antacid and solvent in the stomach and bowels. The most common effects of a deficiency in quantity or quality of the bilious secretion are dyspepsia, acidity of the stomach, and obstinate constipation. It seems quite evident that these conditions are owing more to the absence of a sufficient quantity of good bile in the alimentary canal, than to its retention in the circulation; and for this reason we infer that this peculiar substance is a *secretion*, intended for useful purposes, and not an *excretion*, merely, as is contended by some.

We then propose to treat those affections arising from deficiency either in quality or quantity of the bilious secretion, by administering that which will prove, as we think it will in many instances, a substitute for it. If we may believe the testimony of several distinguished medical philosophers and practitioners who have used the ox bile in dyspepsia, and in its almost infinite variety of attendants, we will be fully convinced of its great utility. It generally corrects the acidity of the stomach and consequent headache which so often harass dyspeptic persons. Its alkaline properties seem to counteract the acid, and thus remove all the symptoms depending upon it. Cases are mentioned, upon the highest authority, in which the patient, after resorting to every article in the list of cathartics, in order to remove the constipated habit so frequently attending dyspepsia, have received permanent relief from the use of the inspissated ox gall. It has been used in cases in which the blue pill was inadequate to the purpose of producing any but the most transient effect, and in which all purgatives would leave the bowels in the same if not a worse condition than that in which they found them. The patients were compelled to linger on for three or four years, being almost daily under the painful necessity of taking cathartics, which would produce much griping

and general uneasiness after taking them. Under these perplexing circumstances, the article to which we have called attention, has been administered in the form of a pill with the happiest effect. One of these pills (5 grains) has been given every three hours, and at these rates they scarcely ever fail to produce, in less than twenty-four hours, full stools of natural consistence, and that, too, without any pain whatever attending their operation. After taking one of these pills twice or thrice per day, for five or six days, the acidity almost always leaves the stomach, also the headache subsides, and the bowels resume their natural and healthy condition.

The first instance in which I have used the inspissated ox bile, was in the case of Mr. R—, aged 25 years, an intelligent young gentleman of my acquaintance, who had been laboring under dyspepsia, attended with slight hypochondria and the most obstinate constipation. He was of a delicate nervous temperament, and much given to sedentary habits. He was very much troubled with pains in his stomach and bowels, accompanied with dizziness in the head. The bowels would remain without a motion for a whole week, unless a purgative was taken, in which instance an unusually large dose was required, and the pain attending its operation was represented as being dreadful. The stool was hard and of a light or brownish color. I might mention, also, that there was a sense of weight in the right hypochondriac region, sometimes amounting to downright pain. He remained in this condition for several years, when he was compelled to give up his avocation, (school teaching,) and turn his whole attention to the malady which was preying upon his system. All the cathartics were tried, that seemed to promise any good in removing the costiveness, which was the most troublesome symptom in the case. He would use one article, (for instance, Rhubarb,) until the system lost its susceptibility of being acted upon by it, and then he would resort to another with the same result. Thus he continued up to the summer of 1849, when I commenced treating him with the inspissated ox bile. On the day that I gave him the first dose, he had had no motion of the bowels for four days. In the afternoon of that day he took two (10 grs.) pills and repeated in four hours. The last dose was shortly afterwards followed by a full, soft and *painless* stool, to the great joy an satisfaction of the patient. He continued to take a pill, night and morning, until the most complete regularity of the bowels was established. The pain of the stomach and bowels entirely subsided, and his general health became much improved. He is almost of opinion that the *Fel Bovinum* is a specific, and prepares it himself, to use as occasion may require. This

is only one of the many instances of the kind, which could be mentioned, in which I have used this article with the most satisfactory results.

I shall mention another case which is of a somewhat different class of patients, who have been signally benefitted by the remedy under consideration. Mrs. ———, aged about 22 years, light complexion and medium size—was a resident, up to the summer of last year, of the State of Mississippi. She is slightly hysterical, and has been for years affected with costiveness to such an extent as to be under the necessity of taking physic every few days. Large doses were required, which after a painful operation, left the bowels in their usual condition. She was somewhat chlorotic, and frequently suffered from what she called "fainting spells," which were very unpleasant indeed. After I had administered in this (as in the first mentioned) case, ordinary cathartics with but temporary effect, I resorted to the inspissated ox bile. I evaporated the gall to the consistence of thick tar, and then brought it to the consistence of pill mass by the addition of the precipitated carbonate of iron, one 5 gr. pill of which was given three times per day, with the effect of establishing perfect regularity of the bowels. I had one pill taken daily for two months, and we had the satisfaction of seeing the complexion of the patient assume a more ruddy and healthy appearance, the fainting fits reduced in frequency, and the strength improved. It would be well to state that, in this case, after about one month, the use of the inspissated gall and iron pills were suspended every other night in order to admit of the administration of a small dose of taraxicum, for its alterative and tonic effect.

This article is of undoubted utility in cases of children laboring under diarrhœa where the stools are light colored, indicating a lack of the normal quantity of bilious secretion in the system. The well known power of bile, whether human or not, of preserving milk from coagulation, has suggested the idea of using it in cases of infants upon whose stomachs the nurses milk curdles, producing vomiting and irritability of that organ. This practice is said to be very satisfactory by those who have ample opportunity of testing it in such cases. From the established fact, also, that a solution of gall, when poured even in very small quantities over hardened fæces which have been voided from the body, reduces it to a soft, pulpy consistence, it has led the inductive medical practitioner to use it as an injection where the rectum is filled with impacted fæces. I have used it with good effect in a marked case of this kind. We very plainly see that the mode of operation of this medicine is the same within as without the alimentary canal. Its contents

are softened, thus enabling the natural peristaltic action to propel them onward. We can in this way account for their *painless* operation, while those remedies which act merely by increasing the peristaltic motion, produce such great pain. It may be urged that this medicine will prove insufficient from the fact that constipation is but a *symptom* of an unhealthy condition behind it; but whilst this may be true, it will be well to recollect that when constipation is once established as a secondary effect, it has a baleful reaction upon the system, and we think that if this symptom be attended to, the first link in the chain of morbid action will often disappear of itself.

We are living in a period of the history of medicine, which is distinguished for the application that is made of a philosophical principle commonly known by the term of "induction." By it we understand, an inference or general conclusion drawn from the existence of one or more well established facts or propositions. The advantages which this furnishes to the modern physician, could not possibly have been appreciated by the older writers on medicine, unless they could have clearly recognized the principle, and seen it exemplified in practical life. This grand system of philosophy which was introduced by the immortal Bacon, was more readily received and more frequently used by *our* profession, probably, than any other. The enlightened medical philosopher is materially aided in his investigations of the essential character of disease, by the effects produced upon the system by therapeutical means, and from other circumstances, in the same manner as the mathematician and astronomer are aided in determining the size and distance of remote bodies, from the well known size, distance and position of those near them. This mode of investigation has been productive of more rich and valuable acquisitions to modern medical science than any which could be named. The various mechanical contrivances which have from this kind of reasoning been applied in surgery, and also the various actions of substances observable in the chemical laboratory, have furnished the greatest possible number of good practical hints to the surgeon and physician. The history of the article under consideration will serve sufficiently to illustrate this position. In the *first* place it was brought into use in cases of constipation growing out of the absence of the normal quantity of bile in the system, from the fact that its properties, both physical and vital, are similar to that of the human. In the *second* place, its use with beneficial effect, as an injection in cases of an impacted rectum, was indicated solely by its demonstrated power of dissolving hardened *fæces out of the body*; and, *thirdly*, its profitable uses in cases of nursing infants afflicted with

acidity and coagulation of milk upon the stomach, was naturally enough pointed out by its alkaline property and by its power of dissolving coagulated milk and preserving it in that state *out of the body*.

I will close this paper by merely expressing my ardent wish, that members of the profession will not forbear making an examination of the merits of this article, notwithstanding the apparent simplicity, for we should ever remember that the whole science of medicine is but a grand and systematic aggregate of great and *little things*.—[*Ohio Med. and Surg. Journ.*

The Employment of Digitaline in Spermatorrhœa. By M. LUCIEN CORVISART. Translated from the French.

M. L. Corvisart, having had the boldness to introduce the use of digitaline in spermatorrhœa, has already used it three times, and each time with success. Without pretending to claim from the observations already made, the conviction of practitioners, he thinks, and justly, as it seems to us, that this medicine deserves attention.

We have here the first case in which he thought he perceived benefit from the use of digitaline in seminal flux.

CASE I. P. M., aged 20 years, a stamper, entered the Hotel-Dieu the 1st of February, 1848, in the ward St. Agnes, under the care of Professor Chomel: appearance feeble, pale; observing during the week strict continence, but every Sunday for more than a year entering into the lowest debauchery and repeating coition five or six times, digestion bad, anorexia, palpitation of the heart, face bloated and hot, dizziness, tingling of the ears, occasional attacks of dyspnœa compelling the patient to walk his room during the night, disturbed dreams, and strange noises during partial sleep. The above symptoms had supervened since November last, at which time, after a debauch, he experienced a severe pain in the precordial region, characteristic of endo-carditis, for which he was subjected at the Hotel-Dieu to a strictly antiphlogistic treatment, (three bleedings, &c.) The patient, being unable to continue his labors, has now returned. The volume of the heart is normal; the blow of the point against the walls of the chest with a *bruit de souffle*, somewhat dry at the first sound of the heart. M. Chomel prescribed first 1, then 2 milligrammes of digitaline.

The sixth day of the treatment, the palpitations having very much diminished, the patient mentioned that since November he had almost every night involuntary pollutions, often leaving no trace in his memory—they were known only by the stains

on his linen. When awakened by the pollutions, the penis was found to be in erection, as was also the case each morning.

Pollutions take place almost every day, under the influence of erotic ideas, or the simple friction of the clothes, with erection and ejaculation, but without lively pleasure; they sometimes occur during defecation.

The patient, for three months, has not seen a woman.

He is astonished that the emissions have not occurred for the last three days, (now the ninth since entering the hospital, and the third of the treatment of the palpitations by digitaline.)

Feb. 13th, one pollution without any remembrance of it; the 14th, one on waking: the 15th, one on waking, very slight, (one or two drops); 3 milligrammes were ordered; no spermatorrhæa from the 16th to the 19th; then seven days entire without a single pollution, although each day the patient examined his shirt and drawers with the greatest care; this was also done several times by myself.

March 7th. One emission on waking.

March 12th. Discharged, no pollution since the last date.

After thirty-eight days the patient had had but five pollutions, and for twenty-two days but one; he was still troubled with erections, although less than formerly. The palpitations were very much diminished.

CASE II. L. H., aged 18 years, sculptor, having never indulged in venereal excesses, was taken when between fifteen and sixteen years with nocturnal emission without pleasurable sensations. At first these accidents occurred almost every night from his sixteenth to his seventeenth year.

The young patient, very much affected, bound the penis with pack thread, bathed the parts in ice-cold water, avoided every occasion of provoking erotic ideas, and lived in the most absolute continence.

About the month of December, 1847, he commenced the practise of masturbation two or three times a week. During the two months in which he indulged in this habit, the nocturnal pollutions disappeared almost entirely; nevertheless the symptoms, which had already begun to manifest themselves, increased. The strength was diminished, the spirits depressed, the memory short, the patient became very sensible to cold, digestion slow and painful, the appetite greedy. About the end of February, 1848, the patient abandoned his practices and renounced all venereal pleasures. A few days after the nocturnal emissions returned as frequently as before. The erections when the patient was awakened, (which was rarely the case,) were but slight; they were not always accompanied by erotic ideas, but sometimes even the reverse.

L. H. remarked that for some weeks, about the close of micturition, the urine was loaded with a white albuminous cloud, which sunk to the bottom of the vessel, and was more abundant in the morning after fatigue.

The patient experienced pain along the spinal column, shooting into the epigastric region, and a general, although light, habitual trembling with stammering.

The patient had used, without success, camphor, internally and externally; he thought the erections more numerous, and the emissions more frequent. M. Huguier had prescribed, about the end of March, bitter drinks, the syrup of gentian, exercise, &c., the pollutions diminished during one or two weeks and then returned.

On the 13th of April the patient came to consult M. Huguier, who at my request had the goodness to prescribe the digitaline, 3 milligrammes a day. Each of the three following days the pollutions persisted, then an interval of six days without any. The 23d of April he returned to know whether he should continue the use of the digitaline. M. Huguier admitted him to his wards in the hospital Beaujon, in order the better to observe the case. The seventeenth day of the treatment, which was continued, he had one pollution, that was all. The patient was discharged the 15th of May, having had in thirty-four days but five pollutions, of which the two last were separated by an interval of twenty-two days. There was no seminal discharge in the urine, that fluid being scarcely troubled and containing no spermatic animalcula. He had neither trembling nor stammering; he was free from pain along the spine; his strength improving, and his digestion good.

The third case is too lengthy to present entire, but it is not less conclusive than the others.

How shall the action of digitaline be explained? The first patient appears to be an example of spermatorrhæa from atony of the genital organs; this was the case also in the third, but certainly not in the second, who appears to have possessed remarkable temperance. In two of the cases the erections were light, not insupportable. It is not by calming these, then, that the remedy acts. So far, its administration is purely empirical, but this is not sufficient reason for disregarding it.—[*North-Western Med. and Surg. Jour.*]

Treatment of Spermatorrhæa.

With the difficulties connected with the treatment of obstinate cases of this malady, most practitioners are familiar. Books without number have been written on the subject; and almost every system of medicine proposes remedies, many of

which, on trial, are found of no value whatever. It is hopeless to undertake to interrupt by medication the repetitions of the misfortune. There are but two methods, we believe, decidedly reliable; one of them is mechanical, the other is left to the ingenuity of the reader to ascertain. For several years past, some of the very worst forms in which the disease presents itself, have been terminated in a short time, and the sufferer restored to permanent health, by a mechanical contrivance, which originated, it is believed, in Boston. The way to proceed is this: Take a piece of firm harness leather one inch wide, and make a ring, or ferrule, which shall be one eighth of an inch greater in diameter than the penis. Thrust the points of four pins, equi-distant from each other, through the walls of the ring, so that they will project through a little way on the inside, and then cut off the projecting part of the pins on the outside. On retiring for the night, slip the ring on the organ, midway, and insert cotton wool between the two, to keep the pins from pricking the flesh. An emission seldom occurs without a full distension of the penis. The theory of a cure, as well as the facts, are simply these. When an erection takes place, and even before, the uniform enlargement presses the cotton, which yields, causing the points of the pins to enter the flesh, and thus the patient is instantly awakened. This occurs as frequently as distension comes on, and the semen is therefore retained. This, we repeat, is superior to any and all other prescriptions made use of. Last week an instrument was left on sale at Dr. Cheever's, under the Tremont Temple, in this city, that acts precisely like the leather ring. It is made of steel, however, clasping like a dog's collar, according to the size required, and having on its inner edge a row of sharp points. Within this steel ring is another, extremely delicate, which opens to receive the penis, and retains it exactly in the middle. When it begins to distend, the small ring allows the member to enlarge till it strikes the sharp points, and then the individual is awake and safe. After interrupting the emission a few times in this way, the morbid tendency in many cases is removed, and the sickly, feeble youth rallies and regains his health. Other cases may require a more constant use of the remedy, until maturer age and different circumstance render it no longer necessary.

[*Boston Med. and Surg. Journal.*]

Surgical Operations in Cancerous Diseases.

[At the last meeting of the American Medical Association, Dr. Yandell, of Kentucky, presented a report from Dr. S. D. Gross, of the same State, on the results of surgical operations

for the relief of malignant diseases. The following is the concluding portion of it.]

From the facts and statements which have now been presented, embracing the opinions of many of the most intelligent, experienced and distinguished practitioners in different ages, and in different parts of the world, the following conclusions may be legitimately deduced :

First.—That cancerous affections, particularly those of the mammary gland, have always, with a few rare exceptions, been regarded by practitioners as incurable by the knife and escharotics. This opinion, commencing with Hippocrates, the father of medicine, has prevailed from the earliest records of the profession, to the present moment. Nature never cures a disease of the kind ; nor can this be effected by any medicine, or internal remedies, known to the profession.

Secondly.—That excision, however early and thoroughly executed, is nearly always, in genuine cancer, followed by relapse, at a period varying from a few weeks to several months, from the time of the operation.

Thirdly.—That nearly all practitioners, from the time of Hippocrates to the present day, have been, and are still averse to any operation for the removal of cancerous tumors, after the establishment of ulceration, rapid growth, firm adhesion, organic change in the skin, lymphatic invasion, the cancerous dyscrasy, or serious constitutional derangements ; on the ground that if had recourse to under these circumstances, the malady almost inevitably recurs in a very short time, and frequently destroys the patient more rapidly than when it is permitted to pursue its own course.

Fourthly.—That in all cases of *acute* carcinoma, or, in other words, in all cases of this disease, attended with very rapid development and great bulk of the tumor, extirpation is improper and unjustifiable, inasmuch as it will only tend to expedite the fatal result, which, under such circumstances, always takes place in a very short time.

Fifthly.—That all operations performed for the removal of encephaloid cancer and its different varieties, are more certainly followed by rapid relapse than operations performed upon scirrhus or hard cancer.

Sixthly.—That in nearly all the operations for cancerous diseases, hitherto reported, the history has been imperfectly presented, being deficient in the details which are necessary to a complete and thorough understanding of the subject in each case. This remark is particularly true in reference to the diagnosis of the malady, the minute examination of the morbid structure, and the history of the case after the operation, as to the

period of relapse, the time and nature of the patient's death, and the result of the post-mortem examination.

Seventhly.—That cancerous affections of the lip and skin, now usually described under the name of cancrroid diseases, are less liable to relapse after extirpation than genuine cancerous maladies, or those which are characterized by the existence of the true cancer-cell and cancer-juice.

Eighthly.—That although practitioners have always been aware, from the earliest professional records, of the great liability of cancer to relapse after extirpation, a great majority of them have always been, and still are, in favor of operation in the early stage of the disease, especially in scirrhus, before the tumor has made much progress, or before there is any disease of the lymphatic ganglions, or evidence of the cancerous cachexy.

Ninthly.—That many cases of tumors, especially tumors of the breast and testicle, supposed to be cancerous, are in reality not cancerous, but of a benign character, and consequently, readily curable by ablation, whether effected by the knife or by escharotics. It is to this circumstance that we must ascribe the astonishing success which is said to have attended the practice of Hill of Scotland, Nooth of England, and Flajani of Italy.

Tenthly.—That all operators insist upon the most thorough excision possible; removing not merely the diseased mass, but also a portion of the surrounding and apparently healthy tissues, as well as all enlarged and indurated ganglions.

Eleventhly.—That the practice has always prevailed and still obtains to save if possible, a sufficient amount of healthy integument to cover the wound, and to unite, if possible, the wound by the first intention; on the ground that these precautions will tend much to retard, if not to prevent, a recurrence of the disease.

Twelfthly.—That much stress is laid by writers upon a properly regulated diet, and attention to the bowels and secretions after operation, as means of retarding and preventing relapse.

Thirteenthly.—That there is no remedy, medicine or method of treatment which has the power, so far as we are enabled to judge of its virtues, of preventing the reproduction of the morbid action after operation, no matter how early or how thoroughly it may be performed.

Fourteenthly.—That life has occasionally been prolonged and even saved by operation after relapse, as in some of the remarkable cases mentioned in a previous part of this report; but that, as a general rule, such a procedure is as incompetent to effect a permanent cure as a first extirpation.—[*Ibid.*

Hemorrhage from the Funis. By WALTER O'REILLY, M. D.

Having occasion, not long since, to attend a parturient case on the west side of the City, after detaching the fœtus, securing the cord, bandaging the woman, abstracting the placenta, and, as is my custom, taking a look at the child before leaving, to see if all was right, found the under clothes saturated with blood. On investigation I was surprised to find the blood emanating from two small orifices in the funis not larger than pin holes, within the eighth of an inch of each other, and about twice that length from the abdominal surface; the cord having been previously ligatured for security in two places, I was obliged to apply a third one below the points of hemorrhage bordering on the umbilicus, which stopped all further flow, and the separation took place without any difficulty or trouble, within the week.

It struck me while dividing the communication between the mother and child, that it was rather shorter than usual, but did not think more about it until searching for the cause. I was inclined to think, perhaps, in removing the child to apply the ligature, the funis might have been stretched, producing a break in its coats. I also noticed in extracting the placenta that it was inclined to give way like a rotten sponge, making it necessary to abstract it with the palm of the hand to prevent any being left; an hour-glass contraction and atony preventing its delivery without interference.

Had I not discovered the bleeding in time, the child might have bled to death; and, of course, would have been charged to the Doctor's carelessness in tying the string.—[*New York Med. Gazette.*]

Antiperiodic Properties of the Humulus Lupulus. By W. Y. GADBERRY, M. D. of Benton, Miss.

As a substitute for quinine is a great desideratum on account of its enhanced market value, I have thought a brief notice of the antiperiodic virtues of the humulus lupulus, or common hop, might not be unacceptable to the profession. I am not aware that any author has ascribed to this plant any such virtue. Having used it for nearly two years I can confidently state that its antiperiodic properties equal, if they do not exceed, those of any other article of the materia medica with which we are acquainted, quinine alone excepted; and, indeed, in my experience, it has often succeeded in arresting intermittents after that remedy had failed. It is harmless in its effects, and will often be borne by patients who cannot take quinine.

Every practitioner is aware of the advantage of combining an anodyne with antiperiodics; and by reference to the works on materia medica, the reader will see that hops possess these properties. When administered alone the infusion is preferable, and should be made of double the strength prescribed by the Dispensatory. One ounce infused in a pint of boiling water may be taken during the interval, or a larger quantity if necessary. If the secretions are properly regulated, and there exists no enlargement of the spleen, it will rarely fail to effect a cure of tertian or quartan ague. It has not succeeded so well in the cases of quotidian type as in those of more protracted intervals. The tincture was used alone in three cases successfully. The following combination is worthy a trial by all who desire a safe and efficient substitute for quinine:

R Tinct. hops, tinct. Peruvian bark, aa \mathfrak{z} iv.

Pulv. black pepper, \mathfrak{z} ss.

To be given in doses of half an ounce every two hours during the interval.

My limited experience will not justify an opinion upon the antiperiodic virtue of lupuline, not having used it except in combination with quinine. Patients to whom I have administered this combination prefer it to quinine alone, on account of its soothing effect upon the nervous system. The hop is indigenous to this country, growing abundantly in almost every garden; and if I have not over estimated its antiperiodic virtue, it will prove a blessing to the poor, in whose welfare the physician should always feel a special interest.—[*West. Journ. Med. and Surg.*]

Cold Water in Dysentery. By F. BLADES, M. D.

If it be the accumulated experience of individuals which gives us our *rules* in the practice of medicine, every one ought to contribute his mite, if it be of any value. I am therefore prompted to send you a slice of my experience.

Last year I had many cases of dysentery to treat. Some of these "wore the livery" of the ordinary non-malignant variety, and were amenable to the usual remedial means; while others, the majority, were of the epidemic or malignant variety and with surpassing stubbornness "went their ways" heedless of cure, i. e., by the mostly practised methods.

Now, we, who have not a reputation to live on after a defeat, cannot well afford—if I may use a sinister expression—to lose many patients consecutively, else we fall into disrepute and strait-away lose our practice.

This motive, which was secondary to the heart-felt interest

I had in the recovery of my patients, as also this latter motive, caused me to depart from the calomel and opium, etc., etc., *land marks* in treating the more malignant variety of dysentery. I have now in my mind a case, which conjointly with Dr. Fowler, then my partner, I was called upon to treat. The malady "waxed exceedingly sore" from its onset. The gripping was positively excruciating; the straining extremely ardent and incessant; the stools exceedingly large, grayish and bloody, containing membranous-like shreds; the pulse was quite frequent and not forcible. This is a rudely sketched outline of the condition of the case as was reported to me to have existed prior to my attendance. The doctor who was first called had treated the case with calomel and opium, q. s., castor oil and laudanum, as a laxative, once in twenty-four hours, with other *adjuvantia* now passed from memory, for three or four days, at which time I was called to see this case with him. The above mentioned symptoms were said to be unabated. The pulse was now feeble and about 120; the tongue was covered with a thick brown fur, and dry, the edges were fiery and the whole tongue was dotted over with elevated papillæ—here and there protruding through the fur-coat. The stomach was so excessively irritable that it would scarcely retain a tea-spoonful of water. I suggested an enema consisting of a *strong* solution of nitrate of silver, which was twice or thrice repeated during the ensuing twenty-four hours. Also camphor spts. and oil of turpentine, equal parts, to be applied almost hot, to the abdomen. It was of no use. The disease increased in severity. We looked upon the mortal issue as being but a few hours in advance of us. Here was our extremity, and *cold water* was the straw caught at. What miraculous buoyancy there was in that dernier resort! We left off medicine entirely—little use was it when none would be retained by the stomach—and determined to try cold water. We wrapt the patient in a cold wet sheet and thereupon—having previously passed a stool every ten or fifteen minutes—he lay one hour and a half, without having desire to go to stool. At the end of this time the surface almost glowed with warmth, and there was the moisture of sweat about the face and neck. The patient was then wiped dry with coarse towels and placed in a dry bed. This operation was thenceforward repeated every five or six hours for the next five days, after which time it was only used once or twice in twenty-four hours for two or three days longer. Instead of the warm fomentations, which had been constantly applied, cloths wrung out of cold water were frequently repeated to the abdomen. As enemas we used cold water, simply—8 or 10 ounces immediately after every

evacuation. In every case in the treatment of which we used cold water injections it was found to be important that it should be administered immediately subsequent to every stool. They were borne without distress, and much longer. I ought, also, to mention that after the first day we used the cold Sitz-bath of the Hydropathists. From the commencement of this treatment the irritability of the stomach was entirely appeased; the stools became less and less in frequency and of a more natural appearance and consistence. As a diet, as well as an auxiliary to the treatment, we ordered the animal broths well salted.

Several other cases I have in my mind of a like character with the above. With the exception of one, however, none of them were so violently attacked. That case being of a more robust habit the disease did not succumb so readily. I commenced treating with calomel, ipecac., and *one grain* of morphine every three hours—at the end of twenty-four hours giving a castor oil laxative; warm fomentations to the abdomen; enemas of cold water and laudanum. This course was kept up with more or less modification until the expiration of a week. I was not flattered by the progress my patient had made for the better. I then resorted to the water treatment—carrying it out as in the first instance. Upon the first using of the wet sheet the bowels were quieted two hours, having been previously moved as often as from 15 to 30 minutes. The patient kept right on improving—steadily, yet, I confess, slowly. It was gratifying to see the complete relief from the excruciating tormina and tenesmus which followed the “wet-sheet-packing.”

In this case I used, as often as once in four hours, the turpentine emulsion, strongly charged with laudanum. I also, occasionally, ordered laudanum in the injections.

In many cases, the cold, wet bandage and cold water injections were used as auxiliaries to other treatment, and with a highly gratifying effect.

I am so thoroughly convinced of the powerful efficacy of cold water in the treatment of dysentery that I do not hesitate to say I regard it as *one* of the chief remedies for combating that formidable disease.

Dr. Bennett, of this place, a practitioner of many years standing, and a correct observer, after being repeatedly disappointed by depending upon the ordinary remedies alone, is, upon fair trial in many instances, enthusiastic in his confidence in cold water as a powerful auxiliary in treating dysentery.

It would be absurd to argue a general rule from such limited experience, yet its effects have been so highly gratifying in the

hands of many practitioners, that it is hard to resist the conviction that cold water deserves a more honorable place among the therapia of dysentery than it has hitherto obtained.—[*North Western Med. and Surg. Journal.*]

Baths and Cold Water.

M. Pinel (nephew) has read a memoir on the treatment of acute insanity, by prolonged baths, and by pouring cold water on the head. The following are the results:

We have treated 157 cases, classified as follows:—Maniacal Delirium, 57; Lypemania, 38; Delirium Aithers Melancholy, 20; Suicidal Delirium, 21; Delirium Tremens, 16; Entomania, 5—Total, 157. Males, 91; Females, 66. The moral causes far exceeded the physical; 122 of the first; and 39 the last; 37 cases were hereditary; 79 were unmarried, (50 males and 29 females); 66 were married, 12 were widowers; the mean age was about 32 years; *one hundred and twenty-five were cured*; 4 died; 7 improved; 21 remaining under treatment. Of those cured, the mean continuance of the disease had been about two and a half months. There had been 25 relapses; but half of these had been treated again with success.—[*Jour. de Med. et de Chirurgie. Am. Jour. of Insanity.*]

Oil of Turpentine as a Dressing for the Umbilical Cord—its prophylactic virtues against Trismus Nascentium. By T. J. GRAFTON, M. D., of Miss.

Dr. Hester—I beg leave, through your journal, to call attention to the use of the Oil of Turpentine as a dressing to the umbilicus of new born children, as a prophylactic against Trismus Nascentium, a disease so universally fatal when once developed, as almost to preclude a hope of cure from any treatment.

So far as my experience enables me to speak, I have never known the disease to occur, in any child that had been subjected to the turpentine dressing. With whom the practice originated I have not been able to learn; my attention was directed to it several years ago by a very intelligent lady, who had used it upon her negroes, and at whose suggestion it has been adopted by others in the neighborhood, with the most flattering results.

This should not surprise us, as turpentine has been long considered by some as a prophylactic in Traumatic Tetanus. *Dr. Armstrong*, in his Lectures upon the Practice of Medicine, says: "Mr. Stewart, who has seen a great many cases of Tetanus, never knew a case arise where turpentine had been applied

to the local injury;" (page 337, 1st American edition, 2d vol.) My own experience, so far, is corroborative of the above extract.

I am led to make this communication with a view of multiplying the number of observers, some of whom will doubtless make known to the profession the results, as the opportunities of no single individual would be sufficient to establish it as a fact.

I am well aware, that turpentine has been used as a stimulating application to the umbilicus, as a means of cure after the disease was developed, yet I do not remember to have seen it mentioned by any writer as a prophylactic.

In no case of the disease have I been able to detect the displacement of the cranial bones, as suggested by Dr. Sims. My own belief is that the umbilicus is the source.

The mode of using the turpentine is as follows:—At the first dressing a few drops of the undiluted turpentine is applied immediately to the umbilicus, around the cord, and it is anointed at every succeeding dressing, the turpentine being diluted one-half, or two-thirds, with olive oil, lard, or fresh butter, which dressing is continued more or less diluted, according to circumstances, (or rather according to the irritation produced,) until healed; though I usually make use of no precaution to prevent contact with the skin of the abdomen, yet I have never known distress follow its use; it would be well enough to prevent too extensive contact.—[*N. O. Med. and Surg. Jour.*

An Inquiry into the Action of the Anthelmintics. By Dr. KÜCHENMEISTER, of Zittau.

Dr. Küchenmeister has examined the various vermifuges, by immersing the living intestinal worms of fowls, cats and dogs, in albumen, at a temperature exceeding 77° Fahr., and adding the anthelmintics in the form of infusion or of powder. In some cases, a mixture of warm milk and water was substituted for the albumen. The experiments were not continued for more than from forty to forty-eight hours, if the worm had not been killed before the expiration of that time. Dr. Küchenmeister made use of electricity as the most delicate reagent for proving the occurrence of the death of the worms. In the first place, electricity cannot be considered as a vermifuge. The author subjected a female *Heterakis vesicularis*, taken from a partridge that had been killed, to the action of a rotary apparatus, which was kept up with longer or shorter intervals during an entire day. The animal was not destroyed by the experiment. He next tried the remedies employed for the re-

moval of *tæniæ*, and first tested kousso in the following manner:—A living *Tænia crassicolis*, procured from a cat, was placed at four o'clock in a mixture of albumen and *dolichos pruriens*. The worm appeared to be perfectly well in this mixture, and at two o'clock on the following afternoon exhibited the most vigorous movements. The *tænia* was now transferred to a vessel containing a mixture of infusion of kousso and some of the infused as well as some of the fresh powder with albumen. The temperature of the mixture was 30° R. (99.5° F.) On its introduction, the worm quickly extended itself; after some time it was found to be dead, its colour having changed to a dirty reddish yellow. Two *Tæniæ serratæ* were placed at about half-past one in the afternoon in a mixture of albumen and kousso; at two o'clock they were dying, and at three completely dead. Two *Tæniæ serratæ* from the same dog were brought in contact with kousso and milk, at half-past one in the afternoon, and at two o'clock were dead. Two *Tæniæ serratæ* were placed at half-past one in the afternoon in albumen, mixed with decoction of pomegranate root and with some of the powdered root: they died in three hours. Two others were placed in milk mixed with the decoction only: they died in three and a half hours. A *Tænia crassicolis* was put into a mixture of albumen with ethereal extract of male fern: it died gradually in three hours and three quarters. A number of *Tæniæ cucumerinæ* were placed in a mixture of albumen and oil of turpentine: they were dead in an hour and a quarter.

A number of the same were put into a mixture of albumen and castor oil; they appeared lively at first, but were dead in seven hours. Similar worms were put into a salad, composed of pieces of unwatered herring, boiled potatoes, large pieces of onion and garlic, albumen, vinegar, and a large quantity of oil. They died in eight hours. Lastly, the author tested the vermifuge powers of the brown oxide of copper; fifteen grains were administered in the course of four days to a strong cat. When the body was opened, the entire intestinal canal was found to be full of fluid, yellow, flaky *fæces*; the intestine was softened, and denuded of epithelium, especially at the termination of the ileum, where the adjoining Peyer's glands were much swollen, particularly in two situations, one of which was an inch and a half long by one-third of an inch broad; the other was nearly circular, and its diameter one-third of an inch. The cat had been purged. The *tæniæ* and *ascarides* it contained were lively. It would hence appear that this substance is both inefficacious as a vermifuge and dangerous to the system. The following table contains the results of the above experiments:—

In milk boiled with kousso *tæniæ* died in half an hour.

In a mixture of oil of turpentine and albumen, in 1 to 1½ hour.

In decoction of kousso with albumen, . . . 1½ to 3 hours.

In decoction of pomegranate root with milk, . . 3 to 3½ "

In decoction of pomegranate root with albumen, . 3 "

In ethereal extract of male fern with albumen, . 3½ to 4 "

In castor oil with albumen, 8 "

In salmagundi with garlic and onions, . . . 8 "

Kousso would therefore seem to be the most efficacious remedy against *tæniæ*. When pomegranate bark and male fern root fail, their failure may be owing to the habit of administering a laxative in from four to six hours after the exhibition of the vermifuge, by which the latter may be carried beyond the worm. With regard to pomegranate root, it must be observed, that in large doses it occasions diarrhœa. The same remark applies to castor oil. The author also alludes to cold water, strawberries, *dolichos pruriens*, and filings of tin. When *tæniæ* are placed in water containing ice, they are instantly benumbed, and if allowed to remain in it, they will always be found at the end of ten hours to be quite dead. Strawberries may be useful as a mild remedy in cases of tape-worm; if large quantities of them be taken on an empty stomach, entire portions of the worm will often be passed. *Dolichos pruriens*, with which the author tried many experiments, appears to possess no power of destroying worms. The author has also minutely studied the medicines recommended for the removal of round worms. In albumen, these worms behave as the *tæniæ*; in water, at about 77° F., they live for some days, but swell, stiffen, become longer, thicker, and more sluggish; they lose their power of suction, and their motions become slow and only partial—they resemble leeches which have gorged themselves. In general, however, the males and young neutrals resist the effects of water longer than the mature, impregnated, egg-bearing females, which become quite rigid and inflexible, and swell considerably. Milk and whey affect the worms like water. The following are the medicines, the effects of which were tested:

1. Camphor. An *ascaris* lived from eighteen to twenty hours in albumen into which some camphor had been introduced. 2. A mixture of oil of turpentine and albumen killed some *ascarides* which were placed in it from two and a half to six hours. 3. *Ascarides* lived forty hours in albumen and wormseed, whether the latter was employed in the form of powder or infusion. 4. Some *ascarides* were placed in albumen mixed with *santonine*; they did not die in it, nor did they die in a watery infusion of *santonine*. When *santonine* was

dissolved in oil, especially in castor oil, and mixed with albumen and ascarides, the latter died in ten minutes. An injection of santonine and castor oil was thrown up the rectum of a cat, and produced numerous motions containing dead worms; and on killing the cat, the entire of the lower portion of the intestinal canal was ascertained to be free from worms, while four were found near the stomach quite rigid and extended, and retaining but little life. A *Tænia crassicolis*, however, was found in the intestines, and appeared to be quite uninjured and very lively. 5. A mixture of albumen and aniseed, with a strong infusion of the latter, killed the worms in about twenty-four hours. 6. Parsley, mixed with albumen, killed ascarides very slowly. 7. Flour of mustard and albumen destroyed them in about four hours. 8. In rue the worms lived upwards of twenty-four hours. 9. The same was the case with millefoil. In contact with tansy, valerian and camomile, great numbers of them lived for twenty-four hours. With onions and garlic they perished in from ten to fifteen hours. A decoction of cloves, with or without albumen, killed them in twelve hours. In an infusion of ginger, with or without albumen, they lived about twenty-four hours. Petroleum, mixed with albumen, killed them in less than six hours, as did also oil of cajeput and albumen.

A series of vermifuges, taken from the class of balsamics, was tried in like manner, namely, assafetida, ammoniacum, balsam of Peru, extract of juniper, and Venice turpentine. In all these the worms lived more than twenty-four hours. Of the class of empyreumatics (brenzlichen stoffe) the following were tried:—Oleum chaberti, [a mixture of four parts of oil of turpentine, and one of the animal oil of Dippel,] oil of amber, castor oil, tar water, creasote, wood-vinegar, and wood-soot. In these, for the most part, the worms lived from twenty-four to forty-eight hours; except the wood-vinegar; in which they lived rather more than twelve; and creasote, in which they died within two hours. Of bitters, the author tried aloes, gamboge, ox-gall, worm-wood, myrrh, gentian, quassia, hops, bitter orange, and acorus calamus; in all these the ascarides lived from twenty-four to forty hours. Of astringents, pure tannic acid, pomegranate root, kousso, extract of walnuts, cinchona bark and quina, elm bark, willow bark, the flowers and stalks of meadow sweet, oak bark, dragon's blood, catechu and kino. In these the worms died in from twenty-four to thirty hours, with but two exceptions, namely, tincture of galls and pomegranate root, both of which killed them in the space of eleven hours. Of saline preparations, sulphate of soda, chloride of sodium, and the roe of the herring, were tried. In the first the

worms died in from fifteen to eighteen hours ; in the second, in from two to six ; and in the roe of the herring, in four hours. The following metallic poisons were experimented on :—Arsenic, calomel, corrosive sublimate, and the salts of tin, of lead, and of copper. Corrosive sublimate alone destroyed the worms in so short a time as two hours ; all the other metallic salts required a much longer period. From these experiments it would appear that santonine, mixed with oil, is the most powerful vermifuge, then chloride of sodium, the roe of the herring, garlic, onions, &c. The author advises that santonine should be given as a vermifuge ; mixed with oil, in the proportion of from two to five grains to an ounce of castor oil. This solution should be given in the doses of a teaspoonful until the effect is produced. As auxiliary treatment, chloride of sodium, herring brine, mustard, onions and garlic, may be employed.—[*Foriep's Tagsberichte über die Fortschritte der Naturund Heilkunde. Pharmakologie, Band 1, p. 317. Dublin Quarterly Journal.*

On the Influence of Parasites in the Production of Disease.

By JOSEPH LEIDY, M. D., Professor of Anatomy in the University of Pennsylvania.

In many animals entozoa and entophyta are almost never absent, and probably when in their natural habitation, and few in number, or not of excessive size, are harmless, as observed by Dujardin in the introduction of his excellent work on Intestinal worms : “ Les helminthes se développent dans un site qui leur convient, sans nuire plus que les lichens sur l'écorce d'un arbre vigoureux. Ils ne peuvent devenir nuisibles, généralement, que par suite d'une multiplication excessive, laquelle semble alors être une des conséquences d'un affaiblissement provenant d'une tout autre cause, d'une mauvaise alimentation, du séjour dans un lieu froid et humide, etc. : sans cela, les helminthes naissent et meurent dans le corps de leurs hôtes, et peuvent paraître et disparaître alternativement sans inconvénients.”*

Many important diseases have been supposed to originate from parasitic animals and vegetables. The former are not the true entozoa, for these are too large, and may be detected

* “ Worms are developed in localities adapted to their necessities with as little injury as the moss which grows upon the bark of a vigorous tree. They can only become injurious, in general, when in excessive numbers, and this multiplication seems to be owing rather to debility induced by improper diet, cold and moist habitations, &c., than to their presence. Otherwise worms will be born and die in the body of their host, and may appear and disappear alternately without inconvenience.”—*Translated by Ed. S. M. & S. Journal.*

by the naked eye, but they are considered to be animalculæ so small that they cannot be discovered even with the highest powers of the microscope. But, independent of the fact that the existence of such entities is a mere suspicion, none of the well known animalculæ are poisonous. At various times I have purposely swallowed large draughts of water containing myriads of *Monas*, *Vibrio*, *Euglenia*, *Volvox*, *Leucophrys*, *Paramecium*, *Vorticelli*, etc., without ever having perceived any subsequent effect.

The production of certain diseases, however, through the agency of entophyta, is no longer a subject of doubt; as in the case of Muscardine in the Silk-worm, the Mycoderm of Porrigo favosa in Man, etc.; but that malarial and epidemic fevers have their origin in cryptogamic vegetables or spores requires yet a single proof.* If such were the case minute vegetables and spores, conveyed through the air, and introduced into the body in respiration, could be detected. The minutest of all known living beings is the *Vibrio lineola* of Müller, measuring only the 36.000 of an inch, and the smallest known vegetable spore is very much larger than this, whilst particles of inorganic matter can be distinguished the 200,000 of an inch in size.

I have frequently examined the rains and dews of localities in which intermittents were epidemic upon the Schuylkill and Susquehanna rivers, but without being able to detect animalculæ, spores, or even any solid particles whatever. I have examined the air itself for such bodies, by passing a current through clear water. This was done by means of a bottle, with two tubes passing through a cork stopper; one tube dipping into the water, the other reaching not quite to its surface. By sucking upon the latter tube, a current of air passed through the former, and was deprived in its course of any solid particles. Ordinarily, when the atmosphere was still, early in the morning, or in the evening, neither spores nor animalcules could be detected. When piles of decaying sticks or dry leaves were stirred up, or the dust was blown about by the wind, a host of most incongruous objects could be obtained from the air; none, however, which could be supposed capable of producing disease.

To assert, under these circumstances, that there are spores and animalculæ capable of giving rise to epidemics, but not discernible by any means at our command, is absurd; as it is only saying in other words that such spores and animalculæ are liquid and dissolved in the air, or in a condition of chemical solution. That the air may be poisoned by matters incapable

* See an ingenious little work by my distinguished friend Dr. J. K. Mitchell, "On the Cryptogamous Origin of Malarious and Epidemic Fevers."

of detection by the chemist, is proved by the emanations from such plants as the *Rhus vernix*, *Hippomane mancinella*, etc.—[*Smithsonian Contributions to Knowledge. Virginia Medical and Surgical Journal.*]

Miscellany.

Spermatorrhœa.—There are doubtless but few of our readers who will not feel interested in the articles contained in this number, on the subject of *Spermatorrhœa*, for we know of no cases with which we are so continually and painfully annoyed as those in which the patients complain of the real or the imaginary forms of this affection. Indeed the mind is so uniformly disturbed in such cases that it is often impossible to determine the true state of things, and therefore difficult to know whether to administer relief to the brain or to the generative apparatus. We are so well satisfied, however, that many of the cases brought to our notice, have been seriously injured by the injudicious use of Lallemand's favorite remedy—cauterization, that we feel it a duty to raise our voice against such treatment, except as a last resort. Our objection to the use of the Nitrate of Silver is that it does not unfrequently occasion strictures and inflammation about the neck of the bladder—either of which may prove to be more serious than the original affection. Such results would, of course, not often follow the application if made by a skillful and experienced operator—but in the hands of the rash, it cannot but be extremely hazardous.

We are free to confess that we have no more confidence in the efficacy of Digitaline than in that of other narcotics, as opium, belladonna, tobacco, &c, for they are all antaphrodisiacs to a certain degree. The suggestion of the mechanical means indicated in one of the articles alluded to, seems to us a good one; but care should be taken not to expose the organ to strangulation, in the event of an erection, without sufficient warning to the patient. The best prescription we know of, is one which, in our land of plenty, can fortunately be almost always carried out without inconvenience, and one which we have never known to fail in establishing a radical cure. We mean matrimony. The only difficulty is to convince the patients that they may form this connexion *before* they get cured.

We have found much advantage from the cold hip-bath night and morning, and the injection two or three times a day of a solution

of sulphate of morphia into the urethra. The solution may contain 4 grs. in 8 oz. of water, a small syringe full of which should be thrown in and retained (by closing the orifice) at least ten minutes, so that it may run up to or into the bladder, and remain long enough to produce its sedative effect. The tonic action of the cold bath and the locally sedative effect of the morphia, will tend to lessen the irritability of the organs upon which depends, in many instances, the involuntary emissions. If the proportions suggested affect the system materially, the solution should be made milder.

Death of Drs. NATHANIEL CHAPMAN and CHARLES CALDWELL.—

It is our painful duty to announce the demise of two of the most deservedly distinguished physicians of our country. Both equally versed in the domains of medical and polite literature, their impressive and brilliant eloquence secured them a degree of popularity rarely attained by teachers of medicine. Nathaniel Chapman, M. D., died in Philadelphia on the 1st of July, in the 74th year of his age—and Charles Caldwell, M. D., in Louisville, on the 9th of July, at the advanced age of 90 years. It is affirmed by some that Dr. Caldwell was only 82 years of age.

Professorial Changes.—As usual, at this period of the year, we have to record quite a number of changes in the organization of our numerous medical schools.

Prof. Austin Flint, has resigned the Chair of Theory and Practice of Medicine in the Buffalo Medical College, and his place has been filled by Dr. Thos. F. Rochester. Prof. Flint is now attached to the University of Louisville.

Dr. Socrates Maupin, has been elected Professor of Chemistry in the University of Virginia, in place of Prof. Smith, resigned.

Dr. G. A. Wilson, has been appointed to the chair of Physiology and Medical Jurisprudence in Hampden, Sidney College, Va.

Dr. E. A. Peaslee, succeeds to the chair of Anatomy, vacated by the resignation of Prof. J. H. Whittaker, in the New York Medical College, and Dr. Edward H. Parker of New Hampshire has been appointed to the chair of Physiology and Pathology in the same institution.

In Berkshire Medical Institute, Dr. E. K. Sanborn of Lowell, has been elected Professor of Surgical Pathology and Microscopy.

The Medical College of Ohio, has lost the services of Professors Bagby and Cobb by resignation.

The chair of *Materia Medica* in the Kentucky Medical School, is filled by Dr. R. J. Breckenridge, in place of Prof. E. D. Force, resigned.

Dr. W. G. Edwards, has resigned the chair of Clinical Medicine in St. Louis University.

Iowa Medical Journal.—We have received the first number of a new periodical bearing the above title. It is published at Keokuk by the Faculty of the Medical department of the Iowa University in Monthly numbers of 32 pages for \$2 per annum. Our pioneer brother has our best wishes for his success.

Abortive Treatment of Typhoid Fever.—It would doubtless be agreeable to our readers to know the result of such trials as may have been made in our section of the country of the abortive and quinine treatment of Typhoid Fever. We therefore invite communications on the subject from the Profession. Our attention has again been drawn to this question by a paper from the pen of Dr. Fenner, of New Orleans, one of the ablest advocates of this plan of treatment in our country. The Doctor says (*N. O. Med. and Surg. Journal*) that “the propositions I have assumed, are—

“1st. That all the continued fevers originating in the Southern States, are *but varieties of endemic malarious fever*, and are controllable by the same remedies, if judiciously applied at their commencement:

“2d. That all these fevers, most probably, proceed from an ærial poison, which enters the blood; first exerting its malign influence upon this fluid, and thence conveyed through the channels of the circulation to every part of the system:

“3d. That the primary perturbation of the system generally recognized as an *attack of idiopathic fever*, consists for the most part in *functional derangement, without any special organic lesion*, and is controllable by such a remedy or combination of remedies as is capable of equalizing the excitement and circulation, restoring the healthy action of the secretory organs, allaying pain and depurating the blood. It is believed that such a combination of remedies may be found in blood-letting, large doses of quinine and opium, and some mercurial, as calomel or blue mass:

“4. That the *organic lesions* found on post-mortem examinations of fever cases do not fully indicate *the disease* that had existed *ab initio*; but rather, *the secondary and ultimate results of disordered action proceeding from a blood poison*; which morbid action or functional derangement might probably have been corrected, and the blood-poison *neutralized or eliminated* by the judicious application of appropriate remedies.

“The idea of the close relationship subsisting between our endemic continued and paroxysmal fevers was thrown out more as a *sugges-*

tion rendered extremely probable by the facts that had presented themselves to my observation, and been reported by others, than as a *demonstrated truth*; but I have now to add, that the more I have investigated it, the more I have become convinced of its truth."

Fracture Tables —Dr. Frank H. Hamilton, of Buffalo, some time ago published tables containing an analysis of 136 cases of fracture of various bones, together with the result of the treatment. Another edition has just appeared, for a copy of which we are indebted to the author, containing in addition to the old tables, a supplement of 325 other cases, taken by Dr. Boardman, of Buffalo, from Dr. Hamilton's note-book. The whole makes an analysis of 461 cases of various fractures, with their results, not only as to life or death simply, but as to deformity, or shortening, or any variance from the condition of the bone before the fracture. The results arrived at must be of interest, and though they are not in all respects comforting to the vanity of surgeons, they are what is far better, instructive.

Of this number of cases, thirty-one resulted in death soon after the accident, or amputation was immediately required, so that they are excluded in the general estimate. All these cases are taken from the practice of Dr. Hamilton, and other well qualified surgeons, leaving out of consideration all cases treated by ignorant men and quacks, so that the result obtained must give a fair conclusion as to what may be expected in the treatment of fractures by the most skillful men.

We call the attention of our readers to the following abstracts from the summary of all the cases, as showing the proportion of *perfect* cures found in the patients of eminent surgeons:

- Of nine fractures of the ossa nasi, one was perfect;
- Of four fractures of the septum nasi, one was perfect;
- Of one fracture of the sup. maxilla, one was perfect;
- Of thirteen fractures of the inf. maxilla, seven were perfect;
- Of forty-one fractures of the clavicle, fifteen were perfect;
- Of three fractures of the scapula, one was perfect;
- Of thirty-eight fractures of the humerus, seventeen were perfect;
- Of twenty-seven fractures of the radius, seventeen were perfect;
- Of twenty-three fractures of the ulna, fourteen were perfect;
- Of thirty-four fractures of the radius and ulna, twenty-five were perfect;
- Of seventy-three fractures of the femur, fourteen were perfect;
- Of seven fractures of the patella, one was perfect;
- Of nineteen fractures of the tibia, fourteen were perfect;
- Of sixteen fractures of the fibula, eight were perfect;
- Of seventy-three fractures of the tibia and fibula, twenty-three were perfect;
- One fracture of the carpal bones was imperfect;
- Of three fractures of the metacarpal bones, one was perfect;
- One fracture of the phalanges was imperfect;
- Of four fractures of the ribs, two were perfect;
- Of two fractures of the pelvis, neither was perfect;

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Vol. 9.]

NEW SERIES.—OCTOBER, 1853.

[No. 10.]

PART FIRST.

Original Communications.

ARTICLE XXXI.

On the External Use of Common Salt in Intermittent or Neuralgic Pains of the Head. By CALVIN J. FALL, M. D., of Stockbridge, Henry Co., Georgia.

The disease in question is an exceedingly painful one, and is frequently very perplexing to the physician, as I have some reason to know, having suffered two well-marked and several slighter attacks of it myself. It is, according to my experience, much more common in the spring season than at any other time, as all the cases, ever seen by me, have been previous to the summer solstice, and none earlier than the vernal equinox. Its invasion is usually gradual—purely periodical, the periods appearing to be controlled by the diurnal motions of the earth; the pain, which is pulsating, or rather thrilling, similar to an electric shock, commencing with the rise of the sun, attaining its acme when the sun is at the meridian, and again declining to perfect ease—hence the term “*sun pain*” applied to it by the people. So far as the throbbing is concerned, by sun-set, to be again renewed on the following day, accompanied with the same phenomena, except that each succeeding paroxysm leaves the integuments more sore to the touch. Its seat is in the nerve which passes out of the orbit, through the supra-

orbital foramen or notch, and ramifies upon the forehead and temple; the general health of the sufferer may be, and is usually, otherwise, good, but after some days persistence some fever will be developed, &c. More need not be said on the symptoms, as I presume most practitioners are but too familiar with them.

Its cause is, to me, inscrutable; it appears to be connected in some way with the restoration of that proper electro-nervous equilibrium which, perhaps, has been deranged by the cold and wet of the preceding winter: this, however, I leave to be settled by those who have more ability and more leisure than I have. The chemical nature and constitution and physical properties of a remedial agent may sometimes throw light on the etiology of disease, where abstract reasoning and post-mortem examination will avail nothing.

Several years experience has convinced me that we have no article in the *materia medica*, which, for simplicity and entire safety in application, or promptitude and certainty of relief, can be compared with common salt, (*Sodæ murias*). This article, so indispensable in domestic economy, may be no less valuable as a therapeutic agent. I was much pleased with an article, written by Prof. Dugas, on the Saline Treatment of Dysentery, and would most cheerfully recommend its adoption by those of my professional brethren who have not already done so. The formula I use for convenience is,

White of egg, . . q. v.

Common salt, . . q. s. ad saturandum.

To be spread thick on tow, or cotton, batted, or simply a cloth folded several thicknesses, and applied to the seat of pain, to be renewed as often as it becomes hot or dry.

That I may be the better understood, I will append a few cases treated solely or mainly with salt.

CASE I. The writer had a severe attack of "Sun pain," in the spring of 1833, when at school; it continued to grow worse, until he was compelled to abandon his studies and go to his home, fifteen miles distant. Imagine his surprise and delight to awake next morning free from all pain. Had no farther

attack until the spring of 1838, just after leaving the Medical College of Georgia. The attack was severe. All the means recommended by the best authors I had an opportunity to consult were faithfully put into requisition, but proved unavailing. I expected to have it to wear out, as on the first attack, until a gentleman (now professional) told me to try salt. I was at the time suffering excruciating pain. Had it prepared as above directed, and applied. Well, the result was as gratifying as unexpected; the pain gradually, but quickly, disappeared, as dies away the echo on the zephyr. The next day I re-applied the salt, and took a few doses quinine, which terminated the disease.

CASE II. In May, of the same year, saw B. W., a young man *æt.* 18 years, a soldier, engaged in the removal of the Cherokee Indians; had labored under all the symptoms detailed above for five days before I saw him. His case had been mistaken for and treated as inflammation of the brain. He was at the acme of a paroxysm, and was pronounced to be moribund when I got to him. Having but recently had some personal acquaintance with the disease, I had no difficulty in diagnosing more correctly and cheering his mind with a brighter prospect. The next morning the salt mixture was applied, accompanied with a few doses quin. sulph. The cure was prompt and complete.

I have since had many cases of neuralgia, otalgia, odontalgia, &c., &c., in which salt was usually prescribed in some way, and I have been seldom disappointed.

CASE III. Mrs. H., *æt.* 50, was attacked May, 1853, with the usual premonitory symptoms of "Sun pain," as I learned when called, which was only when the case was thought to have assumed an alarming aspect. The lady's husband, confident of his own skill, had been prescribing for her, and kept up colliquative discharges from the bowels; applied a large vesicatory to the cervical spine, and sinapisms freely to the forehead, under the impression that it was a case of inflammation of the brain. She had been bled twice. The result was, she grew weaker daily, and the disease more active; a pretty high

grade of irritative fever, furred tongue, and great mental depression prevailed when I saw her. The salt and egg was directed instantar, and quin. sulph. advised. This last, however, was objected to, to which I very readily consented, as I had a desire to test the powers of salt alone, in a bad case, especially as I believed it would be altogether sufficient. It proved so: there was no other paroxysm, and convalescence speedily followed.

I have already protracted this article much beyond the limits I had prescribed for myself when I began. I therefore close, with the wish that it may be found by others to control this painful affection with as much certainty as it has done in my hands.

ARTICLE XXXII.

Ovarian Dropsy. By D. W. HAMMOND, M. D., of Culloden, Ga.

On the 10th day of April last, I was invited to see Mrs. B., a lady about 28 years of age. She informed me, that in the month of September, 1849, she felt, for the first time, a sharp, stinging sensation in the right iliac fossa, a little below and to the left of the anterior superior spinous process of the ilium. The seizure was transitory, passing off in a few seconds, leaving a dull, heavy aching in the part. A few days subsequent to the primal incursion, she discovered a tumor, about the size of a common orange, projecting from the right flank; it was hard and unyielding, and slightly painful to the touch. For several months it appeared to preserve its quasi organization or development. Early in the year 1850, she became *enciente*; the tumor now commenced enlarging rapidly, and continued to enlarge up to term, when she was delivered of a living offspring, the expulsion of the fœtus producing but slight, if any, diminution in the size of the abdomen. The disease now filled the entire hypogastrium and had ascended to the umbilicus. Mr. B., becoming alarmed at the condition of his wife, sent for Dr. Kendal, a distinguished physician of Upson county, who, upon examination and percussion, discovered fluctuation to be

quite distinct in the *cyst*, and pronounced it to be a case of Ovarian Dropsy. The above is, in substance, the history Mrs. B. gave me of its formative stage. From the birth of her child up to the time I first saw her she had continued regularly to enlarge "*De die in diem*." On examination, I found the abdomen enormously distended, and soon satisfied myself, by making the necessary palpations and explorations over the abdominal parietes, that it was correctly diagnosticated by my friend, Dr. Kendal,—it was Ovarian Dropsy.

I advised hydragogue cathartics, and a free use of the hyper-tartrate potass., stating, at the same time, that if the enlargement continued to increase in despite the remedies prescribed, that she should submit to paracentesis abdominis—the only means left which promised any relief to her protracted sufferings. To which she peremptorily objected, stating that—

"Dr. ———" (a steamer) "says there is no water in me, and people says *he* is a very knowing doctor."

"Oh yes, madam, he is very knowing in his way; that is, if knowing how to heat brick-bats, to make red-pepper tea, and to skin Bayberry bark is knowledge, then he is knowing indeed."

"But stop, Doctor, *he* told me how I might tell if I had any water in me, and I've tried it, and I am satisfied there is not one drop."

"What was his test, madam?"

"Why, he told me to strip naked, (*at night, you know*,) and then to set a candle on one side of me, and for my husband to get down on his knees on the other side, and if as so there was any water in me, that he could see the light from the candle shine clear through my belly."

"Pshaw! pshaw! nonsense."

"You need'nt make light of it, Doctor, for my husband tried it, and he could'nt see nothing—no, Doctor, there's no water in me, certain."

I now left her, to enjoy her own opinion, but earnestly requested, before I left, that if she continued to grow larger, and the distention became so great as to produce alarming symptoms, to send for me immediately.

I heard but little from her from the time of my last interview

until the 2d day of August inst., when I was summoned in haste to see her. I found the abdomen of immense size, giving her great pain and uneasiness. The diaphragm was thrust up into the chest, impeding greatly the normal action of the heart and lungs. This hypertrophic and heterologue development gave rise to distressing palpitations and alarming dyspnœa, together with a constant disposition to syncope. Her strength had failed very much for the last few months, and she looked macilent and care-worn. I at once advised her to submit to the operation, assuring her that the tumor was filled with liquid. She replied that I could do as I thought best, stating, that if she was not speedily relieved she must die very soon. I accordingly performed paracentesis abdominis, in the usual manner, and drew off 83 lbs. of dull white liquid, about the consistence of syrup. Specific gravity 1.400. The whole was contained in a large unilocular cyst. On seeing the water flow, the patient exclaimed, "Well, I always thought *he* was a fool and now I know it." Our patient stood the operation well, sitting up the whole time (two hours) without becoming the least faint. 50 drops of laudanum were now administered, a flannel roller passed around the body, and the patient put to bed. After the subsidence of the abdomen, the tumor was found to be thickened and lobated—of an oblong shape, and extending from the right iliac to the hypochondriac region, and was adherent just below the false ribs. The walls of the sac contained several deep fissures or sulci, running in a longitudinal direction, concentric at the point of adhesion, and divergent at the base; which circumstance might be attributable doubtless to the weight of the sac being suspended from the point of adhesion. Our patient rested well through the night.

August 3d. Found her with some fever, and slight peritoneal inflammation. Ordered, sulph. magnesia $\frac{3}{4}$ i., which operated well during the day. About midnight, I was aroused from my bed by her husband, who stated his wife was dying. I found her with spasms of the fingers and facial muscles, and unable to articulate a word distinctly. Administered a full dose of laudanum and sulph. ether, which relieved her in 20 minutes.

4th. Much better; sitting up on the side of the bed—some

tenderness over the abdomen. Advised a seidlitz powder, and camphorated liniment to be applied freely.

5th. Still better; sitting up some, and able to walk a little about her chamber. From this time she continued to improve slowly, and I found her this morning, the 12th, doing moderately well. She walks about the house occasionally every day; but complains very much of the weight of the sac pulling her down from the point of its attachment.

The sac is slowly filling again. I consider the prognosis unfavorable.

ARTICLE XXXIII.

A Case of Glossitis. Reported by J. S. WEATHERLY, M. D., of Palmetto, Georgia.

Miss. P., aged about fourteen years, rather slender, and of a nervoso-lymphatic temperament, had been sick three days when I saw her, on the 8th of April last. The attack occurred after exposure by going about one afternoon with her shoes and stockings off. She complained first of sore throat with occasional chilliness, and the throat grew worse until the 7th, when she had considerable fever and her tongue began to swell and be painful. On the morning of the 8th, the fever being high, headache intense and swallowing difficult, my presence was requested, and I reached the patient at 8 o'clock, P. M. I found her with intense cephalalgia, great difficulty of deglutition, even of liquids; tongue much swollen, protruding between the teeth, very painful and covered with whitish mucus. She spoke with great difficulty and only in a whisper; skin tolerably hot; pulse 145; bowels constipated; urine very red.

Prescribed comp. cathartic powder—a teaspoonful of sp. æther. nit.— $\frac{1}{4}$ gr. sulph. morph.—and 7 drops tr. ver. viride, in a wineglassful of water, every three hours; also, warm applications to the throat and a hot foot-bath.

April 9th, A. M. Tongue more swollen, and protruding far between the teeth: she has some cough; discharges a tough mucus from the mouth; head still aching; pulse 120—has been nauseated by the ver. viride; bowels have not acted.

Applied a blister to the throat; attempted to get some salts, tinct. rhei., and aloes down, but failed, as every attempt at swallowing strangled her.

Seeing that something had to be done which would give at least temporary relief, I determined to scarify the tongue. The blade of a straight bistoury was placed flat upon the dorsum of the tongue, and being guided by the left fore-finger, was pushed as far down as it could be done conveniently; it was then turned upon the edge, and brought out, making a deep incision into the substance of the tongue. Two other incisions were made in the same manner, which bled freely.

12 o'clock M. Can talk distinctly, and is able to swallow. She now took the salts, rhei. and aloes—the blister was ordered to be removed and followed by a warm poultice—the ver. vir. and spts. of nitre to be continued—cathartic mixture to be repeated, if necessary.

April 10th, A. M. Patient nearly well. Bowels had acted freely; tongue reduced to nearly its normal size; pulse 85—complains of nothing except the blister. No unfavorable symptoms occurred afterwards. She had a rapid convalescence.

ARTICLE XXXIV.

A Case of fatal disease of the Bronchial Glands. Reported by
J. F. HAMMOND, M. D., of the U. S. Army.

BARRANCAS BARRACKS, FLA., 8th Aug., 1853.

Dear Doctor—I have just made an interesting post-mortem examination. I was called to a negro man, about fifty years old, a few days ago, whom I found with diarrhœa of three or four days standing, and difficult respiration; pulse a little smaller than natural, and healthy as to frequency; no pain anywhere except in the right hypochondrium, from an old "strain"; head clear; tongue clean; no thirst; moderate appetite; chest resonant throughout to percussion; the bronchial respiration loud; the vesicular sound nearly natural; occasionally slight cough; sounds of the heart a little obscured; a little dull pain on pressure of the right hypochondrium. A blister plaster was applied over the pain. A bath, and castor oil followed by pills

of blue mass, ipecac. and plumbi. acet., combined, relieved him of the diarrhœa in forty-eight hours. I left him at that time as convalescent. And, as I found him at my last visit breathing quietly, I concluded he had been shamming as to his respiration.

Twenty-four hours afterwards I was called to him by his owner. The difficulty of respiration was greatly increased—in every other respect he was apparently *well*, except that there was some tenderness on strong pressure over the vertebræ, from the middle of the column downwards. A blister plaster was applied over the tender part of the spine; and a Medical Officer of the Navy was called in consultation. It was determined that the upper surface of the liver was *possibly* inflamed, involving the diaphragm. Calomel and opium, in combination, were given him every four hours. At the end of seven hours he was dead.

The autopsy was made an hour and a half after death. The viscera of the abdomen were perfectly healthy, except some injection of the veins of the duodenum. In the thorax, the heart was healthy; the right lung was suspended by a net of old adhesions like a cocoon in its mesh; the left lung was collapsed, but there were old adhesions in many places; both were covered by a heavy coating of melanotic deposite, which, in some places, most posteriorly, was spread uniformly under the surface—it seemed to form a part of the tissue as the pigmentum nigrum does of the skin; during the intervals of the deposite the pink hue was a shade deeper than natural; the lungs were crepitant and pervious to air throughout; they floated perfectly on water; they were *very* little congested. In removing the lungs, several of the bronchial glands were cut through—they were the size of large almonds, black throughout; several of them had suppurated, and poured out, when cut, a deep bluish-black pus. Their pressure upon the bronchial tubes had, doubtless, caused the death.

PART II.

Eclectic Department.

Letters upon Syphilis. Addressed to the Editor of L'Union Médicale, by P. RICORD. Translated from the French, by W. P. LATTIMORE, M. D.

TWENTIETH LETTER.

[Continued from Page 531.]

My dear Friend: I propose to entertain you with a still further description of indurated chancre. The subject is somewhat uninteresting, though of much importance; and I need all of your kind attention while I attempt to elucidate it.

This important variety of the primitive ulcer is regularly rounded, in proportion as it is seated upon homogeneous tissues. Its edges are scarcely ever decollés. They are not always perpendicular (*taillés à pic*). A little prominent, they are continuous with the bottom which is hollowed, as it were, *in the form of a cup*. The surface of the ulceration, which is grayish and lardaceous, is sometimes *irised*. Its center is of a somewhat deep, brownish color, which has given rise to the designation of *partridge's eye*, sometimes applied to it.

But at what period does the induration, which constitutes the principal character of this variety of chancre commence? What time elapses between the act which effects the contagion and its first manifestation?

The solution of this question is highly important, inasmuch as from the moment the induration takes place, the disease ceases to be merely local. I have sought to determine the period; but this is not always an easy matter. Patients seldom present themselves until a long time after contagion has been contracted; and, not aware of the importance of the pathological state in question, they have failed to notice its commencement.

In the majority of cases, this want of attention on the part of patients is explained by the fact that indurated chancre is essentially indolent; that it is slow in its progress; that it suppurates little; that it is not perceived until some time has elapsed; and that frequently the induration escapes notice. You are aware that I have already cited examples of this fact. I mention it again, in order that you may recall to the minds of those who are firm believers in the miracle of constitutional poxes *d'emblées*.

We are not always sure of the date of the coition or contact to which the chancre itself is to be referred; consequently, it is very difficult to ascertain when the induration actually com-

menced. However, when it is possible to arrive at a true knowledge of the state of affairs, it is not before the third day that the induration manifests itself. In all cases, it becomes manifest in the course of the first or second week. It would even seem certain—unless more precise observations should prove the contrary—that, if a chancre exists for more than three weeks without becoming indurated, it will not indurate at all. Induration is a precocious phenomenon. Certain conditions may so deceive us as to induce belief in subsequent indurations. Let us examine these conditions.

The specific induration is not always easily recognized. After ordinary contagion as well as after artificial inoculation, the infected part frequently becomes the seat of an inflammation, that which Hunter called *common inflammation*, which, for a given period, incloses and masks the specific induration, so that it is only in proportion to the degree in which the simple œdematous, sub-phlegmonous, or frankly inflammatory engorgement is absorbed, that the specific induration is well described and is found exhumed, as it were, from the inflammatory atmosphere by which it was surrounded. Thus far, the engorgement, whether œdematous or inflammatory, has been so prominent an indication that the specific induration is only thought to commence from the moment it begins to be appreciated; and persons have thus been led to believe in tardy indurations, in chancres which have not begun to indurate till three weeks, a month, and even a longer period, after contagion.

Certain local applications and cauterizations sometimes give rise to factitious indurations, which may be produced at different periods, and thus give rise to misconceptions. These factitious indurations may even be complicated with specific indurations, and render the latter unrecognizable. It is known that unbelievers in a specific virus formerly stated that corrosive sublimate will produce an ulcer similar to the Hunterian chancre. Similar! ay, they were right; but not an *identical* ulcer. In fact, with corrosive sublimate; with chromate of potassa; with liquid acetate of lead, so often employed in vulgar practice, with hot ashes from a pipe, and sometimes simply with the nitrate of silver, accidents may be produced which so closely resemble indurated chancre, as to constantly deceive physicians who have not an extensive acquaintance with the disease. Such errors alone have induced the belief that indurated chancre is not invariably followed by constitutional accidents.

There is another source of error from the influence of which several syphilographers—among others, M. Babington, the

commentator of Hunter—have not escaped. Patients may preserve an induration which has resulted from a primary contagion, and subsequently contract a new chancre on the same spot. Without a clear knowledge of the history of the patient, it might be supposed that the previous induration was the starting point of the latter chancre, and was the first symptom of the contagion. This is a great error; in all cases, the induration consecutively follows the ulceration.

Such cases as these—in which no account has been taken of an induration resulting from an anterior contagion—have induced the belief that the new chancre contracted by the patient, occupying the same spot as the prior induration, becomes, in turn, itself indurated; an error which might occasion the admission of more exceptions to the law of *unicité* than really exist.

You know that several syphilographers assume that all primitive accidents may be followed by secondary accidents, and that, if this remark applies to any accident in particular, it applies to blennorrhagia. Very well! these syphilographers admit with stronger reason, that nonindurated, as well as indurated chancres may be followed by constitutional accidents. It is, therefore, very important to ascertain how far this supposition is true. You have already seen that common inflammation can so mask the specific induration as to induce the belief that another form of chancre exists. It also happens, though more rarely, that the ulceration, after having been indurated, becomes phagedenic. Therefore, if one has not seen the commencement of the disease, he may still be deceived, and believe in the possibility of constitutional accidents succeeding non-indurated phagedenic chancre.

On the other hand, the induration, without losing its immense value as a symptom, is not always well formed; it does not in all cases, attain the same development. Sometimes it is superficial. In order to discover it in the thickness of the skin or mucous membrane, it is necessary to clearly understand its nature. Sometimes it only imparts to the touch the sensation derived from feeling a fold of parchment; at the *Hôpital du Midi*, I call this form the *parchment induration*. Indurated chancres, then, are frequently taken for simple excoriations, and for simple balano-posthites, when they do not wholly escape attention; for they are on a level with the healthy parts, and sometimes even slightly prominent.

The induration ordinarily invades the whole base of the ulceration; but in some rare cases, it only affects the edges, in which case it is annular. To this form of indurated chancre we might apply the designation of *primitive annular syphilis*.

When no complication exists, the induration is suddenly circumscribed by the healthy tissues; it is much more extensive than the ulceration of which it forms the base. It is composed of a hard, somewhat cartilaginous, resisting, elastic, indolent, perfectly rounded nodule, which raises the ulceration above the surrounding parts, and thus constitutes a variety of *ulcus elevatum*.

The induration is sometimes presented under the form of a more or less prominent *crest*, when the plastic infiltration which constitutes it is not formed of homogeneous tissues, and when it meets resistance at some points, as at the reflection of the prepuce to the groove at the base of the glans, at which situation, indeed, the best characterized indurations are found.

If the skin or mucous membrane which covers an induration be compressed, these tissues become pale, presenting to our view something analogous to what we perceive when, turning over the eyelid, we compress the conjunctiva upon the tarsal cartilage.

The induration is usually produced in a slow and gradual manner. Sometimes it increases by *saccades*; in some cases it remains for a long time, but slightly perceptible, then subsequently assumes considerable development. The tissues often become extensively indurated. I have seen the entire length of the base of the glans undergo a cartilaginous transformation which might have given rise to the belief in its cancerous degeneration. One of the most curious cases of this kind was sent me by Professor Andral.

The induration, after having diminished, or even disappeared, is very liable to return. It is not rare, then, to see it acquire dimensions more considerable than it at first assumed.

The duration of the induration is unlimited. In those cases in which it is superficial, in which it resembles *parchment*, I have seen resolution take place, so completely as to leave no traces of its existence, within a less period than a month: at other times, on the contrary, it persists for months, and even years. When it is developed on the groove, at the base of the glands, at which point, as I have said, the most marked cases occur, its duration is greater than at any other spot. It may quickly disappear from the glans, from the neck of the uterus, and from the vulvar ring, where it is but slightly marked and difficult to detect. At the urethra, especially in women, and at the vagina and anus, its existence is quite ephemeral. Much attention is required to prevent mistakes. On the tongue, and particularly on the lips, it remains a tolerably long time. When the induration begins to disappear, the ulceration has, in all cases, been healed for some time.

When resolution takes place, the induration undergoes modifications ; it loses its resistance and elasticity ; it becomes as it were, gelatiniform, and a wrinkled spot, of a coppery-brown tint, finally occupies its place.

The indurated chancre, which is less often multiple than the other varieties, the specific ulcerating period of which is soon limited *sua sponte*, or by the aid of art, nevertheless assumes, at times, considerable dimensions. It extends itself, and excavates the neighboring tissues, so much so, indeed, that we might expect it to occasion great loss of substance ; but when cicatrization is complete, frequently no traces of its existence will remain, for it is the plastic exudation alone which serves as aliment for the phagedenic influence, thus securing the surrounding tissues from the effects of the ulceration. A knowledge of this very common condition of indurated chancre is important, in view of the etiology of constitutional syphilis, for it is not the most numerous nor the deepest cicatrices which prove that the poisoning has taken place.

The specific induration of chancre is absolute proof that the constitutional infection has occurred. It is the intermediate state between the primitive and the secondary accident. In fact, the indurated is that variety of chancre which soonest loses the distinctive character of the primitive accident, to wit, the power of furnishing inoculable pus ; but, if the induration of chancre demonstrates the existence of infection, and if the degree of its manifestation always bears a definite relation to the gravity of the accidents which are about to succeed—if this induration can be considered (permit me to use the expression) as a *syphilometer*—it may also serve as an excellent guide in treatment ; for this form of accident is the one which is commonly most amenable to mercurial treatment. Nevertheless, there are cases in which the induration resists treatment ; but in this case the induration is generally no longer specific, but proves to be an organized tissue, that is to say, an internodular tissue, which has succeeded it. In this way I was able to account for an induration presented by a patient who entered the hospital to be treated for caries of the frontal bone, which came on thirty years after the development of a chancre at the base of the glans. This induration persisted under the form of a very marked nodule. In a great number of cases it is exceedingly difficult to distinguish between the internodular tissue and the specific induration. The thickness of the skin and mucous membranes, the subcutaneous and submucous cellular tissues, constitute the anatomical seat of the specific induration ; but it would seem that it usually selects for its seat the lymphatic capillaries. It is in those regions, in fact

where the lymphatic network are most prominently exhibited, and where they are most abundant, that the induration is most completely formed, and acquires the greatest dimensions. This opinion receives still farther support from the manner in which the induration is propagated, that is to say, it is seen to follow the course of the lymphatic vessels, which are delineated in the form of cords, in proportion as they become more voluminous.

As to the intimate nature and constitution of the induration, organic chemistry, which has furnished so many marvelous results, more, perhaps, than will bear careful investigation, has taught us nothing; while the microscope, which generally promises more than it reveals, has thus far shown the specific induration to be only fibro-plastic tissue, which, proportionably quite abundant does not differ from that met with in nonspecific conditions. Such, up to the present time, is the result of the researches undertaken by one of my highly distinguished disciples, M. Acton, of England, and of those subsequently prosecuted in Paris, by MM. Robin and Marchal (de Calvi.) The same results have been obtained by our learned and industrious micrographist, Dr. Lebert, to whom we owe the production of so many fine works.

Such, my dear friend, are the results of my researches and observations on indurated chancre. I present them to you here, simply as indications; for, as I have often been obliged to repeat, I am not now writing a didactic treatise on syphilis: I am only calling your attention to the principal points of my doctrine, on account of the objections still made to it, and which are addressed more or less directly to myself. Ampler developments form the subject of my oral instruction, and especially of an extensive work which I am preparing, and of which these *letters*, to tell the truth, are merely the *summary*. I am here presenting the general principles, the essential points of doctrine embodied in the latter work, indicating the principal grounds on which they rest; and the present *letters* have no other merit than that which the character of your readers may impart to them—readers who are no longer students, but learned and enlightened practitioners, to whom the indications I have pointed out merely serve the purpose of calling to mind the careful studies and researches of previous years.

TWENTY-FIRST LETTER.

My dear Friend,—*How do chancres cicatrize?* Allow me to say a few words on this important subject.

The period of reparation is indicated by the disappearance of the areola of the ulcer. Its edges become *dégorgés*, sinking and resting on the bottom; and the undermining ceases, if it

existed. The margin becomes of a pale, grayish, pearly tint, and finally assumes the normal color of the surrounding tissues. The bottom cleans off; the gray, lardaceous, diphtheritic layer is at first pierced, as it were, by granulations which, finally occupying the place of the layer, give to the ulceration a granular aspect, and a healthy, rosy tint. The pus becomes less abundant, and of a healthy, creamy character; and it may, in this case, justly be said to be *laudable*, for it ceases to be inoculable. In proportion as the parts fill up, the epidermis spreads from the circumference to the centre, and the cicatrization is completed in the same way as in any wound which has suppurated.

The *cicatrix of chancres* may remain more prominent than the surrounding parts, sometimes being on a level with them, but most frequently depressed, according to the thickness of the tissues affected. In a great number of cases, it is indelible; while in others it completely disappears, as frequently occurs after indurated chancres, or when the chancre is seated upon a mucous membrane.

But, as those whose experience has been extensive well know, the period of reparation is subject to various irregularities. In *serpiginous chancre*, one extremity often cicatrizes, while the other becomes more diseased; sometimes one side heals, while the other continues to ulcerate. Finally, the cure often takes place at one or many points of the centre, while the circumference is constantly augmenting its vicious circle.

You know, in fine, that, in certain individuals, where a proper course of treatment has not been pursued, where the physician has been ignorant of the means of repressing granulations by cauterizations, or where foolish prejudices have prevented the employment of this remedy, the granulations are said to become luxuriant and vegetating, and to give to the ulceration certain aspects which have obtained for it the name of the *budding, fungous, or vegetating chancre*. True vegetations, of varied forms, may then be produced; but, as these vegetations may be considered an accidental, epigenic tissue, they are not of a syphilitic nature, as we shall hereafter see.

At this period, as I have already said, that is to say, when the chancre has infected the economy, it may itself undergo a transformation *in situ*, and finally present the characters of mucous papules, and thus give some countenance to the opinion of those who, from failure to analyze the subject, are unacquainted with these metamorphoses—of those who have admitted, besides, that these accidents could be sometimes primitive and sometimes secondary, and that they were, in all cases, contagious; an opinion which I have already controverted.

But here a point of doctrine arises, on which I insist, and to which I must again call your attention. It is this: That form of chancre which may undergo relapses at different times *never returns when it has once cicatrized*. If a new inoculable chancre develops itself after cicatrization has become complete, we can affirm that the chancre is the result of a new infection.

From what I have stated, it is very certain that, as far as we can arrive at a knowledge of the facts from a due acquaintance with the conditions by which they are surrounded—when we take into consideration the seats which chancres seem to select for their development, as well as their usually limited number—when we likewise know how to appreciate the variations which different chancres present with respect to their period of progress and of specific *statu quo*, and with respect to their course and duration, and the different aspects which they may assume at the period of reparation, and even of subsequent cicatrization—when, finally, we consider the more or less marked influence of the mercurial treatment in certain cases, we can usually arrive at a rational diagnosis which is almost absolute.

The physiognomy of the primitive ulcer is ordinarily so expressive (permit me to use the word), at the specific period, that we are able, by seeing it, to name it. It is even necessary to distrust this first impression, inasmuch as it may occasion indiscretions which can scarcely be repaired. You have allowed me to illustrate my remarks by pathological anecdotes, and I shall avail myself of your kindness, happy if I can relieve the aridity of my previous descriptions.

One of our distinguished *savans* entered my cabinet one day, and without any preamble showed me—a diseased member, saying, “What is that?” I immediately replied, “It is chancre.” “Very well, sir, my wife gave it to me.” “Then, sir, it is not a chancre.” “And why not, if you please?” “Because,” I replied, “that which distinguishes simple ulcerations resembling chancres from true chancres, is the source whence they have been derived.” My patient was not the dupe of an argument which would have been sufficient for certain physicians whom you know; and he contented himself with replying, with much dignity and resignation, “Cure me.”

But is the diagnosis always so easy as some of our classical authorities have assumed it to be? I appeal to M. Lagneau, who is so worthy a representative of these authorities at the present time. Observe now, whether, despite all the care he exhibits, he succeeds in distinguishing the primitive chancre from what, with so many others, he still considers the secondary chancre. Glance again at his synoptical and comparative table of the ulcers which are liable to be confounded with those

produced by the syphilitic virus, and tell me whether he is successful, and especially whether he enables others to be successful in establishing this difference with certainty.

Mercury, that infallible touchstone in the eyes of believers—a touchstone which, in England, has been the basis of the division of syphilis into the *true* and the *false*—is a deceptive reagent. It often cures non-syphilitic accidents, while it aggravates those which are syphilitic, and which sometimes get well without treatment.

How many chancres exist which are unrecognized by skilful practitioners! How many errors are committed with respect to the different varieties of indurated chancre, the most dangerous form of all! Sometimes simple excoriations are mistaken for the disease; at others, the disease is supposed to be a true cancerous degeneration. My friend, M. Vitry, of Versailles, must recollect the case of a patient to whom I was called by a physician in Paris, not to judge of the nature of his disease, but to amputate his penis. I recognized the existence of an indurated chancre, with considerable developement of the plastic exudation, and pills of the protoiodide superseded the knife.

One of our learned professors, belonging to the faculty of Paris, who excels in diagnosing syphilis as well as other diseases, cannot fail to recollect the case of a Russian nobleman whom we saw together at the house of our honored and regretted master, M. Marjolin, and in whom he was unwilling to recognize the existence of a primitive accident, because nothing was observed but the specific induration, and because the nobleman could not explain how he had contracted the accident; yet, a short time afterwards, as I had predicted, we obtained the most striking proofs of a constitutional affection.

If you will allow me, I will relate a short anecdote. Cullerier the nephew, one day sent to me a popular writer in order to obtain my opinion relative to an ulceration situated upon the corona of the glands; an ulceration with an indurated base, and not then presenting those characters at its edges and base which are authoritatively assumed to constitute chancre. Nevertheless, I recognized an ulceration with the specific characters of induration already described, and with the ganglionary prolongation which we are shortly to study. Cullerier was not of my opinion, because he had examined the two women accused of imparting the contagion, and had found them healthy. Admitting neither mediate contagion nor spontaneous syphilis, and placing confidence in the word of the patient, he could not admit the existence of a primitive ulcer. I, who admit all rational sources of contagion, and often doubt until I obtain the

most certain proof, remained convinced either that the patient had been deceived, that he deceived himself, or that he deceived us. In fact, scarcely six weeks had elapsed when very marked constitutional accidents—so marked, indeed, as to be exceedingly difficult to cure—were manifested. But, while Cullerier was yet pondering the question how and why this patient had contracted the pox, I was called to the house of a great lady.

I arrived, knowing nothing of the purport of my visit. This lady was mysteriously seated in her boudoir; and, in spite of the softened light of the room, I could perceive on her face the evident tokens of a secondary affection. "Doctor," said she, "I have to speak to you on a very delicate matter." Wishing to cut short a painful avowal, "I see what it is, Madam," said I to her; "for your face tells me plainly enough why I have the honor to be here." "What do you say?" replied she, with astonishment. "That you are diseased, madam, and for that purpose desire my attentions." "Not the least in the world; and I have sent for you in order that you may assist in curing M. X—— (the writer sent to me by Cullerier) not only of his disease, but also of his dangerous *liaisons*." And she then drew a portrait which was but little flattering of the two women whom Cullerier had examined and found healthy, and who, according to this lady, were the cause of the whole trouble. I had much difficulty, as you may imagine, in making her understand that the source of our poor author's trouble was much closer to me, and in obtaining the avowal that the pressing interest she manifested relative to him was not altogether based on a purely platonic affection.

It is ever thus, my dear friend, and here is the moral of this anecdote—that people of the world never make you complete avowals. By reason of their relations with great ladies, or others in whom they have confidence, their mind is a thousand leagues from the truth; their thoughts are not fixed upon the true source of their disease, and they seek it where it is not.

You see, then, how difficult, frequently, is the diagnosis of chancre, and how wrong we should be in denying its existence when patients will not aid us in tracing it to its source.

I am acquainted with all the difficulties of diagnosis in many cases, and I have seen persons of the greatest skill commit frequent errors in relation to it; and for this reason I have said, and I still assert, in spite of contrary opinions, that the only positive, univocal, pathognomonic sign of chancre, at the period of progress, or of specific *statu quo*, is the inoculable character of the *pus which it secretes*. From this fact I have drawn the following conclusion:

Inoculation furnishes the most certain sign of the specific nature of the ulcer.

I stated in the work which I published in 1838, that, if mercury must be given in all cases where a virulent, primitive accident exists, it is essential to be assured of the fact of virulence by artificial inoculation. But compose yourself, my dear friend; this operation, so repugnant to some persons, and even dangerous, if not properly managed, is unnecessary in practice; and I have never advised its performance as a general rule. The prognosis and treatment of the affection depend on other indications. The induration of chancre, with its accompaniments, in relation to which inoculation furnishes us no assistance, form the conditions whence we deduce the state of the constitution, and point out to us the specific treatment which the disease requires. This fact, I trust, I shall be able to demonstrate.—[*New York Medical Times*.

Our Problems. An Essay to Facilitate United Labours for Rational Medicine. By F. W. BENEKE, late Resident Physician to the German Hospital, London, &c., &c.—*Goettingen*, 1852.

Manifold are the concussions which medical science has experienced in the short time of the last half century; all the errors created by an imperfect generalization, as depicted in the history of medicine, have once more appeared before our eyes in rapid succession. Even now some of them have not yet vanished from the scene; but the conviction more and more is gaining power, that the only hope for the continual and lasting progress of medicine lies in the patient and energetic pursuit of the same method to which the so-called exact sciences of nature owe their flourishing state; we mean the way of sober, unprejudiced observation, and strictly logical induction. Not insignificant is the profit which has already accrued from the exertions of those who have clearly recognized the correctness of this view. No one can, however, help feeling that the subject before us is enormous in comparison with the limited power of a single being. Many are certainly deterred from entering upon it by seeing how much is wanted, how little can be done, when the observer stands alone; where to begin, and how to begin, is unclear to not a few, who feel a real desire to work, and who are also able enough to give aid in the investigation of many important points.

Great is the progress made in anatomy and physiology, in physical diagnosis and pathological anatomy; but with much reason, Beneke remarks that we have, as yet, gained very little

through these collateral sciences for the rational *treatment* of disease, which always must be the main end of our labour. The cause of this sad truth he sees in the circumstance, that our materia medica and our therapeutics are based, to too great an extent, on rude empiricism, uncontrolled by physiologico-chemical experiments, or by pathologico-chemical experience. To remedy this deficiency, Beneke urges the necessity of co-operation according to a certain plan.

If we look at medical and medico-chemical literature, we find that a great number of men, and of able men, too, have spent much of their time in pathologico-chemical researches, without at all promoting the progress of science. The principal cause of failure in these investigations is, that they have been working without a *guiding idea*. In order to avoid this, Beneke, in the first instance, develops his views on the origin and nature of disease in general, and on the ways in which the metamorphosis of material may become anomalous. Our limited space prevents us from giving in detail his idea concerning the essential nature of health and disease; it may be sufficient to state here, that disease is considered as originating either in disorders of the alimentary material, or in one or other of the agencies operating upon this (the organs of the body and the atmospheric air). As the abnormal changes in the constituents of the food and of the blood (the product of the food) form the more frequent source of disease, Beneke gives a brief sketch of the normal metamorphosis of aliments, under their different heads of azotized, non-azotized, and inorganic. The normal course of the metamorphosis of these three groups is indispensable for perfect health; it may depart from the standard by being either *retarded* or *accelerated*;—*retarded*, when the single constituents do not run through the complete series of changes; *accelerated*, when the metamorphosis takes place too rapidly. As the series of changes of the azotized components is greater than that of the non-azotized, Beneke concludes that the latter are sooner decomposed than the former, and proposes, as the first *law of dependency* (*Abhängigkeits-Gesetz*) for the metamorphosis of matter, that, according to the larger or smaller quantity of the non-azotized constituents (entering the blood), the metamorphosis of the azotized is more or less rapid. Although we may admit that this is the case in general, we cannot agree with Beneke in thinking that it always must be so; the circumstance, at least, that the series of changes in the azotized nutriment is the longer one, cannot be a sufficient argument to us, as a longer series may be finished, under certain conditions, more rapidly than a shorter one.

As a second law of dependence, Beneke proposes, “that the

metamorphosis of the azotized and non-azotized alimentary material is, to a certain extent, dependent on the quality and quantity of the inorganic material." (p. 12.)

After these preliminary remarks, our author proceeds to the consideration of the question:—"Of what nature are the abnormal conditions of the food? What is the nature of the morbid changes of the blood resulting from them, and which is the connection between this and the disease of the whole organism?" (p. 13.)

Although food may deviate from the normal in *quantity* and in *quality*, yet the quantitative disorders are the most frequently met with, the more so, as those generally considered as qualitative are in reality quantitative, consisting in the presence of a *plus* or *minus* of one or several of the normal constituents. Guided by the idea, that from the disproportion of one group of the alimentary materials, at first a disorder in the metamorphosis of the *corresponding* group in the blood must ensue, and later only in the *other* groups, Beneke divides these disorders into *direct*, *indirect* and *complicated* ones. If the quantity of the azotized class exceeds the measures which can be fully digested and carried through the normal series of changes, *direct* disorder in the metamorphosis of the materials of this class results, which will show itself, in the beginning, in the abnormality of its ultimate products of decomposition, and later only in its earlier products. A transitory increase of the ingesta will be the cause of a transitory augmentation of urea and lithic acid in the urine; if the increase is continued for a long time, and is considerable, not only urea and lithic acid, but also oxalic acid, will be excreted in large quantity; and doubtless, also, the azotized constituents of the bile, and the proportion of the albuminates in the blood, will be augmented. In a similar manner, by the use of too large a quantity of the non-azotized and inorganic materials, the metamorphosis of these classes may become disturbed.

Indirect Beneke calls those disorders in the metamorphosis of one group, which are not effected by disproportion in the ingestion of material belonging to the same, but of the materials belonging to another group. If, for instance, the quantity of azotized substance taken within a certain time be not above the normal, but the quantity of the non-azotized be increased, then, according to the first law of dependence, the disorder would show itself in the series of the azotized material, and the same form of disease might result as from direct augmentation in the ingestion of the azotized substances. Beneke considers these retardations of the azotized constituents of the blood as essential in the development of scrofulosis, tuberculosis, and

those conditions which are attended by oxaluria; in their highest degree they may cause the appearance of sugar in the urine, which, in such cases, probably has its origin in the azotized elements. Another instance of indirect disorder is found in the fact, that after the habitual use of soda the symptoms of acidity in the stomach and intestinal canal are increased, as well as the proportion of mineral acids excreted in the urine; which phenomena can only be interpreted by a change in the metamorphosis of the azotized and non-azotized organic material of food, effected by the disorder in the ingestion of inorganic materials. The results which Dr. Parkes has gained from his researches on the action of liquor potassæ illustrate, it may be, the same order of facts.

By the coincidence of direct and indirect disorders in the metamorphosis of the single groups, the *complicated* disorders result, which are in reality the most frequent.

Although it is frequently difficult to trace the direct, indirect, and complicated disorders, yet they form in many instances only the first link of a long chain of consecutive conditions, of which the one results from the other, and frequently remains in reciprocal connection with it. As an instance of a more simple kind, Beneke mentions the connection between the abnormal production (or we would rather say, abnormal accumulation and excretion) of oxalic acid and the impaired growth of cells. His view is, that by the directly or indirectly retarded metamorphosis of the azotized substances, the quantity of oxalic acid in the system becomes increased; phosphate of lime being easily dissolved in a solution of oxalic acid, the presence of the latter in an augmented proportion tends of course to deprive the system of a certain quantity of the phosphate. This being further essential to the formation of cells, a deficiency of the latter function must result as a third link in the chain; emaciation is the necessary consequence, originating, in this case, in an abnormal accumulation of the azotized ingredients of the blood.

Although we do not yet think ourselves entitled to consider the morbid increase of oxalic acid in the human organism as a product merely of the azotized substances to the exclusion of the non-azotized; and although we cannot admit, in these morbid states, an abnormally increased production, but only the presence in an increased proportion (probably accumulation in consequence of retarded metamorphosis); yet we highly value the manner in which Beneke investigates the origin of disease. If we look only at the dietetical indications resulting from it, without entering into the medicinal treatment, we see at once that the alimentary material must be diminished to such an

extent, that all the ingredients can run through the normal series of metamorphosis; we cannot doubt, that by increasing the quantity of the so-called strengthening food, a practice to which patient and medical man not rarely resort, the root of the disease is nourished, and the emaciation must necessarily increase.

After having thus alluded to the abnormalities in the mixture of the blood which may arise from the alimentary material, as the source of the blood, and which he calls therefore primary disorders, Beneke passes to the consideration of the *aggressive agencies*—i. e., the agencies causing changes in the blood—namely, the *atmospheric air* and the *organs of the body*. The former of these acts only upon the blood, as the product of the alimentary material—the latter as well on the blood as on the nutriments.

Without further entering into the contents of this chapter, we may only say, that here again the disorder may be quantitative or qualitative. By the diminished proportion of oxygen brought in contact with the blood, the metamorphosis of the latter must be retarded; disorders must arise, similar to those which are effected by too large a quantity of food. On the other side, by an abnormal introduction (absolutely or relatively) of oxygen, the change of matter must be accelerated.

In considering the action of the *living organs*, their *structure* and *function* must be always considered simultaneously. Dr. Beneke, with much reason, particularly dwells on the importance of the influence of the *nervous system* in the operation of the organs on the alimentary material and on the blood, and in the process of metamorphosis in general—a circumstance which we frequently find neglected in physiological chemistry. The specific agency of the living organ—as the effect of the anatomical structure, modified by the presence of nerves, and their connection with the nervous centres—Beneke calls *typical energy*. This is, however, not to be understood as if he were admitting a so-called vital power independent of the laws of inorganic nature, but he uses it merely as a term for that as yet inextricable complexity of various factors. The “*typical energy*” of an organ might therefore be shortly interpreted as the product of all the physico-chemical processes under the control of the nervous system of that organ. We can never be permitted to take only into account the mechanical construction of an organ as a merely physical apparatus, but we must always look at the fact that it is endowed with nerves and connected with the nervous centres. Whatever excites or depresses the latter must exercise an exciting or depressing influence on the metamorphosis of the organic matter. Hence

the striking influence of joy or grief on the physical state in general. We have twice had opportunities of observing the correctness of Dr. Beneke's remark, that under continued depressing mental influences oxalic-acid crystals appeared in the urine constantly and in very large number, and that at the same time the quantity of lithic acid became increased, while no change had taken place in the manner of living. In another subject we almost daily examined the urine during seven weeks of great excitement and intense mental activity; the quantity of twenty-four hours was not changed; the colour was clearer; the specific gravity, in general 1026—1028, had decreased to 1022—1024; not a single crystal of oxalate of lime could be detected under the microscope, while they were seen almost always in a small number, as well before as after that period; the diet had not been altered; no change had taken place in the amount of exercise.

After shortly alluding to another source of disease, in the disorder of the *egesta* (through the skin, the kidneys, the liver), Beneke advances to the proposition and discussion of the *subjects for inquiry*.

Whatever may be the case we have to deal with, we shall always have to answer the questions: What is the nature of the disease? How did it arise, and how may it be removed? To these questions, however, we cannot answer, without knowing what is the state of health, and what are the means of preserving it. Our knowledge on these points is, as yet, so limited, that they must be placed among the principal subjects of inquiry. If we may say that health exists when the quantity and quality of the ingesta and egesta are normal, then we see arising immediately the following points for investigation:

"1. To find the standard measure for the quantity and quality of the ingesta of twenty-four hours.

"2. To find the standard measure for the egesta of twenty-four hours; for the ultimate bodies of decomposition of the different series of metamorphosis.

"3. To find the standard measure for the atmospheric influences.

"4. To find the standard measure of the effect of single organs, and of the whole living organism on the ingesta and egesta." (p. 46.)

Many points connected with these questions are still in great obscurity, and an exact solution of the problems is as yet impossible; much advantage, may, however, be gained already now for scientific as well as practical medicine, by an approximate investigation of the normal quantity of the ingesta and egesta, of their dependence on the difference in the atmospheric conditions, and in the state of the living organism.

The author, therefore, draws our attention, with reason, to these points, as being the principal desiderata for pathology and therapeutics. As the best way to form a correct idea of the normal measure of the ingesta, Beneke advises to collect as many accounts as possible of the quantity and quality of food consumed in twenty-four hours by apparently healthy persons, in the greatest possible number, in different ages, under different atmospheric influences, &c. Much information would be certainly gained by such observations, but we are inclined to think, that we should gain more by the accurate investigation of the process of nutrition and metamorphosis of organic matter in animals, according to the manner shown to us in the admirable work of Drs. Bidder and Schmidt.* The whole amount of the ingesta and egesta, in the same animal, must be examined during one period, when it is allowed to take as much food as it likes; during another period, when it takes only a limited quantity and special quality; then all the principal excretions (kidneys, lungs, intestines,) during a third period, that of starvation; the gain or loss of weight is carefully to be marked on every day. Accurate notes are to be taken of the atmospheric influences, of the activity of the animal, &c., &c. Manifold might be the variations in these experiments; food might be withdrawn and water permitted, according to the desire of the animal; solid food without any fluid; mere vegetable or mere animal food; food without large quantities of chloride of sodium, of earthy phosphates, &c.; food as much as possible deprived of the inorganic constituents, &c., &c. By this method we should find at least how much substance is consumed and decomposed in a starving animal, of a certain species, and of a certain weight, under certain atmospheric influences;—we should further learn, through the quantity of nitrogen contained in the urine and in the alvine excretions, through that of the carbonic acid and water, the proportion of the decomposed albuminous and fatty substance; we should perceive how much food of a certain quality is necessary for keeping the same animal, under certain circumstances, in the same state; and what is the effect of an increase of food, according to the desire of the animal, on the weight, on the excretions, on quickness of movements, &c.—similar observations, although they would seldom have the same accuracy, might be made also on ourselves.

After having shown the necessity of enlarging our knowledge concerning the state of health, Beneke proceeds to the duty of giving the history of cases of disease, which he divides into four parts:

* *Die Verdauungssaeft und der Stoffwechsel.* Milan und Leipzig, 1852.

"I. The account of the *development of the disease*—given in so accurate manner, that, by an accumulation of many cases, the *etiology of certain classes of disease*, principally of the so-called dyscrasic states of the blood would become more elucidated.

"II. Description of the *present state*, in which not only the physical, but also the physiologico-chemical symptoms must be included.

"III. *Plan of treatment*, according to rational and empirical principles, from which is to arise the *science of dietetics and pharmacodynamics*, based on *physiologico-chemical investigations* or on *undeniable empirical facts*.

"IV. *Carefully given daily records*, not only of the apparent general course of the disease, but also of all the external conditions, of all the ingesta and principal egesta, as well in quantity as in quality." (p. 52.)

1. In the history of the development of the disease, Beneke divides the influences which have acted on the patient into three classes: (a) *the more remote*; (b) *the less remote*; (c) *the proximate influences*.

(a) *The more remote influences* concern the parents and grand parents, their occupation, manner of living, the atmospheric influences under which they had been, the diseases they have suffered from, &c., &c. We agree with Beneke, in attributing much more importance to this part of the history of diseases than it is generally considered to deserve. Not only in the so-called hereditary diseases ought we accurately to examine into the state of health of the parents, but in every single case; we must try to find out why in one child of the same parents the tuberculous, in the other one the gouty, diathesis is developed, while a third child may enjoy so-called full health—what may be the state of children whose father was suffering at the time of procreation from syphilitic disease of the bones, while the mother was of a gouty disposition or affected with osteo-malacia?—what was the state of health, the manner of living, the residence of non-tuberculous parents whose children die of tuberculous phthisis, &c.?

(b) *The period of the less remote influences* commences with the birth. The infantile management, the food and treatment at different periods of life, the former and present residence, the manner of living, as well physical as psychical, the previous diseases the patient has been suffering from, should be taken into consideration. We need not dwell on these points; they are in a similar manner treated of in 'What to Observe at the Bed-side, and after Death, in Medical cases.*' We must mention, however, that Dr. Beneke does not forget to allude, under this head, to the very interesting subject, as yet only little investigated, in how far we are entitled to judge from the presence

* London, 1853. By the London Medical Society of Observation.

of a certain group of diseases on a certain state of the blood and of the whole organism—of a certain crisis, as Beneke expresses himself, and from this, on the absence of another crisis.

As a single instance, he mentions that those suffering from oxalic-acid diathesis are not liable to typhus fever; that we may conclude, therefore, from the previous occurrence of typhus, on the absence of the first named diathesis in the same period. The great importance of the establishment of many facts of this kind is striking enough; it would throw light on the essential nature of the different groups of diseases. It is a subject we must certainly aim at; but the history of medicine has also taught us, how careful we ought to be in forming a conclusion, lest it may prove fallacious. Although we do not think ourselves entitled to doubt the correctness of the instance adduced by Beneke, we are as yet unable to corroborate it, and we may refer to it to show the difficulty of establishing such a fact. Three times we have had an opportunity of watching, during an acute affection (twice of acute rheumatism, and once of severe angina tonsillaris,) patients whose urine usually exhibited a large number of crystals of oxalate of lime. In all these three cases, during the whole of the acute attack, and during the time of convalescence, not a single crystal of oxalate of lime could be detected; in two of the cases, they appeared again after some weeks; in the third case, no examination was afterwards made. Whether the disappearance of the oxalic acid during the acute affection depended on the increased aggression of the atmospheric air and living organs on the blood, or on the diminished ingestion of material during the time of pyrexia, or on both causes, is of no importance to the present question; but these observations show, that it would be erroneous to conclude, from the absence of oxalic acid during the course of acute diseases, that there is a want of susceptibility for acute disease in those suffering from the so-called oxalic-acid diathesis.

(c) The duty of describing the *proximate* influences or the exciting cause of the disease is a comparatively easy one after the investigation of the circumstances creating the disposition. We must in this manner gradually find the cause, why the same influence in one case produces pneumonia or bronchitis; in another, acute rheumatism; in a third, diarrhæa; in a fourth, ophthalmia; and why a fifth and sixth remain unaffected by it.

II. The general outlines which Beneke gives of the description of the *present state*, and of the value which he attributes to each symptom, fully show, that he is equally practised in the *physical* and in the *physiologico-chemical examinations*. Every physical symptom must be carefully observed; this is to be

done, however, not merely for the diagnosis of a physical change, but it must also help us in the solution of the principal question—the cognition of the forming material of the organism, i. e., the blood. The observation of those phenomena, from which we may judge of the amount of the typical energy of each organ, and of the whole organism, appears to him of great importance.

Concerning the *physiologico-chemical examination*, Beneke considers the analysis of the blood of less importance than the accurate examination of the process of metamorphosis of the quantity and quality of the ingesta and egesta. Such an examination of the present state, combined with the previous history, must lead to a more complete diagnosis than is generally made and must immediately guide us to a rational plan of treatment. In order to make Beneke's views better understood, we may be permitted to quote the outlines of a case described in the shortest manner. The patient is a young man aged 22 years, who has suffered for the last three days from pain and slight swelling of the joints of the hands and knees. Knowing this, Beneke inquires first for the more remote, then for the less remote, and then for the proximate influences. The patient is the son of a consumptive mother, who died eighteen years ago; his father is still alive, and is a healthy man. He was suckled by his mother. After being weaned, he had milk-and-water with bread for breakfast and supper; and after the 15th year, coffee, with bread-and-butter. Has always enjoyed his dinner; seldom missed meat, but has been used to eat, also, many potatoes and much rye-bread. He generally took much salt with his food; drank soft water; and after his 15th year, he had the allowance of a quart of small beer daily. He had been always very fond of sugar and sweet things. In his early years, the patient lived in the country (north of Germany;) later, he was employed in a sugar-house in London, where he was frequently exposed to intense heat, producing profuse perspiration. Has never been in poor circumstances. When a child, he began late to walk; has since suffered much from toothache (principally from the 10th to the 15th year;) has at present eight decayed teeth. When a child, he had also slight swellings in the neck. Has not been affected with typhus or ague; but has had measles (without epidemic cause) in the 6th year. As a boy, he always suffered frequently from epistaxis. On one of the last days before the outbreak of the disease, the patient took very large meals; and on the day previous to the appearance of the affection of the joints, he was exposed to a noxious cause likely to give origin to cold (working in a cold draught of air, with the upper part of the body sparingly covered.) The patient is

22 years old, weighs (without clothes) 140 pounds, measures 5ft. 4in. His body is moderately developed; muscles rather lax; bones thin; colour chlorotic; hair light; iris bluish gray; conjunctiva pure, slightly-bluish white. Glands of the neck slightly but not visibly swollen. A few spots of acne and ecthyma on the front and on the back of the trunk. Physiognomy in no respect characteristic; expression cheerful. During the consultation, patient appears anxious; rather hasty in his answers; the extended arm is slightly trembling. The pulse beats 110 times in the beginning, only 86 times towards the end, of the visit; is not very elastic, feels soft, and of middle size. The patient sleeps during seven or eight hours, but is awoke by every little noise. The physical examination exhibits: formation and motion (Sibson's chest-measure) of the thorax normal size and situation of heart normal, slight murmur with the first sound at the base (anæmic murmur is heard also in a slight degree in the vena jugular dextr.); diameter of the liver rather increased; sound of percussion perfectly dull. The tongue is red, with a thin, bluish-white covering on the surface. Appetite not altogether absent; taste acid after meals. The wrist-joints are slightly swollen, their temperature is increased, they are tender and painful. The quantity of carbonic acid expired is not measured. The perspiration constantly covering the skin for the last two days has a strongly acid reaction, and a faint smell of acetic acid; saliva slightly acid. Urine and fæces at the first visit not seen; during the following twenty-four hours, about 8 ounces of fæces, of moderate consistency and light-brown colour. Quantity of urine, 34 ounces, specific gravity = 1030; acidity (ascertained by neutralization) = -|- 48 (-|- 15—20 being the standard in health); high gold-yellow colour; on the bottom of the clear fluid, a sediment of lithates (soluble by warming), and some crystals of uric acid and of oxalate of lime (microscope); the quantity of urea (nitrate of) does not exceed the average; but that of lithic acid, sulphuric acid, and the earthy phosphates, is increased; no sugar is detected, but by boiling and nitric acid some albumen is precipitated. Patient has taken, during the last twenty-four hours: water, 48 oz.; milk, 10 oz.; gruel and barley-water, 10 oz.; bread, 10 oz.; butter, 3-4 oz.; mixtur. gummosæ, 6 oz.; with one drachm of nitrate of potash.

From the previous history, and the examination of the present state, Beneke concludes, in this case, on the presence of a complicated disorder of the blood. The scrofulous dyscrasia which the patient inherited is connected with an (indirectly) retarded metamorphosis of the nitrogenized constituents of the blood, and in the former diet of the patient several things were

present to promote this dyscrasia. The circumstance of his having been late in beginning to walk, the insufficient development of the bones, the morbid condition of the teeth, and the flaccidity of the muscles, all point to an abnormal loss of earthy phosphates, and therefore, also, according to Beneke's views, to an abnormal production of oxalic acid. The result of the examination of the "status præsens" shows an increased expenditure of nitrogenized material; the presence of albumen in the urine; an absolute increase of lithic acid; no absolute increase of urea, as only a part of the lithic acid reaches that stage of decomposition. The ingesta being diminished, we have reason to conclude that the proportion of the albuminates of the blood is abnormally augmented. The high colour of the urine, especially if present for a long period, indicates an impaired function of the liver. The quantity of sulphuric acid in the urine being only moderately increased, corresponds to only a slight degree of pyrexia and increase of change of matter. (Beneke holds the opinion, that the quantity of sulphuric acid in the urine, provided no combination of sulphur have been taken except in the food, is proportional to the intensity of the metamorphosis of matter.) The high degree of acidity of the urine principally depending on the acid phosphates, is frequently caused by an increase of the azotized components in the blood, and is, also, often a sign of the presence of a morbid excess of soda in the blood.

From all this we may conclude, with a high degree of probability, on an increase of the azotized materials of the blood; and, from the diet, we must consider this increase as an indirect one, occasioned by retarded metamorphosis of the albuminates. Concerning the inorganic constituents, we may deduce, from the acidity of the perspiration and of the saliva, and from the acid taste, an augmented production of organic acid, by which the presence of an abnormally great quantity of soda in the blood becomes very probable; the large proportion of salt previously taken, and the impaired expenditure of soda through the liver, make this inference still more plausible. The increased quantity of earthy phosphates points to the pathological production of oxalic acid, which is afterwards proved by the microscope. (p. 71.)

Our space does not permit us to give the whole semeiology of the case, with the diagnosis and the therapeutical indications founded on it. We will only add, that Beneke is not contented with the name attributed to such an affection, "*articular rheumatism*," as signifying only a single group of the morbid symptoms; but his diagnosis would be: *chronic disease of the blood at present augmented, (increase of albuminates and alka-*

line basis, diminution of earthy phosphates ;) articular rheumatism ; hyperæmia of the liver ; anæmia. (p. 73.)

We must bear in mind, however, that Beneke intends to give by this only the outlines of a case as it may be taken by men much occupied ; and he wishes merely to show how the process of disease is to be observed, and how the physiologico-chemical examination may help in the diagnosis and treatment. He has given a more elaborate case in an appendix to this work. It may be remarked, also, that in calling the adduced case of rheumatic affection a disease of the blood, Beneke must not be understood as if he was considering the ultimate cause of the disease to lie merely in the blood ; in other parts of the same work he has sufficiently shown that he is always aware of the intimate connection between the solids and the blood.

There are some expressions in the above case which appear to state as facts what are, as yet, merely suppositions ; but this we may attribute to the tendency of a concise style in writing. For instance, we meet several times with "abnormally increased production of oxalic acid," whereas the increased quantity of oxalic acid in the urine is no proof of its absolutely increased production, but only of the formation and excretion of some oxalate which has not been further transformed. "An increased production of organic acid" is another instance of the expressions we allude to.

Concerning the treatment, the diagnosis formed in the manner above described leads Beneke to the following indications :

"1. Diminution of the nitrogenized material of the blood (small venesection of about six to eight ounces, and exclusion of nitrogenized food ;) 2. Acceleration of the metamorphosis of the remaining nitrogenized material (increase of diuresis simply by drinking more water, and moderate doses of salts of potash, particularly the acetate.)"

He of course carefully avoids repeated or larger venesections ; he does not continue the low diet too long, nor the salts with an alkaline basis, especially soda, which already exists in excess ; he does not resort to frequent purgatives, by which, according to *Schmidt's* excellent investigations (Cholera, &c.) the albuminates of the blood are augmented. After having subdued the acute attack, Beneke would not permit too much nitrogenized food, and would warn his patient against taking those non-nitrogenized substances, by which the metamorphosis of the nitrogenized ones is most retarded (sugar, starch, &c.) he would combat the increase of soda, and the diminution of red bloodglobules, by mineral acids, iron, &c.

III. In the determination of the *therapeutical indications* in general, Beneke reminds us again to look for the origin of

disease either in disorder of food and its product the blood, or in some of the aggressive agencies.

As the organism cannot be retained in the state of health without a certain supply of alimentary material in the proper quantity and quality, the establishment of a system of *rational dietetics* must be considered as one of the *principal subjects* for inquiry. It must comprise :

- a. The consideration of all the articles of food ;
- b. Of the other influences of life, which all have a direct or indirect influence on the metamorphosis of the alimentary material.

We must therefore form tables showing the exact composition of all articles of food—the quantity of water, the quantity and quality of nitrogenized and non-nitrogenized, as well as of the inorganic constituents. Much has been done already, particularly as regards the organic ingredients ; but much remains to be fulfilled, and most in regard to the quality of the inorganic ones, which are of the greatest influence on the nutrition of the organism. We must further learn to fix what is the *proper quantity and quality for each individual case*. In doing so it is essential never to omit taking into consideration weight, measure, age, occupation, residence, with atmospheric influences, and principally also the typical power of the nervous system. It is doubtless the case that in a person measuring six feet, and weighing 170 pounds, more matter is decomposed within twenty-four hours, than in another only five feet high, and weighing 120 pounds, whose other conditions are, however, the same as those of the former subject. It is ascertained that two persons of the same height and weight, of whom the one is low-spirited and slow, while the other is cheerful, active, and quick in his motions, require different quantities of food, as the metamorphosis of matter must be slower in the former than it is in the latter. Every influence, whether psychical or physical, by the act of respiration is promoted, must increase the metamorphosis of matter, and must not be forgotten in regulating the diet. Concerning the *quality*, we must likewise choose it according to each individual case. We must learn at first, whether there is an acceleration or a retardation in the metamorphosis of a certain class of alimentary materials—in case of retardation, whether a direct or an indirect one—whether there is a plus or minus of phosphates, a plus or minus of soda, &c., &c.; and according to the result of these investigations we must prescribe the diet.

As another subject for inquiry, Beneke proposes the investigation of the action of those substances the use of which is not necessary for the maintenance of health, which may therefore

be looked at separately from the nutriments in the proper sense of the word—as coffee, tea, spices, wine, spirits, &c., &c. These *articles of relish* (*Genussmittel*) certainly exercise an important influence on the digestion and on the metamorphosis of matter. Researches of some value have been made already by Dr. Boecker, of Bon,* according to which most of these so-called excitants do not accelerate, but retard, the metamorphosis.

For the establishment of rational *therapeutic treatment*, besides the knowledge of the nature of the disease or of what is to be remedied, it is necessary to know the action of the remedies. To achieve this, Beneke seems to put little confidence in the statistical method as recommended by Louis and Wunderlich, but entertains much hope in the *physiologico-chemical experiment performed on the most healthy individuals*. It would lead too far to expose here the preferences and deficiencies of the one method and the other, but we must think the combination of both of them indispensable for the formation of the rational pharmacology.

IV. We need scarcely remark on the necessity of not only elaborate, but frequent records of all the symptoms in each case, the condition of all the egesta, and the kind of ingesta.

After having thus given the outlines of what is wanted for the promotion of a rational system of pathology and therapeutics, Beneke briefly, but with much clearness, alludes to the manner in which the different observations and examinations are to be conducted. It cannot, of course, be required from a much-occupied medical man, that he carefully examines and records all the different points of importance; but most of us might have always one or two cases in hand, of which we could make out the history of the development of the disease; in which we could form besides the physical, also the physiologico-chemical diagnosis, in which we could daily note the progress of the physical symptoms; the quantity and quality of the ingesta, the atmospheric influences, the quantity of the urine, its physical quality, its specific gravity, reaction, its degree of acidity or alkalescence; the approximative quantity of lithic acid and urea; of earthly phosphates, of sulphuric acid and of oxalate of lime; the presence or absence of sugar; the quantity and quality of the alvine evacuations; that of the perspiration, and also the reaction of the saliva, if possible, at different times of the day.

We must not forget to mention that Beneke gives very complete schemes for the examination by which the development

of the disease and the *status præsens* may be investigated; he gives very practical diagrams for taking record of the most important points in the course of the disease, and another scheme for the *post-mortem* examination. He proposes also the outlines of a useful plan, according to which the chemical examinations may be performed. We will, however, at present not dwell any longer on these points, as we hope to be able to return to them ere long, when analyzing the plan for the physical as well as physiologico-chemical examination which is to be adopted by the Society for Clinical Observation in Germany.

True it is that many of the most important points cannot be easily observed in private practice, but doubly great is therefore the duty which devolves on the medical staff of hospitals. No hospital ought to be without a laboratory, and without the instruments necessary for accurate examination.

But the more we enter into the field of scientific examination for the promotion of rational medicine, the more we must see how much is to be done, and how little a single individual can do; with a full conviction of this fact, our author has urgently invited his colleagues as well in Germany as abroad to unite with him, to distribute the labor, and to work according to a certain plan. And we are happy to say that his exhortation has been efficient, as at the last *Versammlung deutscher Naturforscher und Aerzte*, in Wiesbaden, in September, 1852, by the co-operation of professor Vogel, of Giessen, and Professor H. Nasse, of Marburg, a Society for Clinical Observation, or verbally translating the rather long German title, "A Society for United Labors towards the Promotion of Scientific Medicine," has been formed, which counts already among its members many well-known German, and some English names. HERMAN WEBER.—[*Brit. and For. Med. Chir. Review*.

Failure of Vaccine. By J. A. HINGESTON, Esq.

The frequent failure of vaccination is now so generally admitted, that statistical proofs are not requisite in order to establish its truth. People look upon it as an equal chance, whether those who have been vaccinated shall be able to resist an attack of the small-pox or not, should they be exposed to it; while some go so far as to surmise, hastily and rashly enough, that vaccination is all but useless. A few vote for a return to the old variolous inoculation; and a few, still more inconsiderate, boldly declare themselves in favor of the small-pox itself, as the only and the surest guarantee of their safety. There is, of course, a great deal of exaggeration and misrepresentation in

the expressions of this sort, and much more is affirmed against the non-protective agency of vaccine lymph, than, as is usual, a dispassionate inquiry into all the circumstances of the case will justify or imply. The broad and undisputed fact of the actual diminution of small-pox since vaccination has been introduced and practised, is alone sufficient to contradict these wild notions, and to refute the vulgar prejudices afloat upon the subject. For, even during an occasional outbreak, the disease is nothing now to what it used to be formerly, when the old inoculation, which is itself not free from risk, was the only obstacle opposed to its incessant and alarming encroachments. At that time, it was a real plague of terrible malignity, whereas it is now a comparatively mild and transient epidemic. This well-known fact is a satisfactory answer in favor of the protective influence of vaccination, which, if not universally, is at least extensively serviceable and effectual. To medical men, the occasional failure of vaccine lymph presents itself in a very different light from that in which it is viewed by the public at large; and with vaccinators in particular it is a topic of the deepest interest, which they endeavor to explore to the best of their abilities, and in every possible direction. The various points of inquiry offered to their notice, may be reduced to the few following items:

I. A chief cause is the capital oversight on the part of the vaccinator himself, in transferring lymph from an imperfect pock. There is no doubt that this oversight or carelessness is a copious source of the quantity of bad lymph in circulation, as well as of the ultimate failure of vaccination as a protective agent. For it is a maxim in vaccinating, never to transfer lymph from a pock in the slightest degree abnormal, nor from a normal pock in a constitution evidently disordered or unsound; because, if it be so transferred, the inevitable result will be abnormal pocks and inefficient lymph. It is impossible to restore contaminated lymph to its primitive purity; no subsequent care in its propagation can ever recover its lost or defective virtue. Having once become devious or degenerate, it continues to descend, both degenerate and devious, until its power becomes extinct, and it fails to propagate itself, even in a degenerate form. This glaring fact is so unquestionable, that it is necessary to bring it forward, and place it first and foremost among the causes of the failure of the vaccine lymph.

II. Another main cause of failure is, we must candidly own, carelessness in the act of vaccinating. Not only is the good quality of the lymph used not strictly inquired into at the outset, but, what is more unpardonable, the mode of operating is not exactly performed. There is a right and a wrong way of

vaccinating, just as there is a right and a wrong way of amputating, bleeding, or tying an artery. Every surgeon knows the value of performing an operation rightly, and the evil consequences to be apprehended from operating wrongly. It is the same with vaccination, which, in its vital results, takes rank among the capital operations of surgery, and requires, for its proper performance, an intelligence as clear and a hand as dexterous as ever fell to the lot of the best of surgeons. By neglecting, or by not being aware of, the precise way of operating, it happens that so many vaccinations turn out, either eventually or immediately, inefficient. The puncture is made too wide or too deep, or irregular; each of which faults materially affect the shape and character of the vesicle; or else, if it have been properly performed, and the lymph effectually inserted, it is not punctually watched from day to day throughout its progress. Success demands constant vigilance. In performing the operation, the cuticle alone must be raised, and the cutis beneath exposed, but not wounded, or wounded as little as possible. The cutting, or rather the scratching, of the cuticle, should be done lightly and delicately, with the least possible irregularity, and over the smallest extent of surface consistent with the application or insertion of the lymph. Its course must be watched and noted on the third, eighth, and fourteenth days in particular; and no case can be pronounced safe, unless every step in its progress have shown itself to be regular, critical, and complete. It is very necessary to point out these causes of failure; first of all, because they are frequent, and seem, in a great measure, to have lost of late years something of their due weight and importance in the estimation of medical men. From the medical pupil never having been hitherto distinctly educated on the subject of vaccination, he is naturally liable to fall into mistakes through inexperience, which can only be overlooked out of tenderness to his unavoidable ignorance, in consequence of his never having been taught, *ex officio*, how to proceed with scientific precision. But, upon public grounds, inexpertness is inexcusable, and cannot be too severely censured and condemned.

III. Owing to a want of attention to the proper time for taking it, the lymph is transferred too soon or too late, and the obvious consequence is, that unripe or effete lymph is procured and propagated. Of course such lymph is weak, or altogether inefficient. It ought never to be taken away earlier than the eighth, and sometimes not before the ninth day. At too early a day it is ichor rather than lymph. Again, it may be taken away too late; that is to say, after the ninth day, when it is becoming purulent, and the vesicle is turning into a pustule.

The exact time is eight times twenty-four hours, dating from the hour of the day of vaccinating. The lymph will remain genuine till the ninth day is over, and sometimes even till the tenth; for occasionally the whole course of the pock is stage by stage a day after its time: thus, the inflamed point of the third day is delayed till the fourth, and the inflamed halo of the ninth does not reach its climax till the tenth, and so on. This delay in the progress of the symptoms is regarded as a favorable prognosis; and certainly, in cases disposed to reject the specific action of the virus, the inflammation and vesicle, besides their being abnormal, rise and disappear much too quickly. Lymph taken after the tenth day is universally reprobated; and none but such as are miserably ignorant or indifferent would ever think of transferring it at this late epoch.

IV. It is advisable never to transfer lymph that has become purulent. A prudent vaccinator never does. Not that pus *per se* invalidates the lymph any more than blood does; for it may appear on the ninth day mixed with the genuine lymph: nor would its accidental presence alone weaken or destroy its efficiency; but it indicates more inflammation than is necessary for the production of the true vesicle, and it moreover signifies the co-existence of an inflammation different in kind from that which produces the genuine lymph. After the tenth day, pus is sure to be mixed up with the contents of the vesicle, which is thenceforth rapidly declining, and ceasing to be prolific. A blow or injury of the vesicle will hurry on the appearance of the tenth day, and consequently render the lymph unfit for transfer. Sometimes the first stages of the pock are suppressed, and then it breaks forth all of a sudden into its normal appearance of the eighth day: but such a pock is invalid.

V. Many parents, especially in the upper classes of society, object to lymph being taken away from their child's arm, lest, as they fancy, it should weaken its ultimate effect. A prejudice of this kind would extinguish vaccination altogether. Nevertheless, there are not wanting acute observers who consider that, if all the vesicles be exhausted of their lymph, the prognosis is thereby rendered unfavorable, and, moreover, that such exhaustion causes local irritation and disturbance. Indeed, it is affirmed that convulsions and death have ensued from such a proceeding. Be this as it may, it is certain that every one concurs in the propriety of leaving one vesicle intact, for the express purpose of judging of the normal progress of the pock from first to last; and no one would, we should suppose, be so imprudent as to irritate the exposed surface with the lancet or an ivory point, merely for the sake of draining it of every drop of its lymph. Yet it may be well to bear in mind,

that one of the causes of the failure of vaccination as a protective agent, is imputed to thus draining the exposed vesicle—an error which, if it be one, it is easy enough to avoid.

VI. Lymph should never be taken from any but a primary pock. The pock resulting from a revaccination is not to be relied on. If there is any reason to doubt the vesicle being a primary one, it ought to be rejected.

VII. The actual or suspected presence of scrofula, syphilis, porrigo, etc., is quite sufficient to condemn the subject of it as a legitimate source of fresh lymph. A vesicle on such a subject, however perfect, should be allowed to die out by itself. Indeed, any eruption of the skin is a barrier against vaccination, except under the imminent risk of catching the small-pox; but as to transferring matter from such a constitution, it is out of the question.

VIII. The sudden accession of constitutional disturbance during the pock, or the increase and extension of the inflammation around it, should render the validity of the operation more than doubtful. A second vaccination should be attempted within six months from the first, and the sensitive feelings of mothers opposed to such a proceeding should be steadily overruled. Many a vaccinator, not satisfied with a first pock, would wish to repeat it until he felt confident in the normal character of the last produced. But he is seldom permitted to adopt this wise precaution: and this may be numbered among the causes of failure in vaccinating. The forms of society tie our hands, and force us to comply with its own conventions.

IX. The number of vesicles is said to modify the result of vaccination. Some maintain that one alone is sufficient, while others declare that several are requisite, to insure efficiency. There is no proof to support either the one or the other of these two assertions. Pathologically speaking, it would seem, *à priori*, that the existence of a single good pock manifests the saturation of the system with the virus as entirely as any multitude of them would do. But as a matter of fact, there is nothing to help us in arriving at a decisive conclusion respecting it.

X. The most popular notion concerning the failure of vaccine, is that derived from its supposed contamination by passing through so many successive generations of mankind. This is the most popular belief, and the one which appeals most readily to the greater number of minds. Nevertheless, of all the different reasons alleged for accounting for the acknowledged fact of deteriorated lymph, this would seem to be the most untenable. It is an hypothesis built up in the face of facts that directly contravene it. For genuine lymph, dating from the time of its first introduction, continues to produce a genuine

vesicle from a genuine vaccination performed on a subject in a genuine state of health, as exactly now as it at first produced it under the hands of Jenner himself. Lymph, fresh from the cow, is certainly more energetic than such as has been long ago in use ; but the pock that it produces is identical, and its immediate and ultimate effects upon the constitution are the same. This hypothesis, likewise, fails in explaining the failures that happened even in persons vaccinated by Jenner. We must look for the cause of deteriorated lymph in other sources than this.

XI. Dry lymph is said to be another cause of failure, and the use of liquid lymph is enjoined. No doubt, liquid lymph, and vaccination from arm to arm, is always preferable, but it cannot always be brought about. Besides, there is no solid ground for supposing that the virus, when dry, upon points, between glasses, or in a crust, is not equally as active as when it is still moist and quite new. Every vaccinator is aware of this. Lymph may be taken to India and brought back again, and still prove energetic and effective. After all that has been said, the most apparent cause of failure is that of vaccinating with lymph taken from an irregularly formed vesicle, or from an unsound constitution, or at a wrong date of the pock. Independent of all other causes, this inadvertence or carelessness cannot be overlooked. The signs of an obnoxious or doubtful vesicle ought to be closely studied. They are both constitutional and local. The health may be cachectic, the child suffering from debility, or some congenital infirmity or malformation. In these instances the pock is worthless. Regular vesicles may coexist with an irregular one, which alone is sufficient to invalidate the integrity of all the rest—unless its irregularity can be accounted for by something purely accidental. The following eight points should be impressed on the memory:—1. Irregularity of form throughout all the stages of pock. 2. The vesicle not being round. 3. The color of the inflammation not being fresh and rosy, and that of the vesicle not of a pearly whiteness. 4. Its fluid contents being straw-colored, instead of colorless and transparent ; or else being purulent on the eighth day instead of the tenth. 5. The areola, or surrounding inflammation, not being defined and circular, but, on the contrary, irregular, confused, and, as it were, blended with the vesicle, whereas it ought to be distinct from it. 6. The crust forming prematurely, looking pale or yellowish brown, and being friable and gritty, instead of dark, round and compact. 7. The vesicle forming on the fifth day, and rising up of a conical shape, or festering like a small pustule. 8. The areola becoming efflorescent, or scurfy, or shooting out into a

figure like the margins of a map. All such cases should be rejected without hesitation, and a second vaccination should be earnestly advised. It is by vaccinating from vesicles, more or less imperfect according to this description, that so many failures are recorded.—[*Assoc. Med. Jour.*

Relations of Vaccination and Inoculation to Small-Pox.

From a very interesting and valuable paper upon this subject, communicated to the Epidemiological Society of London, by Dr. Waller Lewis, F. G. S., we take the following extracts. The author commenced by considering the various questions in regard to this disease, of which an elucidation is now anxiously called for. He then stated numerous cases illustrative of the protective power of vaccination, and of the superiority of vaccination to inoculation. He then referred to the probability of the fact that measles are rendered milder by vaccination.

“Joler has described an epidemic of measles that took place in Retzat Circle, in Bavaria, in the district where he himself resided. He says that the disease was much milder among the vaccinated than among the unvaccinated; 15 in 52 died among the non-vaccinated, while barely 1 in 300 died among the vaccinated, showing that measles was 86 times more fatal among the former than the latter.”

Examples of imperfect vaccination were then dwelt upon, and the author expressed his opinion clearly and decidedly that where well-marked cicatrices were not left, the operation should be accounted a failure, and should be repeated, although he owned that this was not the opinion held by many German and English physicians.

Vaccinating from re-vaccinated persons, from those who had been inoculated, and from such as had previously had small-pox was strongly denounced, as vaccinia must be extremely modified in such cases. The author added:

“When we interest ourselves strongly in the propagation of vaccination, we must guard ourselves from furnishing arms to its adversaries. And is it not furnishing them with arms to employ a virus of which we are not certain?”

A most interesting collection of cases was then read, in which small-pox had attacked the same individual three or four times; among others, the following, that had come under the author's own attention, was narrated:

“Robert D., a tradesman, living in North Audley street, had small-pox the first time at the time of his birth, his mother suffering from it at her confinement. He was attacked with the disease a second time

when a boy at school, between nine and ten years of age. When eighteen years of age, he took it, for the third time, from his sister, who died of it. All the attacks were severe, but the last the most so. He lost his hair and his nails, and the skin of his feet; he was blind for several days, and his life was despaired of. However, he is still alive, and not much disfigured. He was never vaccinated nor inoculated. I believe, if again exposed to the disease, he will take it again."

Cases were then adduced to show that several members of the same family appear sometimes to show great susceptibility to take the disease. The following curious case of small-pox in the lower animals was then adduced, the author adding, that any similar well attested cases would be very valuable additions to the facts collected by the Society on this subject:

"The following case was related to me by a lady of rank, on whose veracity I can place the greatest reliance. Some years ago, just after her confinement, she was seized with small-pox. It became necessary to have her breasts drawn, and, as no child could be obtained, recourse was had to a puppy, which answered the purpose. At the usual time the puppy sickened, and had the disease known by the name of the "distemper." It is said that vaccination, when successfully performed on puppies, will almost to a certainty prove a prophylactic against distemper."

Then followed some interesting cases of individuals who could be neither vaccinated nor inoculated. The last cases adduced were of individuals who appeared to have perfect immunity from small-pox.

"I have detailed the case of Robert D., who evidently possesses a strong innate susceptibility to the action of the small-pox virus, as shown in his having already taken the disease three several times. I have now to draw your attention to a case the most directly opposite to this. Strangely enough, it is that of his own brother, Thomas. From the elder brother, Robert, as well as a sister, having taken the small-pox, the parents believed that all their children must take the disease, and refused to have the subject of this case vaccinated or inoculated. He was accordingly exposed when a child to the contagion, lying in the same room with his sister, while she was suffering from the disease, as well as waiting on his brother in his second and third attacks. Although since that time he has been several times exposed to the contagion, he has never felt the slightest ill effects from it. . . . Examples of persons possessing a natural immunity from the disease are rather numerous. Dr. Jackson, of Philadelphia, saw a man at the small-pox hospital, engaged in laying out and burying the dead, who had never had an attack of the disease. He had been frequently inoculated and vaccinated, but always unsuccessfully. Van Swieten speaks of a physician, 70 years of age, who had practised through numerous epidemics of the disease, but had never taken it. Diemerbroek states that immunity from small-pox

was a privilege of his family. It was possessed, he asserts, by his grandfather, grandmother, his father and himself."

The author drew the following deductions from the cases adduced :

"1. That vaccination is a most eminent protection against small-pox.

"2. That when perfectly performed it is almost, and, in some instances, more protective, than inoculation or small-pox itself.

"3. That it appears to render some exanthemata, *e.g.*, measles, milder than they would have been otherwise.

"4. That neither vaccination, inoculation, nor small-pox, guarantees the individual, in every instance from small-pox.

"5. That small-pox attacks some persons three times, or oftener.

"6. That there exists certain individuals who have perfect immunity from vaccination, inoculation, and small-pox.

"7. That great susceptibility to, or perfect immunity from, small-pox, is sometimes found to be common to several members of the same family."—[*Western Journal of Med. and Surg.*

On the Influence of Noxious Effluvia on the Origin and Propagation of Epidemic Diseases.

Although some diversity of opinion prevails among medical men in reference to epidemic disease, especially on the subject of contagion, all are agreed as to the noxious influence of overcrowding, defective ventilation, and other similar defects, prevalent in populous districts. It has occurred to me, that, without entering into the wide field connected with the nature and operation of noxious effluvia in general, it might not be altogether unprofitable if some elucidation of the facts which have fallen under my observation, both as regards the causation of, and exemption from epidemic disease, were laid before the members of this Society. No one is more ready than myself to subscribe to the doctrine so well enunciated by my friend, Dr. Carpenter, in his valuable paper "On the Predisposing Causes of Epidemics," that it is not simply the collection and tabulation of facts, nor even mere empirical generalization, that will suffice ; it is the principles and laws springing out of them which are demanded, if sanitary investigations are to be raised to the rank of a science. But fully recognizing this as constituting the great aim and end of all these researches, and not forgetting the large amount of practical knowledge acquired of late years, it yet appears to me that there is abundant room and ample reason for elucidating evidence. Many points of prime importance to the public health as to matters of fact are still in much uncertainty. Doubts relating to agents assumed

by sanitary inquirers to be deleterious still linger in the profession, and by no means only among its least distinguished or influential members. The exact operation of animal effluvia, of a cesspool atmosphere, of excessive moisture—conditions often combined in the miserable courts and alleys of our large towns—is by no means fully ascertained. Extended inquiries of late years have abundantly proved that the same deleterious agents operate as predisposing causes in regard to the whole class of zymotic diseases; that what will develop the exciting cause of fever will also develop scarlatina, small-pox, diarrhœa, or cholera. So certain and notorious is this fact to those who practise among the poor, that before the outbreak of any epidemic, knowing where the predisposing causes are rife, they can foretell the precise localities where it will occur, nay, even name the alley or point to the exact house that will suffer. Such considerations have long induced me to conclude, that in regard to zymotic affections, the predisposing are infinitely more important than what are called the immediate or exciting causes. In regard to low fever, for example, it is certain that its efficient cause, the *materies morbi*, is never absent from London and other large towns; and yet it is rarely, many would say never, developed, unless there be superadded to it some predisposing cause. So true is this, that we not only daily see in the metropolis and elsewhere hundreds and thousands of persons living in the front streets exempt from typhoid fever, while the inhabitants of the wretched courts behind are scarcely ever free from it; but if by chance a given number of persons are planted in the very centre of an epidemic district, but freed from the predisposing causes of zymotic affections, they also, as a rule, will still escape.

• “On the Influence of *Human Effluvia*.—According to my observation, the most injurious of all the causes operating on the diffusion of epidemic diseases, are the effluvia proceeding from the human body, particularly from the lungs and skin. The special deleterious agent consists of the effete, and, as it has been proved experimentally, highly putrescent organic matter, mingled with the expired air. That it is, when reintroduced into the living body, highly injurious, might be inferred from the very fact of the careful provision made by nature for its incessant elimination from the system. That it is small in amount is no objection to the intensity of its action; for, to the physiologist it is well known that a minute quantity of a powerful agent—the putrid matter introduced on the point of a needle in the inspection of a dead body, a single drop of concentrated prussic acid placed in the mouth of an animal, is sufficient to destroy life. It is in overcrowded bedrooms, in unventilated schools, workhouse dormitories, &c., that this effete matter taints the air, and, entering the blood, poi-

sons the system. Although there is a great diminution in the amount of carbonic acid in the air evolved in the lungs, still the evil, quoad the development of fever, scarlatina, cholera, and so forth, depends on the organic, not the chemical products of respiration."

The learned author referred to some experiments proving the truth of this assertion. He then continued—

"It is, however, familiar to all practitioners, that human effluvia specially exhibit their poisonous influence when either multitudes of human beings are crowded together, or where a smaller number are placed in confined and unventilated sleeping places. Many instances of the influence thus exerted on all kinds of epidemic disease, have come under my notice, but only a few illustrative examples can here be adduced. The following case illustrates the effect of overcrowding in respect to cholera. During the epidemic of 1849, the inmates of a reformatory establishment for young women suffered intensely from the pestilence, 40 out of a total of 96 being attacked, and 15, or rather more than 15 per cent., falling victims to the disease. Now, these poor sufferers were previously in perfect health; they were well fed, well clothed, and carefully attended; but the dormitories were low and much crowded; the windows, for the sake of seclusion, were partly closed up, thus greatly interfering with the ventilation. After a careful investigation, I could detect no other cause than this for the sudden outbreak occurring at a period when there was little cholera in the neighborhood. As regards the influence of overcrowding in the development of low fever, I may appeal to the experience of every medical practitioner whose duties call him much among the poor. It matters not whether we speak of the closely packed common lodging-house, of rows of houses built back to back, of the small, unventilated, and often single sleeping apartments of the mechanic, or of the ill-built cottages in rural districts, with their one bed-room, overhanging thatch and small lattice, wherever, either from the presence of numbers or the absence of ventilation, you have the fetid sickening air generated by human effluvia, there assuredly you will find fever. Although observed especially among the poor, fever, as it occurs in this country, is not especially dependent on poverty and destitution. Want may, indeed, aggravate the evil, and actual famine, as we unhappily saw a few years ago in Ireland, may give immense development to typhus; but that persons well fed, living in comfort, and strong in health, may suffer severely from low fever, is shown by a large experience. One of the best examples, perhaps, is furnished by the sailors belonging to the collier vessels frequenting the Thames. These men, as a body, are in the prime of life, robust and well fed; but as I found by examining many of these vessels, the place where they sleep—the forecabin—is excessively small and confined, and with this serious additional evil, that as the hatchway is usually flush with the deck, whenever there is much sea, it becomes necessary to close the hatchway, where the unfortunate sailors must be without any window, as if shut up in a close box. When, too, the vessels come to

London, as only one man is required to keep watch at night, all the sailors are crowded at the same time into their closely-packed berths. Some years ago the attention of Mr. Busk, the distinguished surgeon of the seaman's hospital ship, was attracted to the large number of typhus cases which were admitted. In 1841, they amounted to 147; in 1842, to 167. It appeared that of all the vessels in the Thames, the colliers furnished the most fever cases. In investigating this question I could detect no other cause than the polluted air these men must have breathed in the confined fore-castle. That there is nothing connected with a sailor's mode of life to expose him to typhus, is proved by the experience of well-managed vessels, and, as one among the many proofs which might be adduced, I may mention that Mr. Clark, who has made ten voyages to India as surgeon in Messrs. Green's fine vessels had never had a single case of typhus."

The author, after referring to the very great improvement in the health of those of the working classes who inhabit the model lodging-houses erected in different parts of the town by the Society for the improvement of the Dwellings for the Laboring Poor, and to the highly satisfactory working of the admirable act carried by the exertions of the Earl of Shaftesbury for controlling common lodging-houses, said that his own experience of the deplorable conditions of these abodes corroborated the statements of Capt. Hay; all tended to show that such pestilential places were the habitats of disease, and the cause of enormous expense to the rate-payers.—[*London Medical Times and Gazette*.

Aphonia of twenty months standing, relieved by Iodine Inhalation. By EDWARD B. STEVENS, M. D., Lebanon, O.

In a communication to the American Medical Association, in its Vol. of Transactions for 1850, Prof. Pancoast has given the record of two cases of loss of voice. The one of six, the other of seven months standing, both cured by inhalation of a dilut. colorine vapor.

In connection with these cases, Dr. Pancoast remarks: "The form of aphonia here alluded to, is that which many practitioners must have met with, following an ordinary cold, without leaving any perceptible organic lesion in the pulmonary apparatus. The voice is reduced to a faint hoarse whisper, distinguishable only at the distance of a few feet, and at continued attempt to talk, though it gives no pain, becomes quickly attended with a feeling of fatigue, as though there was some obstruction to the passage of the air through the larynx. In breathing merely, there is little or no difficulty, in these cases, as the individuals are capable of undergoing considerable ex-

ertion without very unusual signs of fatigue. The difficulty has appeared to me to be in the paralyzed condition of the muscles of the larynx, whose business it is to dilate the rima glottidis, during the act of articulation."

The conclusion of Dr. Pancoast is, that such agent as will excite a healthy and proper degree of stimulation in the affected structure ought rationally to restore the power of articulation. He consequently used the dilut. chlorine vapor, with entire success in the two cases referred to,—at the same time suggesting that iodine, or other similar agents would doubtless produce a similar effect.

The following case of this kind lately occurred in my practice, chiefly remarkable from the long duration of absence of voice, being twenty months, in other respects similar to those related by Dr. Pancoast.

April 6, 1853.—Miss —— applied for medical advice and treatment, in a case of loss of voice, of twenty months standing, supervening upon a slight attack of influenza. Has been subject to brief attacks of hoarseness, lasting for a few days at a time, for several years. General health delicate. Since the present attack, has been subject to a great variety of treatment, including the application of nit. silv. in strong solution, within the larynx by means of the sponge probang. Nothing, however producing any effect upon the voice. I find upon careful examination, no especial evidence of disease in the fauces; there is an entire inability to produce sound of any description with the proper vocal organs; all attempts at speaking are made with the lips—*whispering*. But not being able to divest myself of the idea that a follicular inflammation of the throat and bronchial tubes was the cause of the mischief in some way, I commenced the treatment by directing the inhalation of nit. silv. prepared with the lycopodium, as an inpalpable powder, and inhaled by means of the apparatus introduced by Dr. Ira Warren. This treatment was faithfully persevered in for one month, with no better results than the previously tried remedies.

May 7.—Acting upon the idea suggested by Prof. Pancoast of paralysis of the muscles of the larynx, I now determined to try the iodine vapor. I accordingly selected an apparatus, consisting of a metallic vase or urn, with a close fitting cover, flexible tube, and mouth piece attached, (used some years since for breathing medicated vapors in the treatment of consumption.) And directed my patient, after filling the vessel half full with hot water, to drop in 20 drops *tinct. iodine*, and inhale the vapor produced by heated water. Inhalation to be repeated once to thrice daily, according to the irritation or

effects, otherwise, produced. The first inhalation produced great nausea for a short time, and copious bloody expectoration but accompanied by an almost immediate, though partial restoration of voice. The dose of iodine was directed to be reduced to 15 drops; and thereafter no unpleasant effects were produced. The voice continued to improve steadily under this treatment, until at the end of a week it had acquired the natural fulness and distinctness of tone.

June 15.—More than a month has elapsed since the restoration of voice; it continues distinct and natural.—[*Western Lancet*.

Aneurism of the Supra-orbital Artery cured by the Injection of the concentrated Per-chloride of Iron. By M. DESLONGCHAMPS.*

At a meeting of the Surgical Society of Paris, March 30th, 1853, M. Larrey presented the details of a case of aneurism of the supraorbital artery, communicated by M. Deslongchamps, a military surgeon.

A patient of the latter presented a tumour upon the supra-orbital region of the size of a pigeon's egg, in which a regular pulsation was very manifest. The pulsation was increased by pressure above the tumour, but disappeared when pressure was made below or directly upon it.

M. Deslongchamps was convinced that the tumour was of an aneurismal nature, and as its locality was favorable for that method, he determined to treat it by compression. A pad kept in place by a watch-spring, compressed the artery for twenty-five days, but the tumour did not diminish. M. Deslongchamps then resolved to employ the plan indicated by the experiments of M. Pravaz.

An aid obliterated the course of the vessel above and below the aneurism, and the operator made a minute puncture with a bistoury in the internal portion of the sac. A syringe was introduced into the sac, and about ten drops of concentrated per-chloride of iron was injected. In three minutes the tumour became hard, and no pulsation could be detected. The syringe was withdrawn, and not a drop of blood escaped.

The following day there was considerable inflammatory swelling, which was combatted by resolvent applications and repose. The second day the tumour had returned to its original volume; pulsation could no longer be detected, and after seven or eight days, during which there was a slight sero-purulent discharge from the little wound that had been made, the tumour gradually diminished in volume; about one month after

the operation nothing could be seen of the aneurism except a slight redness and thickening of the skin.—[*L'Union Médicale. Virginia Med. and Surg. Journ.*

On Elective Elimination by the Salivary and other Secretions.
By M. CL. BERNARD.

In this paper M. Bernard calls attention to the fact, that some of the secretions rapidly eliminate certain substances, while other substances, equally soluble, are either eliminated much more slowly, or not at all. He relates the results of a series of experiments, in which iodide of potassium, iodide of iron, lactate of iron, cane and grape sugar, and yellow prussiate of potassa, were injected into the veins, and the various secretions then tested for their presence.

Of these, *iodide of potassium* appeared, at latest, in from 30 to 40 seconds in the saliva, and was also rapidly observed in the tears and pancreatic juice. It required more than an hour to become detectable in the urine or the bile; and if injected in very small quantities, was not found in these at all. Introduced into the stomach, and especially fasting, it was found in the saliva in $1\frac{1}{2}$ minute. The *yellow prussiate of potassa* was not discernible in the saliva, while in 7 minutes it was found in the urine and abundantly eliminated; the serum of the blood also exhibiting a notable quantity in an hour and a half. It was also found in the bile, while, although it was thus circulating in the blood, no traces of it could be found in the pancreatic juice. *Grape and cane sugar* never passed into the saliva or pancreatic fluid, while it was manifested in the bile and urine, though less rapidly than the prussiate. As various authors state they have detected sugar in the saliva in *diabetes*, the author examined that of several such patients under M. Rayer's care. In none was sugar detected, although the bronchial mucus and sputa evidently contained it. The *mammary gland*, which, in the normal state, contains the sugar of milk in its secretion, refused passage to grape or cane sugar, even when these substances existed in large quantities in the blood. A saturated solution of *lactate of iron*, thrown into the veins, never gives rise to iron in the saliva; but when the iron is injected as an *iodide*, it obtains admission into the saliva, both the iron and the iodine being then detectable.

This expulsion of certain salts by this or that secretion, is not the only peculiarity the history of elimination presents. Some substances are eliminated rapidly and completely, while others remain within the tissues for a more or less long period. It is well known that certain of these, as mercury, antimony,

and arsenic, become localized in certain organs—e. g., the liver—and are then gradually eliminated: but it has not been noted that others, as the iodide of potassium, which are perfectly soluble, and remain soluble in the economy, wherein they circulate without enduring any accident, may remain for a certain time in the substance of the organs. Two or three weeks after iodide of potassium had been introduced into the stomachs of several dogs, and long after its supposed entire elimination by the urine, in which it had ceased to appear, it was found in the saliva and gastric juice. If, however, purgatives were employed after administering the iodine, it ceased to be detectable in a few days in any of the secretions.—[*Archives Générales*, from *Med. Chir. Rev.*]

Arsenic in Chronic Scabies. By PHILIP H. WILLIAMS, M. D.

CASE.—I was consulted, February, 1852, by H. B., aged 22, who stated that she had been suffering from an eruption during nine successive months. The description clearly indicated scabies; and the appearance on the arms, wrists, fingers, legs, ankles, and toes, left no doubt as to the nature of the disease. There were a few scratched spots on the face, which, although a comparatively rare seat of scabies, appears in chronic cases liable to be affected. The itching was, and had been, commensurate with the extent of the malady. Various applications had been tried without benefit. On the hands and toes several distinct pustules had recently occurred, which, in the words of Dr. Bateman, were “moderately inflamed round their bases, and matured and broke in two or three days; then ulcerated, with increasing pain and inflammation.” Hence it would seem that the decidedly purulent species (*scabies purulenta*) was supervening on the ordinary development. The pustules subsequently appeared on other parts, especially on the arms and thighs, and remained after the simple eruption had died away. The constitution, at the commencement of the attack, was stated to be good; and at the time the patient came under my notice, the health was not impaired. The catamenia were quite regular. I prescribed sulphur ointment, giving iodide of potassium and sarsaparilla in the usual doses, and compound rhubarb pills at proper intervals. This treatment was continued for a month, with very little success. I then ordered three minims of the liquor potassæ arsenitis to be taken three times daily, in camphor mixture. At the end of three weeks the face became quite free from all traces of the eruption, and at the end of the fourth week the disease was declining over the body generally. The mixture was omitted for a few days,

in consequence of slight headache, and was then resumed in the same dose, twice daily. In three weeks from that date, the patient was cured, with the exception of some of the larger pustules, which were not healed until a fortnight afterwards. Poultices of linseed meal were applied to them.

Up to the present time there has been no return of the malady.

A younger sister of the patient suffered at the same time from simple scabies, which yielded at an early period to sulphur ointment and tonics. A companion of Harriet B. also contracted the disease, which became chronic, and required the same arsenical treatment for its removal.—[*Assoc. Med. Jour.*

On the Treatment of certain cases of Rheumatism with Aconite.

By G. W. BALFOUR, M. D.

While the employment of aconite in rheumatic fever, where it is desirable speedily to affect the system, entails an amount of care and watching, from the powerful nature of the remedy, which renders it unsuitable for country practice, there are certain cases of sub-acute rheumatism for which it is most suitable. These are chiefly synovial in character, with a more or less marked febrile state of the pulse. Where that is wanting, as in more chronic cases, aconite is rarely useful. In less urgent cases of affection of only one joint, and in many cases of sciatica, regulation of the bowels, and the local employment of the aconite, are often sufficient.

As aconite, without being cumulative in its action—*i. e.*, it is never prolonged or developed after the immediate action of the last dose has ceased, yet has this peculiarity, that one dose paves the way for another, rendering its action more powerful often than was to be anticipated, the dose requires to be very cautiously increased, and very often gradually diminished. I have in one case gradually increased the dose of Dr. Fleming's tincture to ten minims thrice a day, and as gradually diminished it to one; still keeping up the same amount of action. The patient was much benefited, but not cured. Sometimes it produces symptoms of gastric disturbance, which, when they threaten, are best prevented by giving the aconite after a meal.

Diaphoresis was believed by Störk to be one of its principal actions, yet Dr. Fleming never observed it, and I have only seen it once; but in that case it was most copious, the whole bedding being saturated after each dose. The patient was not benefited by his perspirations, and on account of them the aconite was stopped. To obtain the curative action of the aconite, its physiological one requires to be fully developed—

the coincidence of the two is generally well-marked. It is best given in plain water; and for external use, its tincture may be mixed in various proportions with plain soap liniment. The presence of opium, so often combined with it, is unfavorable to the development either of its physiological or curative action.—[*Monthly Journal*.]

The Physiology of Sea-sickness. By MARSHALL HALL, F.R.S.

I have recently made a voyage from Liverpool to the United States, and I seized upon this occasion to study the physiology of sea-sickness.

All of the phenomena of this disease induce me to believe that the spinal cord is the nervous centre, and that the pneumo-gastric, diaphragmatic, intercostal, and abdominal nerves, are the eisodic and exodic nerves which, in connexion with this centre, present the origin and course of the catastatic and diastatic actions, in these circumstances.

It appeared to me that the rolling and pitching of the ship influenced especially the course of the blood in the spinal cord; when the vessel rises, the force of the impulse of the blood upon this organ is diminished; when it falls, the impulse is augmented. There is then constant change in the force of this impulse; from which results perpetual excitement and irritation of the cord, of the pneumo-gastric and diaphragmatic nerves, etc.

The movements of a carriage or of a swing, if they are long continued produce the same effects upon very susceptible persons.

The influence of the position of the body, by which the movements of elevation or depression are increased or diminished, is very remarkable. If a horizontal position in the direction of the axis of movement of the vessel is selected and is preserved, the traveller may escape sea-sickness, and it is only upon changing this position that he suffers the first symptoms.

At first he experiences an indescribable uneasiness, a faintness at the stomach; presently acidity, eructation, hiccough supervenes; these symptoms being produced by the influence of the pneumo-gastric and diaphragmatic nerves upon the secretions and movements of the stomach, of the diaphragm, etc. Almost simultaneously the patient becomes pale, and the action of the heart becomes feeble and irregular, and sometimes palpitations occur; this also is an affection of the pneumo-gastric nerve. One of my friends suffered for years from irregular action of the heart, after experiencing violent sea-sickness during a voyage of a few hours. Lastly, nausea occurs, and

distressing vomiting; the stomach is first emptied, and afterwards bile, mucus, etc., is ejected.

The phenomena are repeated by paroxysms; a suffocating sensation of heat compels the individual to throw off the clothing, the warmth of which had been previously agreeable; then vomiting takes place, the pores of the skin are opened and a cold perspiration occurs, which is followed by a sensation of chilliness. It is impossible to convey any idea of the complete prostration that the patient experiences during an attack of sea-sickness, both morally and physically. In one case, that of a young lady, this cruel disease terminated in death.

Before and after the paroxysms, the action of the pneumogastric nerve is defective; its influence as an internal excitor of the respiration, is imperfect; this state is relieved by the free exposure of the face, and even of the hands, to the open air, and especially to the fresh wind, and by strong voluntary efforts of inspiration.

If the sea becomes calm, one becomes habituated to a slight movement, but if a storm arises, he is presently sick again. I had enjoyed six days of physical comfort, when we experienced the effects of a gale; I suffered from sea-sickness for forty-eight hours.—[*Comptes-rendus de l'Académie des Sciences. Virginia Med. and Surg. Jour.*

Therapeutical Record.

Anchylosis.—In partial anchylosis of the knee-joint, Mr. Hancock (Lancet, Jan. 29) has met with great success from forcible extension; while the patient is under the influence of chloroform. Only in one of six cases did inflammatory symptoms follow.

Ascites—Teissier (Gazette des Hop., 1852, p. 120) relates 3 cases of ascites treated by iodine injections, composed of 60 to 70 grains of iodide of potassium, 7 to 10 drachms of tincture of iodine, in 6 ounces of water. The iodine was rapidly absorbed and excreted through the kidneys.

M. Ore (Bull de Thér., Sept. 1852) has used iodine injections in 5 cases: two were cured; three died, but not from the injection. The first effects were sinking, pallor of face, lowering of pulse, and severe pain; in ten minutes these symptoms went off; subsequently, there was heat, pain, fever, some meteorism, colic, and sleeplessness. No important peritonitis, however, ever appeared to come on. The strength used was one part of the tincture to three of a vehicle. The remedy is contra-indicated in ascites, dependent upon liver or heart affection, or when there is kidney-disease. When the ascites is from peritonitis, or follows ague, the injection is to be used.

Buboes.—M. Bonnafont (*L'Union Médicale*, 1852, No. 155) describes a plan of treating suppurating buboes, by which he says much time is saved, and unsightly scars are avoided. When the bubo is quite ripe, it is pierced in its most tense part, and in the direction of the ileo-pubic axis, by a seton needle, carrying four threads. The two apertures of entrance and exit of the needle must be formed beyond the perimeter of the phlegmasia of the skin. The pus is gently but effectually evacuated by pressure, and then compression is applied by means of pieces of agaric or a dostil, occupying all the space between the apertures without obstructing them. To these scraped lint is applied, and the whole is supported by a spica bandage. In the evening, somewhat forcible compression is again to be applied, so as to empty the abscess; and the pad is to be again applied. This is to be repeated twice daily until complete cure. Care is to be taken not to draw out the threads unless free discharge is secured, and their ends should be tied together. The medium time a bubo so treated requires for cure is twenty days although some are cured in ten; and the points made by the seton leave hardly any marks. When the bubo has been opened before the entire gland had suppurated, the case is sometimes more tedious, a clear or sanguinolent fluid continuing to ooze. In such cases, vinous injections, combined with compression, have been found useful.

Cancer.—M. Devay (*Gaz. Méd.*, 1852, No. 52,) of the Hôtel Dieu Lyon, has long been engaged in investigating the therapeutical properties of conium in cancer, being of opinion that Storck's experiments should be resumed with the aid of the improved chemical knowledge of the present period. He finds the best preparations to be an extract and balsam, containing 1 per cent. of conicine, made from the seeds of the plant, gathered when at maturity, of full weight, and of an ash-grey colour. As the result of his researches he states.—1. That an ointment, applied externally, in chronic enlargements of scrofulous glands, possesses a resolvent power greater than that of any other substance. 2. In engorgements of the uterus, or inflammatory hypertrophy of the organ—so frequently complicating its prolapsus or deviation—this medicine, employed internally and externally, is of great service. 3. In cancerous affections it exerts remarkable calming effects, and in some cases even cures seem to have resulted from its employment, especially in the atrophied form of scirrhus. Its use is less satisfactory in soft and rapidly increasing tumours, but the progress of some of these has seemed to be retarded. In other cases, it has diminished the size of secondary tumours, rendering the primary ones more amenable to surgical operation. As a means

of assuaging suffering, whether used topically or taken internally, it is invariably preferred by the patients to opium and all other narcotics.

M. Manec, surgeon to the Salpêtrière has just obtained a recompense of 2000 francs from the Académie des Sciences (*Gaz. Méd.*, 1853, No. 19.) for the perseverance he has shown in investigating the action of Frère Côme's Arsenical Paste in more than 150 cases of cancer, in some of which he obtained unhoped-for results. His experience leads him to these conclusions:—1. That the arsenical paste penetrates the cancerous tissue by a sort of special action which is limited to it. This action is not simply escharotic, for beneath the superficial blackish layer, which the caustic has immediately disorganized, the subjacent morbid tissue seems struck with death, though it may retain its proper texture, and almost its ordinary appearance. Later, the cancerous mass is separated by the eliminatory inflammation which is set up around its limits. The same paste, which extends its action more than six centimetres deep in a cancer of close texture, when applied to superficial gnawing ulcers, usually only destroys morbid texture, however superficial this may be, and respects the sound parts. 2. The absorption of arsenic is proportionate to the extent of surface to which it is applied; and as long as this does not exceed a two-franc piece in size, there is no danger from this source. A large surface should only be attacked by successive applications. 3. Arsenic which is absorbed is chiefly eliminated by the kidneys, during the space of time of not less than five, and not more than eight days, as amply demonstrated by Pelouze. Thus, if we allow nine or ten days to intervene between successive applications, all danger from absorption may be avoided.

M. Gozzi, (*Bull. delle Sc. Med.*, xx. p. 231) strongly recommends the following caustic for the destruction of cancerous growths:—Corrosive subl., ʒj; caustic potass, ʒss; arsenic and cerussa, aa, gr. vj.—to be made into a paste with starch and white of egg. While using this or other caustics, emollient poultices, ointments, &c., should be avoided, as diminishing their effects, unless the irritation produced by their application has been excessive. M. Guzzi objects to the usual plan of destroying the tumour, layer by layer, from the apex to the base, the latter becoming very indurated after these repeated applications, and offers great obstacles to the approximation of surrounding granulations and their cicatrization. He prefers applying the caustic laterally, in the direction where the tumour seems most inclined to separate.

M. E. Cazenave (*L'Union Médicale*, ix.) speaks very highly

of a caustic formed by pouring hydrated sulphuric acid on powdered saffron.

Convulsions puerperal.—Mr. Bolton (Lancet, Jan. 29) relates a case in which severe puerperal convulsions, coming on immediately before labour, and unchecked by depletion, were completely arrested by the inhalation of chloroform.

Dr. Holst (Neue Zt. für Geburtsk., vol. xxxii. p. 82.) in a bad case of puerperal convulsions, attended with great rigidity of the os uteri, threw up warm water against the os uteri for six minutes, as in Kiwisch's method for bringing on labour. The os speedily dilated, and labour was completed by the forceps.

Croup.—Mr. Smith (Med. Times & Gaz. March 5) relates four cases of croup in which *tracheotomy* was performed. All the patients were in extremis, and in all the result was fatal.

M. Guersant (Bull. de Thérap., xlii. 293) details one case, and refers to another, in which tracheotomy was performed twice in the same child. M. Guersant has now operated in croup 150 times. The earlier cases were less successful than the latter; 13 of the last 40 private cases, and 13 of the last 31 hospital cases, recovered, or at the rate of 36.62 per cent. In 3 cases there was faulty deglutition after the operation, and food escaped through the tracheal opening. In such a case the child must be fed through the œsophageal tube passed in through the nares.

Diabetes Mellitus.—Dr. Hanekroth, (Schmidt's Jahrb., 1853, p. 173) recommends a mixture of sulphate of iron 3ss., tinct. cinch. c., acq. menth. aa. 3vj.; 20 to 30 drops every two hours. In two cases there was perfect recovery.

Mr. Sampson (Lancet, Jan. 8) states that the permanganate of potash (grs. ij.—v. in solution thrice daily) has a marked effect in reducing the quantity of urine in cases of obstinate dyspepsia, and in diabetes mellitus. In a case of the latter disease, the quantity of urine fell from 10—12 pints to its normal amount, but still contained sugar.

Diarrhœa.—Mr. North, (Med. Times and Gaz., Feb. 12.) in noticing the employment of *dilute sulphuric acid*, (in 3ss. doses every 2 or 3 hours.) states that it is in the serous forms especially when attended with cramps, that it is most useful.

Dyspepsia.—Dr. Bennett (Ed. Monthly Journ., Feb. 1853.) in a lecture on Dyspepsia, after insisting on the necessity of seeing that there is no excess in eating and drinking that the food is properly masticated, and that proper rest is taken after food, remarks that the sense of load or weight is best relieved by acids, especially the hydrochloric. Acid eructations and cardialgia are best relieved by alkalies and bitter tonics. In cases in which fatty matters do not appear to be digested, liq.

potassæ is recommended. When the flow of bile appears deficient, mild mercurials and rhubarb is the best treatment.

Epilepsia.—Dr. Marshall Hall, in a series of articles (Lancet Jan. and Feb.) brings forward evidence to show the advantage of *tracheotomy* in preventing the severest attacks of epilepsy, by rendering impossible the *laryngismus*, on the occurrence of which depends the violence of the fits.

Dr. Tyler Smith (Lancet, March 5) has performed tracheotomy in a case of "uterine epilepsy." The operation appeared useful but the ultimate issue of the case is not recorded.

Dr. Abbot (Amer. Jour. of Med. Sc. Jan. 1853) gave the *cotyledon umbilicus* (grs. v. night and morning) to a man aged 50, who had been subject to epilepsy every two or three weeks for five or six years. The fits entirely ceased, but the patient began to totter in gait, and had attacks of partial loss of consciousness. The cotyledon was discontinued, and the fits returned.

M. Moreau, physician to the Bicêtre, has been induced, by the recommendation of M. Herpin, to try the oxide of zinc. His results do not support M. Herpin's statements.

Erysipilas.—Dr. Creighton (Edin. Month. Jour. Dec. 1852) speaks highly of the *tinct. ferri sesquichloride*, in doses of ℥ x. to ℥ xv. every 2 or 4 hours. It appears, however, that ant. pot. tart. (gr. $\frac{1}{4}$) was also given, so that the inference appears doubtful.

Fever, Intermittent.—Dr. Pepper (Amer. Jour. of Med. Sc. Jan. 1853) has tried *bebeering* and *cinchonia*, in ague. The former succeeded in 2 cases, failed in 2; the latter seemed more useful.

Fever, Continued.—Dr. Barclay (Med. Times & Gaz., Jan. 8) records the effect of large doses (10 grains every 4 hours) of *sulphate of quinine*, in 18 cases of continued fever, chiefly typhoid? In 5 cases there was marked depression; in two the pulse became slow, without depression: in 11 no physiological effect was produced. The average duration of the fever was not shortened by the treatment, and cinchonism in fever is considered not to be "more speedy safe and effectual" than other methods.

Dr. Dundas (Med. Times & Gaz., Jan. 29) denies the accuracy of Dr. Barclay's inferences, states that "no prostration of the vital powers" is produced by quinine, and relates a case of typhoid (?) fever in which the remedy was useful.

Dr. Douglas (Amer. Jour. of Med. Sc., Jan. 1853, p. 282) states that he has never seen the least good follow the use of large doses of quinine in *typhoid fever*.

Fistula in Ano.—M. Alquié (Gaz. des Hôpitaux, No. 48)

states that he has found it a very beneficial practice to cauterize the lips of the wound by means of nitrate of silver, after the incision of fistula, premature adhesion being prevented without the necessity of interposing tents, lint, or other material. The nitrate should be applied, but only to the lips, twenty-four hours after the operation, and repeated first every, and then every other morning. He was led to the practice by observing the condition of wounds when touched by the nitrate. The pelticle or superficial eschar is eliminated in a day or two; the surface so covered being unsuited to contract adhesions. This covering also enables the wound to tolerate the presence of the intestinal discharges which pass over it. Most persons complain little of the pain caused by the application; but in some cases it is severe.

Fractures, Compound.—M. Trastour details in a series of papers (*Archives Générales*, vols. xxix. and xxx) numerous cases of compound fracture he has witnessed in M. Chassaignac's wards illustrative of the favorable results that have followed their treatment by "occlusion." As our readers are aware,* this consists in the immediate application of a cuirass of adhesive plaster, which is restrained *in situ* for several days, all surrounding inflammation being kept down by leeching, if necessary. M. Trastour's report is highly favorable; under this plan the wound far more rapidly heals, pain and traumatic fever are much diminished, as is the chance of the occurrence of nervous delirium, tetanus, erysipelas, and purulent infection. In cases in which it may be doubtful whether amputation will be required, it enables us to wait with safety for the decision; and brings these traumatic cases in nearer relation to the cases in which amputation is performed for disease, and in which its results are so much more satisfactory. Since he has adopted this practice, M. Chassaignac never amputates for traumatic injuries of the fingers, however violent the injury may have been. Even when re-union does not take place, very much longer and better stumps result from leaving the case to nature.

Fractures of the lower end of the Radius.—M. Robert (*L'Union Médicale*, 1853, Nos. 6 & 7) observes that the various plans of treating fracture of the lower end of the Radius with antero-posterior displacement, are either insufficient when the amount of pressure is slight, or dangerous when it is great, in consequence of the possible inflammation and gangrene of the skin of the wrist. Moreover, long-continued pressure gives rise to slow inflammatory action and adhesion among the synovial sheaths, leading to stiffness of the wrists and fingers, and an in-

* *British and Foreign Medico-Chirurgical Review*, vol. v. p. 262.

ability to employ the limb for many months after the accident. The following is M. Robert's own plan of treatment:—The fracture need not be reduced; but the patient, lying in bed, stretches out his arm horizontally and parallel to the trunk, and puts it on its palmar surface, upon a pad well filled with oat-chaff. This cushion terminates by a thick lower edge, which corresponds to three fingers' breadth above the palmar fold—i. e., a little above the fracture, so that the hand hangs pendant over this border, the fingers being prevented coming in contact with the bed. When inflammation exists, leeches, fomentations, &c., are applied to the exposed part. At the end of the eighth or tenth day, the patient may get up, supporting the arm in a sling, and still allowing the hand to hang down; or, indeed, he may be up from the first, resting his arm on a table. M. Robert has thus treated from fifty to sixty cases, in all of which rapid consolidation, with absence of deformity, has resulted. The greatest advantage, however, attendant upon the plan, is the freedom of the synovial sheaths from adhesion, so that the patient can at once resume his occupations.

Furunculus.—In the furunculoid epidemic lately prevalent, various means are proposed by different medical men; nitro-hydrochloric acid, alterative-doses of mercury, cholurate of potash, quinine, and iron are among the chief measures recommended.

Galvanism.—Mr. Springsfellow exhibited to the Medico-Chirurgical Society (Lancet, March 5) a new form of galvanic-battery, resembling Pulvermacher's, but more portable. For a full description see report in Lancet.

Heart Disease.—M. Beau (Archiv. Gén. Fev. 1853, p. 181.) in a long paper on heart-affections, in which several original and disputable views are advocated, recommends *digitalis*, not as a sedative, but as a *stimulant*, and states that it acts by relieving the imperfect action of the heart, or what he calls the state of *a-systolia*, in many cases of hypertrophy. In a case of this kind the heart beats rapidly, perhaps 120 times per minute, but the pulse is feeble and unequal. After the use of *digitalis*, the action falls perhaps to 60 per minute; but the beats are much stronger, and are more regular; at the same time the face ceases to be injected, and the dyspnœa diminishes. M. Beau believes that the *digitalis* *augments* the contractility of the ventricle, and thus produces these effects. He calls *digitalis* "the cardiac cinchona." He gives 20 centigrammes of the leaves infused in a cupful of water, every morning, on an empty stomach. The diet must be good.

Intestinal Obstructions.—Mr. B. Philips (Lancet, Jan. 1.) advises, in intestinal obstruction, that the drastic-purgatives,

such as croton oil, should not be given at an early period; but one or two full doses of calomel and opium (8 to 10 grs. of calomel to 2 grs. opium) should be first given, and large emollient enemata be thrown up every six or eight hours. If these means fail, Mr. Philips pushes mercury to salivation, mercurial inunction as well as administration by the mouth being employed.

Neuralgia.—Cazenave (Rev. Med. Chir. in Med. Times and Gazette, Feb. 15) recommends in facial neuralgia, an ointment composed of chlorform 20 parts, prussiate of potash 10 parts, and lard 60 parts; a piece the size of a walnut to be rubbed over the painful parts. An oiled-silk cap is then to be worn for some hours.

Ovarian Cystis.—M. Duplay (Archiv. Gen., Feb. 1853) relates in a case in which an ovarian tumour was tapped, 16½ litres of fluid drawn off, and 250 grammes of a fluid (composition, water 100 parts, alcohol 50, iodide 5, iodide of potassium 5) were thrown into the cyst, and then made to issue out again through the canula, by pressure; all but 2 grammes were discharged. Some sharp pain and fever followed; but in three days all unfavorable symptoms disappeared, and the patient up to the date of report (exact time not given) had continued well.

Palate, Fissure of.—Dr. J. Mason Warren describes (Am. Jour. of Med. Sc., Jan. 1853) a new forceps to be used in the operation for cleft palate; the instrument has a double curve, one anterior, the other lateral; and the posterior jaw of the forceps is longer than the other. The instrument has double teeth.

Phlegmon.—Polli (Gaz. Tosc. 1, 1852) confirms the statements of Bellini, as to the abortive cure of sub-inflammatory swellings of the skin and subcutaneous cellular tissue by the application of a few drops of liquor of ammoniæ. In syphilitic bubo this method is also useful.

Puerperal Miasmata.—Dr. Busch, Director of the Berlin Midwifery Clinique (Neue Zeitsch. für Geburtsk., xxxii. p. 313) after remarking upon the great difficulty there exists in keeping a lying-in hospital free from puerperal fever, relates the result of an experiment he tried at Berlin. During February and March, 1851, after an epidemic of influenza, one of puerperal fever prevailed extensively amidst all classes at Berlin, the hospital suffering severely during the latter month. It was evacuated, thoroughly cleaned and ventilated for six weeks, and re-opened in May, when, however, every woman admitted became affected soon after delivery. Reflecting upon the influence of hot, dry air in destroying contagious fomites, Dr. Busch had stoves introduced into the wards, and all the bed-

ding, utensils, &c., were exposed to a temperature of 150° to 170° Fah. during two days. On patients being re-admitted, no more cases occurred, although the disease still prevailed in Berlin. In December, 1851, four women were seized with the disease in one apartment, one of them dying. A heat of 170° Fah. was resorted to, and no extension of the epidemic occurred. To the time of writing, June, 1852, no recurrence had taken place.

Rheumatism.—Dr. Bennett (Edin. Monthly Journal, Dec., 1852) relates six cases of acute rheumatism, in which *nitrate of potash* was used. In all, the medicine was useful; in two the benefit was very marked.

Sarcenæ Ventriculi.—Dr. Hassall (Med. Times and Gaz., Jan. 29) relates a severe case in which *sulphite of soda* (as recommended by Dr. Jenner) was used with great benefit; infusion of *quassia* and *bicarbonate of potash* were also employed, and seemed useful.

Dr. Bennett (Edin. Monthly Journal, Feb., 1853, p. 168) refers to a case in which the sulphite of soda was ineffectual.

Scarlatina.—Dr. Gillespie (Edin. Monthly Journal, March, 1853) recommends the repeated use of the warm bath, mercurial laxatives on first admission, afterwards small repeated doses of a diaphoretic, containing antimonial wine, spirit of ether, nitrate and liquor ammoniæ acetatis; and the application of a strong solution of nitrate of silver to the throat.

Scurvy.—Dr. Hammond (Amer. Jour. of Med. Sci., Jan., 1853) recommends strongly, from experience of twelve cases in New Mexico, the use of the *salts of potash*, especially the bitartrate. The remedy was first employed in consequence of Garrod's recommendation.

Stomatitis Ulcerosa.—Dr. Mackenzie recommends sponging with the dilute nitric acid of the Pharmacopœia, and giving internally carbonate of ammonia, and citrate of iron.

Syphilis.—Professor Gamberini (Bull. delle Sci., Med. xxi., p. 253) confirms the favourable opinion expressed by Dr. Daverts, of the *iodide of sodium*, and believes, from an experience of 116 cases, that it may be advantageously substituted for the iodide of potassium. The dose was at first 3j., raised gradually to 3ij. per diem.—[*Brit. and For. Med. Chir. Rev.*

Iodine Injections in the Treatment of Dysentery.

M. Delioux has obtained remarkable success in the treatment of dysentery by injections of iodine. The first effect of these injections is to increase for a time, the alvine dejections; but they speedily diminish afterwards and rapidly change in their

appearance, and soon cease entirely. They have never been known to produce any bad effects when used after the following formula :

R	Tinct. Iodine,	10 to 30 grammes.
	Iodide potass.	1 to 2 " Mix.

An emollient enema should be ordered before the iodine injections are given in order to perfectly empty the intestines. Seldom are the patients enabled to retain the injections beyond fifteen or thirty minutes ; yet they have time to make a decided impression upon the parts affected, and have been found far preferable to the argentine injections.--[*Revue Medico-Chir.*, 1853. *New Orleans Med. and Surg. Journal*.

Miscellany.

Epidemic Furuncles and Whillows.—This section of country has been visited during the last few months, as it was in the summer of 1851, with an epidemic tendency to furuncles and whillows, which has in a great many instances proved extremely annoying, not to say painful. We do not know that it has been as general this year as it was in 1851, when it prevailed throughout the United States and a portion of Europe.

We would like to hear from our correspondents on the subject. Some individuals have had as many as 15 or 20 large furuncles coming on in succession and in different parts of the body, notwithstanding strict attention to diet and the use of saline laxatives, as usually recommended in such cases. We must confess that we never knew the furunculoid diathesis, if we may use the expression, to be controlled or benefitted by a resort to this antiphlogistic treatment. We have sometimes thought that the use of a genuine syrup of sarsaparilla was advantageous. Ought not these affections to be regarded, as well as carbuncles, as evidence of an impaired stamina, rather than of a phlogistic diathesis? The fact is, that many of the furuncles we have seen were of the kind vulgarly called "*cat boils*," and which we have been long in the habit of calling *carbuncular furuncles*.

Satisfied that antiphlogistics do no good, we have generally advised our patients to take nothing, and to live as usual. If our suspicions of the state of the system be correct, however, it would be well in such cases to administer tonics.

These remarks are submitted to the profession in the hope that more attention may be given to the subject, and that the tonic plan of treat-

ment may be tried by others, as we intend to do in future, when the opportunity presents itself.

Savannah Medical College.—We have just received the Circular, announcing that this Institution will go into operation on the first Monday in November next. The course of lectures will be delivered by Dr. R. D. Arnold, on the Theory and Practice of Medicine; Dr. P. M. Kollock, on Obstetrics and the Diseases of Women and Children; Dr. W. G. Bulloch, on the Principles and Practice of Surgery; Dr. C. W. West, on Medical Chemistry; Dr. H. L. Byrd, on *Materia Medica* and Therapeutics; Dr. E. H. Martin, on Physiology; Dr. J. G. Howard, on Anatomy; Dr. J. B. Read, Demonstrator of Anatomy, on Pathological Anatomy.

We wish our neighbors every success in their laudable enterprise.

It must be gratifying to their Alma Mater to find that two of the gentlemen appointed to professorships in the Savannah Medical College are graduates of the Medical College of Georgia.

Prospectus of the "Georgia Journal of the Medical Sciences."—Dr. George F. Cooper proposes to publish, in Savannah, a Journal under the above title, to be issued in monthly numbers of 64 pages, at \$3 00 per annum. The first No. will be issued when two hundred subscribers shall have been procured. Dr. Cooper is a talented physician, and will, we doubt not, discharge the duties of Editor with credit to himself and usefulness to the profession. We will be happy to extend to him the hand of fellowship.

New Medical Journals.—We cheerfully add to our list of Exchanges, "The Peninsular Journal of Medicine and the Collateral Sciences," edited by E. Andrews, A. M., M. D., Demonstrator of Anatomy in the University of Michigan, published monthly, at Ann Arbor—and "The People's Medical Gazette," edited by John Davis, M. D., published monthly at Abbeville C. H., So. Ca. They both promise to be useful additions to our periodical literature, and the latter will doubtless exert a favorable influence upon "the people," for whom it is specially designed.

Our old friend the "Transylvania Journal" has passed into other hands and changed its name. It is now the "Kentucky Medical Recorder," and is edited by H. M. Bullitt, M. D. and R. J. Breckenridge, M. D., of Louisville.

The Practice of Surgery. By JAMES MILLER, F. R. S. E., Professor of Surgery in the University of Edinburgh, &c., &c. 3d American from the 2d Edinburgh edition. Edited, with additions, by F. W. SARGENT, M. D., &c. Illustrated with 319 engravings on wood. Philadelphia: Blanchard & Lea. 1853. 8vo, pp. 729. [For sale by T. Richards & Son, of this city. Price \$3 25.]

No encomium of ours could add to the popularity of Miller's Surgery. Its reputation in this country is unsurpassed by that of any other work—and, when taken in connection with the author's *Principles of Surgery*, constitutes a whole, without reference to which no conscientious surgeon would be willing to practice his art. The additions, by Dr. Sargent, have materially enhanced the value of the work.

Efficacy of the Inversion of the body in cases of Syncope from the Inhalation of Chloroform.—Some unfortunate accidents having been lately reported from the inhalation of chloroform, much attention has been directed to the discovery of means for their prevention. Among others, exposure to air, titillating the nares or soles of the feet, introducing the finger into the fauces and raising the epiglottis, insufflation of air from mouth to mouth, etc. M. Nelaton has tried all these without success, and thinks them useless. From his experience, he believes that syncope is the principal cause of death, the subjects of it being pale, rigid, and corpse-like; and the plan which he has found most efficacious is the rapid and complete inversion of the body.

The first time, at the Hospital St. Louis, while performing an operation on the hand, he observed the patient becoming pale, and the pulse faltering, and immediately had him inverted; when the face, before palid, at once became congested, and the interrupted respiration re-established.

At the Hospital de la Faculté a similar case occurred last year; and lately M. Denonvilliers, having heard of the plan pointed out by M. Nelaton, tried it with equal success. In place, then, of the ridiculous proceeding of titillating the nose or feet in persons who do not feel the amputation of a limb, the body should be quickly inverted, as being the most prompt and easy way of arresting the syncope which threatens the life of the individual.—[*Jour. de Med. et de Chirur. New Orleans Med. and Surg. Journal.*]

WORKS RECEIVED.

Transactions of the Tennessee State Medical Society at their 24th annual session—Nashville, 1853—contains a number of excellent papers.

Quarterly Summary of the Transactions of the College of Physicians of Philadelphia—from May to July, 1853—contains an able Biographical Notice of Dr. Drake, by Prof. Meigs, besides other interesting matter.

SOUTHERN MEDICAL AND SURGICAL JOURNAL.

Vol. 9.]

NEW SERIES.—NOVEMBER, 1853.

[No. 11.]

PART FIRST.

Original Communications.

ARTICLE XXXV.

ON THE VENOMOUS SERPENTS OF GEORGIA.

PART I.

Natural History and Physiology. By JOHN LE CONTE, M.D.,
Professor of Natural Philosophy and Chemistry in the University of Georgia.

No class of animals has enjoyed so large a share of the marvellous as the snake, which, from the earliest times, excited the wonder, the respect, or the abhorrence of mankind. In antiquity, the Serpent played an important part in the systems of cosmogony and mythology. The facility and rapidity of its motions, although destitute of any external organs of locomotion;—the insidious stealthiness and noiselessness of its movements, as well as its voiceless character;—the remarkable brilliancy and beauty of some species, as contrasted with the dingy hideousness of others;—its extreme tenacity of life, frequently exhibiting signs of vitality after the most serious mutilations;—the extraordinary fecundity of many species;—the very striking phenomenon of a periodical exuviation of the entire skin, typifying a complete renovation of the animal system;—and, above all, the wonderful and truly incomprehensible lethiferous powers of some kinds, which enabled them to destroy the largest animals, by means of weapons which seem-

ed to be absolutely insignificant, and with a certainty and rapidity which appeared to be supernatural as well as superhuman:—constitute a combination of qualities and powers, well calculated to produce a powerful and vivid impression upon the imagination of primitive races of men. They would naturally look upon a class of creatures endowed with such extraordinary powers, as, in many respects, superior to man, and, consequently, as the appropriate type of some of the attributes of exalted beings.

Hence it is, that Serpent-worship (*Ophiolatreia*) is found to be a primitive form of idolatry, either actually or symbolically celebrated in the religious systems of almost every nation of the ancient world. From Babylonia, we may trace it East and West through Persia, Hindostan, China, Mexico and Peru, Britain and Gaul, Scandinavia, Italy, Illyricum and Thrace, Greece, Asia Minor and Phœnicia; and North and South through Scythia on the one hand, and Africa on the other. Some venerated it with unbounded horrors:—many of their Gods were accompanied by it as a type of wisdom; and several religions considered it emblematical both of a good and bad Deity. In the mythology of Egypt, Greece and Italy, the mystic Serpent consecrated almost every temple, attended upon almost every Deity; was imaged in the heavens, was stamped upon the earth, and characteristically appeared in every thing belonging to Tartarus. By some nations, as the Hebrews and Persians, it was the emblem of cunning, deceit, and wickedness; by others, such as the Egyptians and Phœnicians, it was looked upon in a double point of view,—as emblematic of the invincible power of royalty,—and likewise as a good genius (*Agathodemon*), and worshipped as the symbol of fertility and healing; while among the Greeks and Romans, it appeared under a variety of symbolic representations. As an accompaniment of *Æsculapius*, the Serpent was the well-known emblem of the healing art; and at the present time, a snake with its tail in its mouth is regarded as a symbol of eternity. The Serpent appears also to have held a place in the Hindoo, Mexican, and Scandinavian mythologies, where it was considered as the type of an evil Deity; and the tempter of mankind was represented under the same form. Even in

the early ages of the Christian church, a sect of the Gnostics called Ophites, worshipped the Serpent which tempted Eve as the father of all sciences.

At the present day, throughout the whole Christian as well as the Mohammedan world, snakes are looked upon with unmixed feelings of horror and detestation. The distinction between the venomous and non-venomous species—so well known to the Egyptians and other nations of antiquity—is almost universally disregarded;—fear and prejudice have combined to throw obliquely upon the whole order of Ophidian reptiles; and, by the general consent of society, every child is invested with the powers of indiscriminate destruction, to the great detriment of many really useful and harmless kinds of Serpents. It is in the highest degree desirable, that every intelligent person, and, especially every Physician, should be able to distinguish the dangerous from the innocuous species, in order to warn mankind against the real danger, and preserve them from groundless terrors. Ignorance on the part of the members of the medical profession in relation to this subject, is still more lamentable in another point of view:—it tends to foster and perpetuate error in regard to the treatment of snake-bites. A person receives a slight scratch on the leg from the bite of a harmless Serpent; he screams with fright;—his companions pronounce the snake poisonous, and it is destroyed;—the limb is ligatured, and, of course, swells and becomes edematous;—the physician is called in, administers some reputed antidote, removes the ligature, and the patient recovers;—and, finally, the case is published, forms an integral part of the medical experience of the day, and is cited as a valuable contribution to therapeutics. It is to be feared, that such instances are neither imaginary nor uncommon.

Without going into minuteness of detail, the following are a few of the most *general* physiological characteristics of the Ophidian reptiles. In common with many other members of the Class Reptilia, the heart consists of two auricles and one ventricle. The venous blood, collected from all parts of the body, accumulates in the vena cava, and thence passes into the right auricle. From the right auricle it passes into the single ventricle, and by it is impelled through the aorta into all parts

of the body. A small branch leads to the lung, and the blood, when purified, is returned to the left auricle, which drives it into the ventricle. This ventricle thus receives venous blood from one auricle, arterial from the other, and it is a mixture of this kind which is distributed through the body. In consequence of this incomplete circulation, the blood is imperfectly aerated, and the animal is cold-blooded.

The Serpents are especially characterized among Reptilia by having no sternum nor any vestige of a shoulder-blade, but whose ribs still surround a great part of the circumference of the trunk, and are only wanting at the tail. The vertebræ are curiously arranged :—the body of one is articulated by a convex surface to a cavity in front of the succeeding vertebra. This structure of ball-and-socket allows of free *lateral* motion ; but the spinous processes of the back prevent motion, *up and down*, to any great extent. The third eye-lid and the tympanum are deficient ; but the malleus of the ear exists under the skin, and its handle or shaft passes behind the tympanic bone. The left lung is generally abortive or rudimentary ; and the respiratory motion is unassisted by a diaphragm, and is principally effected by the ribs and abdominal muscles.

The bones of the head, excepting in a few species, possess a great deal of mobility. The lower jaw, instead of a direct articulation with the upper, is brought into connection with it, through the medium of two bones, moveable on each other ; and, in fact, the whole maxillary apparatus is conformably and peculiarly modified to permit the requisite distention of the soft parts surrounding the mouth, and the transmission of the undivided prey to the digestive cavity. But the mechanism by which this distention is accomplished,—and which is, in truth, a dislocation of the adapted parts, which return to their original positions after the act of deglutition is performed,—requires, to be well understood, a more particular description. The following is an abstract of the very clear account given by Prof. Richard Owen in his great work on “Odontography.” “The two superior maxillary bones have their anterior extremities joined by an elastic and yielding fibrous tissue with the small and single intermaxillary bone. The symphysial extremities of the lower maxillary *rami* are connected together by a simi-

lar tissue, allowing of a still wider lateral separation. The opposite or posterior extremity of each ramus is articulated to a long and movable vertical pedicle, formed by the tympanic or quadrate bone, which is itself attached to the extremity of a horizontal pedicle formed by the mastoid bone, so connected as also to allow of a certain yielding movement upon the cranium. The palatine and pterygoid bones have similar loose and movable articulations, and concur, with the other dentigerous bones of the mouth, in yielding to the pressure of large bodies with which the teeth may have grappled."

In Serpents, the male organs of generation are usually concealed within the cloaca: they are bifurcated and armed with recurved spines. The two penises are protruded from the two lateral commissures of the lips which bound the transverse opening of the cloaca. In the rattle-snake, the intromittent organs appear to be double on *each side*, from the great development of the bifurcations of the glans penis. It was this structure which led Dr. Edward Tyson to announce, that this snake had *four* penises. (*Phil. Trans.*, vol. 13, p. 25.)

But the most obvious as well as the most striking characteristic of Ophidian reptiles, is the total absence of any external members of locomotion. It is true, that in some genera (*Boidæ*) the hinder limbs are developed under the skin, formed of several bones, and ending in a short exserted spur or hook, placed one on each side of the vent; but they are so rudimentary as to be discoverable only on dissection or very close examination, and are used as organs of prehension rather than progression. Not a single species belonging to the *Boidæ* is found in the United States: our Serpents are, therefore, destitute of even rudimentary limbs. The popular idea that our common snakes possess distinct feet, which they may be made to exhibit under certain circumstances, especially when thrown into the fire alive,—has, doubtless, originated from the fact, that the bifurcated and thickened *penis* is protruded from the cloaca in such cases.

Although destitute of limbs, the usual organs of motion, yet some Serpents are capable of very rapid progression. This progression may take place in several ways:—thus, the body may be straightened out entirely in contact with the ground,

and a slow motion produced by the action of the scales and ribs, somewhat similar to that of the earthworm with its setæ. This mode of locomotion has been fully established by the observations of Sir Everard Home. (*Phil. Trans.* for 1812, p. 163.) Again, the body may be thrown into several undulations in a horizontal plane, the posterior of which is used as a fulcrum, or *point d'appui*, the straightening of the anterior must result in the advance of the head, which in turn is fixed, while the rest of the body is again flexed. This is the ordinary mode of progression. The opinion that, Serpents sometimes gather up the whole body into one *vertical* arch or hoop like a bent spring, the head and tail alone in contact with the ground and more or less approximated; and that by a sudden straightening of the hoop, they can progress with great rapidity:—is *probably erroneous*. We have already seen, that the arrangement of the spinous processes of the vertebral column limits the vertical flexion, and seems to forbid their progression by vertical undulations, as they are often represented in the older books of Natural History, and frequently on the stages of theatres. Snakes frequently spring upon their prey by throwing themselves into a coil,—which is done by contracting all the muscles on one side of the body,—and then, suddenly calling into violent action all the muscles on the opposite side, the whole animal is propelled, as if by the release and unwinding of a powerful spring, with an impulse which raises it to some height from the ground, and projects it to a considerable distance. But, assuredly, the Kangaroo performances of the rattle-snake,—reported from hear-say testimony by Prof. Benjamin Silliman,—which raised itself upon the extremity of its tail, and in this position, executed a series of extraordinary leaps, with sufficient rapidity to overtake a man running at full speed,—must lose its credibility in the same proportion as such a feat is perceived to be *mechanically* impossible. (Vide. *Silliman's Journal*, 1st series, vol. 2, p. 229.)

Before proceeding to an enumeration of the characteristic marks which distinguish the *Venomous* from the *Non-venomous* Serpents, it will be necessary for us to present our readers with an accurate Catalogue of all the Snakes which are known to inhabit the State of Georgia. This is rendered more impera-

tive from the fact, that the confusion of terms in the popular names is so inextricable, that, otherwise, I should find it difficult to designate the several species of Serpents. The presence or absence of noxious properties, leads to a very natural division of this tribe of reptiles into two great groups—viz: Venomous and Non-venomous Serpents. In the following Catalogue, we have appended the popular appellations as far as they are known.

Catalogue of Serpents inhabiting the State of Georgia.

VENOMOUS SERPENTS.

CROTALUS.—*Linnaeus*.

1. durissus.—*Lin.* Banded Rattlesnake.

2. adamanteus.—*P. de Beauvois.* Water Rattlesnake. Diamond Rattlesnake.

CROTALOPHORUS.—*Gray*.

3. miliarius.—*Lin.* Ground Rattlesnake.

TRIGONOCEPHALUS.—*Oppel*.

4. piscivorus.—*Lacépède.* Water Moccasin. Cotton Mouth.

5. contortrix.—*Lin.* Copper-head. Highland Moccasin. Rattle-snake's Pilot.
ELAPS.—*Schneider*.

6. fulvius.—*Lin.* Bead-snake.

NON-VENOMOUS SERPENTS.

COLUBER.—*Linnaeus*.

7. constrictor.—*Lin.* Black-snake.

8. guttatus.—*Lin.* Corn-snake. Red Chicken-snake.

9. couperi.—*Holb.* Indigo-snake. Gopher-snake.

10. punctatus.—*Lin.* Ringed-snake.

11. alleghaniensis.—*Holb.* Pilot Black-snake.

12. quadrivittatus.—*Holb.* Chicken-snake.

CORONELLA.—*Laurentius*.

13. getula.—*Lin.* Chain-snake. Thunder-snake. King-snake.

14. rhombo-maculata.—*Holb.*

15. doliata.—*Lin.* Bead-snake.

HELICOPS.—*Wagler*.

16. erythrogrammus.—*Daudin*.

17. abacurus.—*Holb.*

BRACHYORRHOS.—*Kuhl*.

18. amœnus.—*Say.* Red-snake.

CALAMARIA.—*Boie*.

19. elapsoidea.—*Holb.* Bead-snake.

20. striatula.—*Lin.* Brown-snake.

RHINOSTOMA.—*Fitzinger*.

21. coccinea.—*Blum.* Scarlet-snake. Bead-snake.

PITUOPHIS.—*Holbrook*.

22. melanoleucus.—*Daudin.* Pine-snake. Bull-snake. Horn-snake.

PSAMMOPHIS.—*Boie*.

23. flagelliformis.—*Catesby.* Coachwhip-snake.

LEPTOPHIS.—*Bell*.24. *æstivus*.—*Lin*. Green-snake. Grass-snake.25. *sauritus*.—*Lin*. Ribbon-snake. Garter-snake.TROPIDONOTUS.—*Kuhl*.26. *fasciatus*.—*Lin*. Water-snake. Water-moccasin.27. *erythrogaster*.—*Shaw*. Copper-belly. Water-moccasin.28. *taxispilatus*.—*Holb*. Water-snake. Water-moccasin.29. *rigidus*.—*Say*.30. *sirtalis*.—*Lin*. Striped-snake. Garter-snake.31. *ordinatus*.—*Lin*. Grass-snake. Garter-snake.HETERODON.—*Palisot de Beauvois*.32. *simus*.—*Lin*. Hog-nose-snake.33. *niger*.—*Catesby*. Black-viper. Spreading-adder.34. *platyrhinos*.—*Latreille*. Hog-nose-viper.OPHISAURUS.—*Daudin*. (Not properly a *Serpent*.) *ventralis*.—*Lin*. Glass-snake.

The foregoing Catalogue shows, that our Fauna is comparatively rich in this order of reptiles. Omitting the Ophisaurus,—which can scarcely be considered a Serpent,—Prof. J. E. Holbrook has described, in his “North American Herpetology,” 47 species of Ophidian reptiles as inhabiting the United States; and of these, 10 belong to the Venomous group. Dr. J. E. De Kay found but 16 species inhabiting the State of New York; of which, 2 are Venomous. It will be perceived, that there are 34 species of Serpents inhabiting the State of Georgia; and of these, 6 kinds come under the Venomous division. It is, therefore, sufficiently obvious, that we enjoy the company of a large share of the species composing this order of vertebrata. It is proper to observe, however, that, of the 6 species classed among the Venomous, *two* of them,—*Crotalophorus miliarius* and *Elaps fulvius*,—can scarcely be considered dangerous to man. The former, is greatly dreaded by the common people, as it gives but a very slight warning with its diminutive rattle, and is frequently the aggressor. Its bite is very poisonous, is generally followed by violent *local* inflammation, and even by gangrene and sphacelus; nevertheless, as the snake is small, the quantity of venom is insufficient to produce death in large animals. As for the *Elaps fulvius*, although it is furnished with an immovable fang on either side of the upper jaw, yet the *poison gland* is probably in a rudimentary condition. It is almost universally considered perfectly harmless, and is constantly handled with impunity. It seems to have been

placed among the venomous snakes, more from the fact, that it is the Northern representative of the dreaded *Elaps lemniscatus* of South America, than the possession of any noxious qualities. According to the best observations, it cannot be induced to bite under any provocation whatever.

There are, consequently, but 4 species of our Serpents, which can be considered *dangerous* to man. Moreover, from the well-ascertained fact, that the venomous snakes are endowed with powers of fecundity, far *inferior* to the harmless ones, they are nothing like so numerous. The rattle-snakes rarely produce over 9 or 10 at a birth, while 81 living garter-snakes (*Tropidonotus sirtalis*) have been taken from a single individual. From this fact, it is comparatively *seldom*, that the poisonous Serpents fall under observation. The universal dread in which they are held, has enormously exaggerated the number and abundance of these reptiles. Every one who has resided many years on the sea-coast of Georgia, has, doubtless, seen a great number of Rattle-snakes; but it would be very erroneous to make this a measure of their comparative abundance. Every specimen which falls a victim to man, is brought home in triumph, and all the neighbors must see the dreaded monster. In this manner, I have had numerous opportunities of seeing them:—nevertheless, during many years rambling—at all seasons—through the woods on the sea-coast of our State, I have never met with but *one* Rattle-snake. Other persons may have been more fortunate; but I apprehend, that the number furnished by each man's *personal* experience, would be comparatively small.

Again, the relative abundance of our Venomous snakes has been vastly multiplied, from the fact, that several of our most common harmless Serpents, have been, almost universally, confounded with their dangerous congeners. The dread of the fatal *Trigonocephalus piscivorus* has brought into suspicion and disrepute several other snakes that live in the same localities and possess a greater or less similarity of appearance and habits,—as the *Tropidonotus fasciatus*, *T. erythrogaster*, and *T. taxispilotus*, (constituting our most abundant water-snakes), which are not only harmless, but really useful in destroying vermin. They are all called “Water Moccasins,” and are indiscriminately dreaded and detested.

Nothing can be more ridiculous than a fear of the common Water-snakes, chicken-snake, green-snake, black-snake, and other species. Even should they be forced to bite in self-defence, the wound can never be more serious than a similar scratch of a pin or of a point of a knife. The same may be said of the *blowing or hissing* snakes of the genus *Heterodon*, usually termed Viper or Adder in the United States, and which present a formidable appearance from the power they have of flattening the head and anterior part of the body when irritated. Some of the species coil themselves when disturbed, assume a threatening attitude, by flattening the head and neck, which they lift and wave with an undulating motion, hissing loudly at the same time, and projecting the head towards the object of its annoyance; but they cannot be provoked to bite or even to open the mouth. These snakes are very generally regarded as venomous. This unjust obliquy has, doubtless, been, to some extent, perpetuated, by the fact that some of the species are vulgarly called Vipers or Adders:—thus associating them with the really venomous European Vipers. The common Black-snake is generally known to be perfectly harmless:—nevertheless, it is active, bold, and, sometimes, quite impudent; refusing to give way to man. Numerous stories are current of their pursuing individuals. It is quite possible that, under some circumstances, they may follow after a person who flies in terror before them; but such is not the experience of the herpetologist, with whom the case is exactly reversed:—the snake here being usually the *fugitive*, and too often escaping by his superior agility, and thus eluding the just claims of science to his body.

Distinctive Characteristics of our Venomous Serpents.

It is not my purpose to give detailed scientific descriptions of the dangerous snakes inhabiting our State. To the naturalist and man of science who has access to books on herpetology, this would be a work of supererogation; while to persons not versed in the technicalities of zoology, it would be as useless as unintelligible. It is my object, to call attention to a few of those *general* distinctive characteristics, which are readily recognized by casual observation.

1. RATTLES.—The several species of *Crotalus* and *Crotalophorus*, are readily and most obviously distinguished from all other Serpents, whether dangerous or harmless, by the remarkable *rattle* at the extremity of the tail. This singular appendage is made up of many pieces, from one to 30 or more, which are perfectly similar to each other in their form, and are articulated together by a very beautiful mechanism. The piece of rattle immediately connected with the body seems to be moulded on the last vertebra of the tail, which it encloses, and from which it is only separated by an interposed layer of the dermis or true skin, by which it is secreted. Its surface presents three nearly circular elevations corresponding to three protuberances:—of these, the first, or that nearest to the body of the animal, is the largest; the other *two* rings are encased in the succeeding piece, which is connected in a similar manner to the next ring, and so on throughout the series;—the posterior two-thirds of each piece being embraced by the following, so that of the three prominent rings that project from each piece, only the *anterior* is visible, the two posterior being contained in the next element of the rattle; excepting the ultimate one, in which all of the three protuberances are exposed to view. The last two rings of each piece thus enclosed in the first two of the succeeding, retain it in its place; but as the diameter of the former is less than that of the latter, each piece is quite loose and plays freely about upon that which it envelopes. None, except the first are connected with the skin of the animal by any muscle, nerve, or vessel. It is, therefore, merely an external appendage of the corneous cuticle, moved, as any foreign body would be, when the end of the tail is agitated. The pieces of the organ are formed successively on the skin of the tail, receiving from it the materials necessary for its development, and adhering to it until its growth is complete. A second piece, entirely similar to the first, is formed under it, and detaches it from the end of the tail. It is pushed backwards, leaving between its edge and the skin an interval occupied by the first ring of the new piece. A third piece is formed under the second, pushing it backwards, but retaining it, by its posterior rings being included in the cavity of the second piece. If the vertebræ of the tail continue of uniform

diameter, all the pieces will be of the same size, and the rattle, consequently, is of one breadth throughout. On the contrary, if the vertebræ grow while the rattle is in process of formation, the pieces increase in size, and thus it tapers to its end. It is very evident, from what has preceded, that the rattle is nothing more than a modification of the corneous epidermis of the tail, and that the only reason why it is not thrown off at each periodical moulting, is, that its *mechanical connections* prevent such a result. It is also obvious, that only one piece can be found at each exuviation of the cuticle. The idea that the number of rattles mark the age of the snake, is unquestionably erroneous. Dr. Holbrook has known *two* rattles added in one year, and Dr. Bachman has observed *four* produced in the same length of time. On the other hand, the rattles are liable to be lost. Mr. Peale, of the Philadelphia Museum, kept a living rattlesnake 14 years;—it had, when it came into his possession, *eleven* rattles; many were lost, and new ones were formed, so that at the end of the time, there were still *eleven* pieces.

2. REPRODUCTION.—All of our poisonous snakes are viviparous, or rather, ovo-viviparous. Not that their mode of reproduction is really different from that of the oviparous serpents, but that the eggs are developed and hatched in the oviduct before exclusion from the body, so that the young animals are expelled alive. It is, therefore, essentially *oviparous*, and must not be mistaken for the viviparous *placental* reproduction of mammalia. Among Serpents, as with birds, the development of the embryo is complicated by the presence of an allantois, and the amniotic sac with its peculiar liquid. As the period of hatching approaches, the yolk bag, or vitelline sac, together with its remaining contents, is gradually taken into the cavity of the abdomen, through the umbilical aperture. Should the young snake be hatched before this sac is completely drawn into the abdominal cavity, its presence *externally*, might, very naturally, lead to the erroneous conclusion, that it possesses a *true placenta with an umbilical cord attached*.

This ovo-viviparous peculiarity appertains to *all* venomous Serpents, and hence, they have been included under the family

Viperidæ. Nevertheless, it is important to bear in mind, that *it is not restricted to them*. With us, some of the harmless species,—as the different kinds of *Tropidonoti*,—are likewise ovoviviparous. With some, it appears to depend, in a measure, upon the latitude, mean temperature, and the greater or less abundance of food ; so that the same snake may be made oviparous at one time, and ovoviviparous at another, merely by altering the circumstances, so as to accelerate or retard the period of extruding the eggs.

3. HEAD.—The head of our venomous Snakes is disproportionately large, triangular, rounded or truncated anteriorly, and flattened above. The enormous breadth of the posterior part, as contrasted with the remarkably small and contracted neck, gives a *hastate* appearance to the head. This peculiar conformation of the head seems to be connected with the wonderful distensibility of the several parts composing the cranium, requisite for swallowing the large animals on which these Serpents prey. To this end, the tympanic bones,—which connect the *rami* of the lower jaw with the posterior part of the cranium,—are enormously elongated. This gives great width to the posterior region of the head ; which is still farther increased, by the presence of the large poison gland on either side behind the eye. Armed with the power of destroying comparatively large animals, nature has invested them with a conformation admirably adapted for transmitting the undivided victim to the digestive cavity. Nothing is more common than for our Rattlesnakes to swallow full-grown rabbits.

This distinctive mark, founded upon the peculiar shape of the head, does not belong to *all* poisonous Serpents, but is quite characteristic of those inhabiting our State. The Elaps is the only exception ; which, as we have seen, is probably innocuous. Among the harmless snakes, the several species constituting the genus *Heterodon*, are the only ones in which, the character of the head is liable to produce an erroneous impression. Some of these have the head large, flattened, triangular, and broader behind:—but then, *the enormous size of the neck*, (as large as the head,) and the *pointed snout turned upwards at the tip*, will readily distinguish them from their dangerous congeners.

4. SCALES ON THE HEAD.—All of our Venomous snakes, excepting the Elaps, have a great portion of the superior region of the head covered with *scales* instead of *plates*; a mechanism which appears to be conformable to the extreme dilatability of the parts, to which reference has been made. In the *Crotalus*, this peculiarity is very conspicuous, inasmuch as the scales cover the *vertex* as well as the occiput, extending anteriorly as far as the eyes; the front being the only part covered with plates. In the *Crotalophorus* and *Trigonocephalus*, the scales do not extend so far forward; and consequently, this character becomes less striking. In fact, as a similar arrangement in the anterior extension of the scales of the neck exists, to some extent, in the innocent *Heterodon*, it loses more or less of its value as a distinguishing mark between this genus, and the *Trigonocephalus*.

5. PIT BETWEEN THE EYE AND THE NOSTRIL.—In all of our poisonous Serpents,—the Elaps being excepted,—there is a *deep pit* or *fossa*, of considerable size, situated on each side between the eye and the nostril, which penetrates in the direction of the poison apparatus, at the base of the fang. Dr. Tyson first pointed out the fact, that these orifices have no connection with the organs of hearing, and that they led into cup-like cavities formed by the bones of the skull and those of the upper jaw, which were not perforated. (*Phil. Trans.*, vol. 13, p. 26.) Many years later, they were more minutely described by Sir Everard Home. (*Phil. Trans.*, for 1804. p. 72.) It seems that, these pits are found in several different genera of Venomous snakes, and that, they have never been seen in any of the Non-venomous species. The teleological import of these pits is not understood. They have no direct communication with the cavity containing the poison, but are connected with the lachrymal passages. Dr. Richard Harlan having invariably found an exceedingly delicate transparent membrane over the osseous cavity in the bone at the base of the fang, has suggested; that, “this membrane, whilst it intercepts any direct communication between the sack and the external canal, might at the same time permit the action of the atmosphere on the fluid contained in the sack, and thus change its chemical properties.”

(*Trans. of Amer. Phil. Society*, New Series, vol. 3. p. 311. 1830.) This conjecture appears to receive some support from the general opinion, that the *activity* of the poison is increased by long retention.

6. UNDIVIDED SUBCAUDAL PLATES.—All of our Venomous snakes,—the Elaps being excepted,—have broad *undivided plates* under the tail posterior to the vent, similar to those under the abdomen. Sometimes a few of the plates are *bifid* near the extremity of the tail. All of our Non-venomous Serpents, *without a single exception*, have the subcaudal plates *bifid or divided* as far forward as the transverse slit of the vent. This is *not* an absolutely *general* distinctive mark between Venomous and Non-venomous snakes; but, with respect to those inhabiting Georgia, *it is absolute* and highly characteristic.

The *six distinctive external characters* as above given, are abundantly sufficient to distinguish the dangerous from the harmless Serpents inhabiting our State. But the arrangement of the teeth and the organization of the mouth, constitute the most important marks of distinction. The character most commonly adduced from the dental system, as distinguishing the Venomous from the Non-venomous snakes, is, that the former have *two*, the latter *four* rows of teeth in the upper jaw; the two outer or maxillary rows being wanting in the Venomous species, and their place being supplied by a single poison fang. Prof. Owen has shown, that this *is not* an infallible character; inasmuch as some poisonous snakes, have large immovable fangs associated with a greater or less number of maxillary teeth. Among *our* Serpents, the Elaps is the only one thus organized; and we have seen that it is not dangerous. In all of our truly Venomous snakes, the poison-fangs are associated only with their successors; constituting all of the teeth attached to the maxillary bones, which are here shortened to mere tubercles on the anterior and lateral portion of the head. Consequently, only the *two* rows of *palatine teeth* are seen in the roof of the mouth; the arrangement of teeth in the *lower jaw*, is the same in all Serpents.

These isolated fangs are usually said to be movable; but, as M. Cuvier remarks, it is, properly speaking, the exceedingly

short maxillary bone that moves. The structure of the venom-fangs of Serpents, and the machinery by which their deadly agency is brought to bear against those who are so unfortunate as to be the objects of their attacks, are so interesting, that, with the assistance of Prof. Owen, we will endeavor to lay before our readers as clear a description of them as possible.

1. MACHINERY FOR ERECTING THE FANG.—The superior maxillary bone diminishes in length with the decreasing number of teeth which it supports. The transverse or external pterygoid bone elongates in the same ratio, so as to retain its position as an abutment against the shortened maxillary, and the muscles implanted into this external pterygoid style communicate, through it, to the maxillary bone the hinge-like movements backwards and forwards upon the ginglymoid articulations connecting that bone with the anterior frontal and palatine bones. As the fully-developed poison fangs are attached by the same firm basal ankylosis to shallow maxillary sockets, which forms the characteristic mode of attachment of the simple or solid teeth, they necessarily follow all of the movements of the superior maxillary bone. When the external pterygoid is retracted, the superior maxillary rotates backwards, and the poison fang is concealed in the lax mucous gum, with its point turned backwards. When the muscles draw forward the external pterygoid, the superior maxillary bone is pushed forwards, and the recumbent fang withdrawn from its concealment and erected.

In this power of changing the direction of a large tooth, so that it may not impede the passage of food through the mouth, we may perceive an analogy between the typical genera of venomous snakes and the *Lophius*; but in the fish, the movement is confined to the tooth alone and is dependent on the mere physical property of the elastic medium of attachment; in the Serpent, the tooth has no independent motion but rotates with the jaw, whose movements are governed by muscular actions. In the fish, the great teeth are erect, except when pressed down by some extraneous force. In the Serpent, the habitual position of the fang is the recumbent one, and its erection takes place only when the envenomed blow is to be struck.

2. STRUCTURE OF THE FANG.—That the poison fangs of venomous snakes are perforated, with the view of injecting the venom into the wound, was very well known to the ancients:—Galen tells us, that the mountebanks of his day used to stop these perforations with some kind of paste; after which, they would publicly expose themselves to be bitten without danger. But the peculiar structure of these organs was first described by Redi and Fontana. A true idea of the structure of a poison-fang will be formed by supposing a simple conical tooth to be pressed flat, and its edges to be then bent towards each other, and soldered together so as to form a hollow cylinder, or rather cone, open at both ends. The flattening of the fang and its inflection around the poison-duct commences immediately above the base, and the suture of the inflected margins runs along the anterior and *convex* side of the recurved fang: the poison-canal is thus in *front* of the pulp-cavity. The basal aperture of the poison-canal is oblique, and its opposite outlet is still more so, presenting the form of a narrow elliptical longitudinal fissure terminating at a short distance from the apex of the fang. It is more strongly curved backwards than the ordinary teeth, but its acute and slender apex is frequently bent slightly in the contrary direction, as in the rattlesnake.

The duct which conveys the poison, although it runs through the centre of a great part of the tooth, is nevertheless, as we have seen, really on the *outside* of the tooth; the canal in which it is lodged and protected being formed by a longitudinal inflection of the parietes of the pulp-cavity or true internal canal of the tooth. This inflection commences a little beyond the base of the tooth, where its real nature is readily appreciated, as the poison-canal there rests in a slight groove or longitudinal indentation on the *convex* side of the fang:—as it proceeds, it sinks deeper into the substance of the tooth, and the sides of the groove meet and coalesce, so that the trace of the inflected fold ceases, in some species, to be perceptible to the naked eye; and the fang appears, as it is commonly described, to be perforated by the poison-duct.

3. POISON GLANDS.—According to Prof. Müller, the poison glands occupy the sides of the posterior half of the head; each

consists of a number of narrow elongated lobes, extending from the main duct which runs along the lower border of the gland upwards, and slightly backwards. Each lobe gives off lobules throughout its extent, thus presenting a pinnatifid structure; and each lobule is subdivided into smaller secerning cæca, which constitute the ultimate structure of the gland. The whole gland is surrounded by a double aponeurotic capsule, of which the outermost and strongest layer is in connection with the muscles, by whose contraction the several cæca and lobes of the gland are compressed and emptied of their secretion. This is then conveyed by the duct to the basal aperture of the poison-canal of the fang. We may reasonably suppose that the rage which stimulates the venom-snake to use its deadly weapon must be accompanied with an increased secretion and great distention of the poison glands; and as the action of the compressing muscles is contemporaneous with the blow by which the Serpent inflicts its wound, the poison is, at the same moment, injected with force into the wound from the apical outlet of the perforated fang.

4. REPLACEMENT OF FANGS.—In the typical and most deadly venom-snakes—such as the *Crotalus* and *Trigonocephalus*,—the poison-fangs acquire their largest size, and are associated only with their successors, destined to replace them should these be destroyed by accident. These are clustered in greater or less number behind the fully-developed fang, presenting the same structure, but of a size proportionate to their degree of development, and further differing in being loosely imbedded in the thick and wide mucous gum, which likewise conceals the fixed and functional fang in its ordinary position of retraction and repose. It thus appears, that in the posterior part of the large mucous sheath of the poison fang, the successors of this tooth are always to be found in different stages of development. The pulp is at first a simple papilla, and when it has sunk into the gum the succeeding portion presents a depression along its inferior surface, as it lies horizontally, with the apex directed backwards: the capsule adheres to this inflected surface of the pulp. But how the cylindrical cavity of the dilated fold is occupied in the loose growing poison-fang, and by

what contrivance it is brought into the same relation with the severed duct of the poison gland as the displaced fang which it succeeds, *is not yet clearly understood*. (On the foregoing points, vide Owen's "Odontography," *Passim*, and Todd's "Cyclopedia of Anatomy and Physiology," Articles, "*Reptilia* and *Teeth*.")

HABITS OF OUR VENOMOUS SERPENTS.—We will conclude this Paper with a few observations on the *habits* of each of the *four* species of dangerous snakes inhabiting Georgia. On this point,—as well as on many others to be noticed in a future communication,—I must express my profound obligations to my friend W. E. Dearing, M. D., of Augusta, Georgia. For many years he has studied the habits of our Ophidian reptiles with an assiduity and discrimination beyond all commendation. Endowed by nature with an intuitive *power of taming*, he has collected and domesticated a great number of our snakes, and has, thus, been able to investigate their habits, under the most favorable circumstances. This has been done in the face of popular prejudices which were so strong, as to characterize such studies,—so foreign to the spirit of utilitarianism,—as little else than manifestations of mental hallucination.

1. CROTALUS DURISSUS.—This snake lives on rabbits, squirrels, rats, etc., and is a remarkably slow and sluggish animal, lying quietly in wait for his prey, and permitting any one to pass within a few feet of him unmolested. When suddenly disturbed, he generally throws himself into a coil, and warns the aggressor by rapidly vibrating his rattles; but Dr. DeKay records a case in which an Indian was struck without the slightest warning. (*Nat. Hist. of New York*, "Zoology.") It is a very commonly received opinion, that the rattlesnake never strikes unless *coiled*; so that if once thrown from this position, he may be approached without danger. But Dr. Dearing has seen them strike repeatedly without coiling.

The Hon. Paul Dudley informs us, that these Serpents rattle most fiercely in clear fair weather. When rainy, they make no noise; for which reason, he affirms, the Indians do not like to travel in the woods in rainy weather. (*Phil. Trans.* vol. 32,

p. 292.) According to the observations of Dr. Dearing, they will rattle in all kinds of weather; but the sound is comparatively feeble when the atmosphere is moist. He assigns a *physical cause* for this fact, which affords a perfectly satisfactory explanation of the phenomenon. Owing to the strongly *hygroscopic properties* of the corneous rattle, it becomes quite *soft* when the atmosphere is damp, and, consequently, its vibration produces a comparatively feeble sound.

2. *CROTALUS ADAMANTEUS*.—This terrible snake is restricted to the Southern States. It chooses for its abode, damp and shady places, keeping constantly near the water, from which circumstance it is sometimes called the "Water-rattle;" although, as Prof. Holbrook says, there is no evidence of its taking to the water in search of prey. In other respects, its habits are similar to the preceding species.

3. *TRIGONOCEPHALUS PISCIVORUS*.—This snake is found about damp, swampy places, or in water;—far from which it is never observed; and hence, called "Water Moccasin." It lives on fish, frogs, toads, tadpoles, and many of the smaller reptiles. This Serpent is very vicious,—attacking every thing that comes within his reach—and its bite is justly dreaded. The tail is terminated by a small *horny point*, about half an inch in length, which seems to be the *homologue* of the rattle of the preceding genus. This excrescence, though perfectly harmless, has, according to the older writers, given a dreadful character to its owner, imposing a belief on the credulous, that he is the terrible Horn Snake, armed with death at both ends. Lawson informs us, that he "heard it credibly reported by those who said they were eye-witnesses, that a small locust tree, about the thickness of a man's arm, being struck by one of these snakes at 10 o'clock in the morning, then verdant and flourishing, at four in the afternoon was dead and the leaves red and withered." (*Lawson's New Voyage to Carolina*, p. 130.) The foregoing story is only surpassed by one recorded by Sir Hans Sloane: Col. James Taylor of Metapony, told Col. Beverley, that, having found a rattlesnake, they cut off his head with three inches of his body. A green stick, the bark being

peeled off, was put to the head ; it bit it. Small green streaks were observed to rise up along the stick towards the hand. The Col. threw the stick down, and, "in a quarter of an hour, the stick, of its own accord, *split* into several pieces, and fell asunder from end to end." (*Phil. Trans.*, vol. 38, p. 321—*cited from Beverley's Hist. of Virginia*, 2d Ed., p. 260.) When such absurd and preposterous notions find a place in grave philosophical transactions, it is not at all astonishing, that "Snake Stories" should have become typical of everything incredible.

4. *TRIGONOCEPHALUS CONTORTRIX*.—This reptile chooses dark and shady places for its residence in general, though at times it is found in meadows of high grass. Dr. Dearing has frequently observed it about fallen timber and old deserted houses. Its usual food seems to be field-mice, wood-rats, and small birds. This snake is equally as vicious as the preceding, and its bite is considered very dangerous.

It has long been a popular belief,—in relation to the European Viper, as well as our Rattlesnakes,—that, when the female is suddenly surprised, she opens her mouth and permits her young to *run down her throat*. The English viper-catchers deny that any such thing ever happens ; and I have been disposed to ascribe the origin of the opinion to the fact, that, these Serpents being ovo-viviparous, they are sometimes found with the young in the oviduct ; which might lead persons to suppose that they were in the stomach. But the following account given by the well-known French naturalist and traveller, M. Palisot De Beauvois, is so *direct and positive*, that my faith in the above explanation has been shaken. He asserts, "that he saw a large Rattlesnake, which he happened to disturb in his walks, coil itself up, open its jaws, and instantly receive small ones, which were lying by it, and instinctively rushed into its mouth. M. De Beauvois retired, and watched the snake, and in about a quarter of an hour after, saw her discharge them. He then approached a second time, when the young retired into its mouth with greater celerity than before : and the snake immediately moved off in the grass and escaped." (Rees' "Cyclopædia." Am. ed., Art. *Crotalus*, cited from Amer. Phil. Trans.)

As regards the "Fascinating" or "Charming" power of snakes, it is now generally considered a *fable*: a modification, as Prof. Holbrook suggests, of that of the *basilisk* of the ancients, "a creature whose deadly glance would alone prove fatal." The curious behavior of birds and some of the smaller animals, when in the vicinity of these reptiles, is more rationally explained by supposing that they are victims to maternal solicitude, or, that they are bewildered and paralyzed with terror, in suddenly finding themselves in the presence of an enemy of such threatening aspect,—rather than to imagine them to be endowed with a mysterious and indefinable influence over their weak and defenseless prey. In those cases in which the animal has been observed to struggle and *die*, under the gaze of the snake distant several yards,—it is probable, that the victim had been previously *struck*, and the serpent was quietly awaiting the result of the envenomed blow.

Capt. Richard F. Floyd, of Camden county, Georgia, in a very interesting letter to Rev. James H. Linsley, says, "I have often drawn near the rattlesnake and looked it steadily in the eye until the intensity of my gaze became confused and dim from the most natural cause, without having any strange effect produced upon me." (vide *Silliman's Journal*, 1st series, vol. 46, p. 45, Note. 1844.) Some have imagined that their victims are overpowered by the horrible *stench*, which they are capable of emitting from their bodies. But it seems to me very doubtful, whether this effluvium is *ever* thrown off except for purposes of *defense*, as the polecat does; and even if it was emitted under other circumstances, it is still more problematical, whether it could, by any possibility, have an *intensity* so great as *instantly* to overpower the animal, and prevent its escape.

It is a popular opinion, that *hogs* are particularly destructive to these reptiles, and that they are quite invulnerable to snake-bites. There can be no doubt, that these pachyderms are very seldom killed by the bite of venomous serpents:—Dr. Dearing has known *two* instances in which they have been bitten, without the slightest injury. Doubtless this arises from no special immunity from the effects of the poison; but from the fact, that the slight vascularity of the skin, and the thick external

layer of *fat*, prevent the venom from finding its way into the circulation. If struck under favorable circumstances, they would be killed. Dr. De Kay was informed by a respectable farmer in Dutchess Co., N. Y., that he lost three hogs in one season by the poison either of the copper-head or rattle-snake. (Nat. Hist. of New York. "*Zoology*.")

It is likewise a popular belief, that the poisonous Serpents are often killed by the common Deer (*Cervus virginianus*), which leap on them, with all four feet touching each other, and off, so quick, that the snake has no power to bite, and this he repeats until he completely dispatches him. Mr. Linsley says that, "Mr. George Walter assures me that he witnessed this fact this season in Missouri, while secreted in the bushes near the operation." (Vide, *Silliman's Journal*, 1st series, vol. 46, p. 44. Note. 1844.) Dr. Dearing informs me, that he has frequently seen domesticated deer kill various species of the common snakes by leaping on the body or head; but has never seen them perform the operation on the venomous serpents. A few astute lexicographers have been able to discern in the Greek word for the stag, "*Elaphos*," a very early, if not an *intuitive*, knowledge of this remarkable habit of the cervine tribe. For example, the learned Schrevelius derives this word from "*Elein*," and "*Opheis*," "*quod serpentes è cavernis trahat*." It is true, that to those who are not initiated in the mysteries of philology, it would seem *primâ facie* probable, that the progenitors of the Greeks would have given a *name* to the stag *long before* they had observed such a *habit* in the animal:—but in a science in which *vowels have no power* and *consonants are interchangeable*,—what may we not expect a prolific classical imagination to accomplish?

Athens, Georgia, Sept. 20th, 1853.

ARTICLE XXXVI.

Soothing Liniment—proposed by Dr. T. DOUGLASS, of Alexander, Burke Co., Ga.

Prof. GARVIN :

My Dear Sir—I beg leave to present you, for examination and trial, a small specimen of a saponaceous compound which,

on account of its very prompt and soothing qualities, I have honored with the name of *Lethean Liniment*.

It is made by digesting a bar of fresh turpentine soap and four ounces of gum of camphor in a gallon of good alcohol for two weeks in the heat of the sun. It is then bottled up while hot, and one drachm of chloroform added to every four ounces, set in a cool place, and shaken occasionally while coagulating.

The turpentine affords the best means, in my opinion, of applying chloroform to the skin, because, by its adhesiveness it holds that volatile fluid longer and more firmly in contact with the surface than any other substance could do.

My mode of applying it, is to coat the part well with the liniment and cover it immediately with a piece of good paper, which adheres firmly and produces a gentle burning, tingling sensation, which, in neuralgia, rheumatism, irritability of the stomach, cramp colic, &c., is perfectly delightful.

Did I think more of money than of honor and humanity I might possibly make this compound avail me something. But since I do not wish to become a competitor with all the host of pain-killers and extractors and eradicators in the country, you may, if you think it worthy of a trial by our noble profession, hand it over to Professor Dugan for publication in the Southern Med. and Surg. Journal.

Very respectfully,

TILMAN DOUGLASS, M. D.

PART II.

Eclectic Department.

Letters upon Syphilis. Addressed to the Editor of *L'Union Médicale*, by P. RICORD. Translated from the French, by W. P. LATTIMORE, M. D.

TWENTY-SECOND LETTER.

[Continued from Page 600.]

My Dear Friend—It would afford me much pleasure to say a word relative to the treatment of chancre; but you know that, according to the plan I have proposed to follow, I cannot, in this connection, enter into many details.

Perhaps you will permit me to say something here in relation to prophylaxis. Medical police has advanced much of

late years, especially since I and others have made examinations with the speculum, in private hospitals and in public dispensaries.

It is very certain that, since this mode of investigation has been generally employed, a great amelioration in the health of public women has been observed. Thus, in 1800, according to Parent-Duchâtelet, one woman in nine was found diseased; since 1834, the proportion has been reduced to one in sixty. The speculum has had a great share in this amelioration.

But, to be thoroughly successful, I have always insisted that women should be visited every three days, without distinction of rank; whether they be *en maison* or *en carte*; whether they dwell in Paris or at the *barrières*. You remember that inoculable pus may be formed after the second day of an artificial inoculation. Swediaur admitted that chancre may be developed within twelve hours. Frequent examinations with the speculum are therefore indispensable, if we expect the surveillance of public women to furnish a certain guarantee of freedom from disease.

I write this word *guarantee*, with special design; for there are some people who, after contracting an accident in their adventurous loves, think they have the right of claiming indemnity from the government. Perhaps you think I am not serious. I will offer a fact in proof of my assertion. Some years since, I received a visit from a merchant of Lyons, who was in a state of great exasperation against the prefect of police. He came to get a certificate setting forth the fact that he had contracted a chancre in a house of prostitution, the character of which he imagined to be *guaranteed* by the authorities. He did not know that *toleration*, like all commissions, receive no guarantee from the government.

I pass on to state that the ameliorations daily introduced in the surveillance of prostitution, and the zeal exhibited by the brethren on whom devolves the painful service of the dispensary of public health, and of the Hospital of Saint Lazarre, will furnish yet more suspicious results.

Public women are a necessary evil; this fact is generally admitted. I do not wish to examine the reasons favorably or adverse to the proposition, for this is not the place to examine them. But if the evil is necessary, it does not follow that its quantity, so to speak, should be extended, as a learned Belgian brother lately seemed to wish; but it is essentially necessary to inspect it well in relation to its quality.

In insisting that public women shall not communicate disease, it should be so arranged that they shall not be liable to contract it from those who have commerce with them. How

is this result to be accomplished? Is it necessary to institute an examination of the persons who visit them, and to prevent these visits should they prove diseased? But, apart from the difficulties of such an institution, the danger which we should thus seek to prevent would be augmented; for, instead of falling into a sewer which the police could cleanse, the unclean would go elsewhere.

The establishment of lazarets and of quarantines, suggested by my friend Diday, of Lyons, in a moment of laudable philanthropy, where a clear patent of immunity from the pox, along with a certificate of vaccination—a patent that should be as indispensable as a passport, a patent without which no one should be admitted to any public office—could be furnished, cannot be thought of at this day.

Whatever may have been said by the author of this ingenious proposition, the difficulties in the way of such an institution seem insurmountable.

There was a time, you know, when those affected with pox were banished from Paris, and condemned to the rope if they returned; a time, when at Bicêtre, patients were scourged at their entrance and exit. All this did not diminish the number of the infected. On the contrary, the scourgers perhaps deserved, in their turn, to be punished. These barbarous measures have fallen into disuse.

It is undoubtedly necessary to subject to rigorous surveillance all persons whom we can reach—soldiers, for example—and to sequester every patient over whom we have control; but a certain degree of toleration, the pardon of a fault which is sufficiently often involuntary, and excellent hospitals, with such attendance as may at present be found there, and which time will still further improve, are the best means of general prophylaxis, or those at least which will tend to diminish the gravity of the disease.

Moreover, all who are acquainted with the conditions to which women are subjected in the present state of society, with respect to labor and its remuneration, have for a long time acknowledged the fact that herein lies one of the most fruitful sources of prostitution, and consequently of the propagation of syphilis. Therefore, to ameliorate the condition of women with respect to labor, is to do a kind office as well to humanity as to morals and public hygiene.

You remember what I said with respect to the manner in which chancres are produced. It is necessary to remember this fact, in order to shun the sources of contagion pointed out. The most important fact which science teaches us relative to prophylaxis, is the necessity of avoiding exposure. This re-

mark doubtless appears a little *naïve*; but let debauchees remember it, for it is the very truth. I am now about to touch upon a delicate subject which is full of shoals. It is still an unsettled question in morals and medical deontology, whether the physician ought to give advice, with respect to preservation from evil to those who voluntarily expose themselves to the liability of contracting disease from immoral persons. I do not pretend to be more severe than Parent-Duchâtelet, who treated this subject with a purity of intention of which you are well aware. Besides, am I not encouraged by the very character of the journal which gives such liberal hospitality to my letters? I address the learned—those who are physicians; and was it not yourself, my dear friend, who said that science is chaste, even stark-naked? Be not alarmed; for, after all, I shall only glide over this ticklish subject.

There is no sure and absolute preservative against chancre. This is my declaration.

If, in spite of a knowledge of this fact, one is still willing to run his chance, some precautions may be taken. It is especially necessary to bear in mind the precept of Nicholas Massa, so energetically rendered by the elder Cullerier: "The connection should not voluntarily be prolonged." At this moment, indeed, it is necessary to be egotistical, as was remarked by Hunter, but not egotistical after the manner of Madam de Staël, who called love the egotism of two.

The most minute attention on the part of suspected persons ought to be required in houses of prostitution. A fact with which we have been for a long time familiar, namely, that a deposit of virulent pus may be held in reserve in the genital organs of women, demonstrates the necessity of this precaution. This is a means of always preventing mediate contagion. I have told you that numerous experiments have proved to me, that by decomposing the virulent pus we can neutralize it. Alcohol in water; water mixed with one-fifth Labarraque's liquid; all the acids, diluted in water so as not to be caustic; wine; solutions of sulphate of zinc and acetate of lead, destroy the inoculable power of virulent pus; while, if this pus remains unaltered, excessively minute quantities of it—homœopathic quantities, if you please—retain their power to act. M. Puché informs us that, at the *Hôpital du Midi*, the effects of inoculation have been obtained by him from one drop of pus mixed with half a glass of water.

Fatty substances are very useful, especially to medical men who are obliged to practice the touch in dangerous localities. Astringent lotions, which slightly tan the tissues, have frequently prevented contagion.

But, if cleanliness is necessary before intercourse, on the part of the one who may impart the disease, it need be minute only subsequent to the act in the one who is simply exposed to infection.

There is another means which the moral sense repudiates, and in which the debauchee has great confidence: this, undoubtedly, often serves as a security against infection; but, as was observed by a woman of much *esprit*, it is a cuirass against pleasure, and a cobweb against danger. This mediate *process* is an article which is often porous, or may already have been used; it frequently becomes displaced; it fulfils the office of a bad umbrella, which the storm may rend, and which, protecting badly enough from the tempest, does not prevent the feet from getting wet. I have seen, in fact, numerous ulcerations of the root of the penis, of the peno-scrotal angle, of the scrotum, &c. in those who had taken these useless precautions.

Many patients believe themselves safe from contagion when they do not terminate the venereal act. A lady who consulted me on her own account, was very much astonished when she found that she had communicated a disease to her lover; because, she said, *he did not conclude*.

Some syphilographs believe that the urethral infection in particular is effected after ejaculation, which creates a vacuum, and because nature abhors a vacuum. But numerous facts have taught me the reverse of this statement. Ejaculation, on the contrary, must be considered as a powerful injection from behind forwards, thus cleaning the urethra; and if urethral affections, already so common, are not more frequent, we must perhaps attribute the fact to this condition. Thus, an old, excellent precept recommends a prompt emission of urine after every suspicious intercourse.

The circumcision of the prepuce, the excision of nymphæ that are too long, also constitute hygienic measures relative to the genital organs, inasmuch as these appendices greatly favor contagion.

I ask your pardon for this digression; but it is the duty of science to destroy the influence of charlatanism with respect to the dangerous employment of a deceptive prophylaxis.

It would be necessary, were it possible, to indicate all the measures that prevent contagion and, therefore, the propagation of syphilis; not in order to protect or to encourage libertinage, but to protect the virtue and the chastity which so often become its victims.

I am yet to speak of cauterization as an abortive remedy against chancre. But I will make this the subject of my next letter.—[*New York Medical Times*

Odontalgia; its Pathology and Treatment. By J. P. Togg,
M. D.

The simple definition of the technical term odontalgia, is to be found in our vernacular word tooth-ache. No theory whatever is implied in it. It simply means what it most emphatically is, a *pain in a tooth*. The character of this pain is Proctean. It may be a slight uneasiness, it may be an intolerable anguish. It may be dull, heavy, constant like rheumatism, acute and lancinating, paroxysmal, darting, boring, throbbing. Any term of the frightful vocabulary of human suffering may be applicable to this torture. It may be cumulative, beginning moderately and increasing to intense severity, or it may at once attack the sufferer, armed in all its terrors. Yet tooth-ache is, after all, only a symptom of some lesion either in the pulp, the tooth, the jaw, the nerve, the brain, or the system at large.

The sensibility of the teeth depends entirely upon the nervous filaments distributed through that nervo-vascular mass, usually known as the pulp. Shut up in a solid box of bone, removed by several lines from the surface of the enamel, this expansion of nerves is, nevertheless, capable of perceiving very delicate impressions made upon the outside of its hard, encrusting ivory. The disagreeable sensation produced by even a few soft fragments of food, intervening between two perfectly healthy teeth, is a sufficient evidence of the delicacy of its sensibility. This natural and healthy irritability may be easily exalted to the morbid condition of irritation by impressions acting upon the pulp itself or upon the nervous system generally. Any cause, therefore, which shall unduly excite these organs may produce odontalgia. Draughts of ice-cold water, or extremely hot beverages, mechanical irritation, as blows, or hard substances forcibly pressed against them, are well known causes of tooth-ache, owing to the direct impression they make upon the nervous filaments. Any thing, indeed, which is capable of exciting pain elsewhere may excite it here. But, besides these, the teeth are subject to peculiar irritations. No other nerves are so likely to be exposed by the common accidents of disease. The regular progress of caries is to advance inwards towards the cavity, and to lay bare the pulp, by the destruction of its solid case. This having been accomplished, the sensitive centre of the organ is brought in direct contact with all those varied irritants we are constantly putting in our mouths. Like any other exposed nerve, it resents these insults, and warns us to be more cautious, by the quick, sharp, darting pain which thrills us. One of the characteristics of this form of tooth-ache is the

suddenness of its invasion, and the rapidity of its subsidence, after the irritating substance is removed. To this form we may give the name, *tooth-ache of direct irritation*.

Irritation, however, may and often does run on to inflammation, so that the causes of *odontitis* or inflammation of the dental pulp will be all those accidents which have already been enumerated as sources of irritation, mechanical violence, irregularities of temperature, caries, direct contact of irritating agents, such as alimentary substances, acrid humors or hot or cold fluids touching the exposed pulp.

The pain, in this form of odontalgia, differs materially from that which we have already described. It begins ordinarily, with a dull gnawing uneasiness in the part, accompanied sometimes by a sensation of heat. After a time, this becomes aggravated to a severe throbbing pain, which is ordinarily the precursor, here as elsewhere, of suppuration. Should the inflammation not extended beyond the pulp cavity the pain is not increased by pressure on the tooth, nor is the tooth started from its socket. The application of cold to the tooth is also followed by temporary relief.

The cause of the pain experienced in *odontitis* will be understood on the most superficial examination of the structure of a tooth. The pulp is inclosed in a hard, unyielding cavity, which everywhere adapts its walls to the soft imbedded mass. In a state of perfect health, there is no room between the pulp and the walls of its cavity. If now, inflammation sets in, it is accompanied, of course by distention of the capillaries of the pulp, but these, meeting on all sides a hard, resisting bone, must press inwardly upon the soft, sensitive nervous filaments that are intermingled with them. The continued increase of the engorgement of the capillaries must, therefore, be accompanied by a continually increasing aggravation of the pain. Nor is this latter relieved by the occurrence of suppuration, unless the fluid gets exit through the fang, or until complete disorganization of the pulp has taken place, for the pus which is poured out only aggravates the difficulty by increasing the already intolerable pressure upon the nerves. Any excitation of the heart and arteries must also be accompanied by a marked increase in the pain. Like all other inflammations, it has exacerbations at night. These are partly due to attendant circumstances. During the day, the erect posture being maintained, the action of gravitation exerts the same influence over this, as over all other inflammatory affections. By the simple physical obstacle which it opposes to circulation it diminishes the infraction of the vessels. At night, however, the patient being in a recumbent position, the engorgement is greater, because of the

removal of this salutary obstacle. The warmth of the head, also, imbedded in soft pillows, increases the disturbance. Another point which must be observed in connection with the symptom of pain, is one which always attracts attention in other inflammatory diseases. We allude to the temperament of the patient. Every physician knows that there is great variety of susceptibility to pain in different individuals. Temperament and idiosyncrasy are both concerned in this. Nervous and sanguine persons suffer more from the same morbid change than the lymphatic and the bilious. What is the most intolerable anguish to one, is not much more than uneasiness to another. Every surgeon knows that some persons bear operations much better than others, and that, too, not from superior fortitude but actual oligæsthesia; some patients will suffer little from the gravest inflammatory disturbance, while others will be cruelly tortured by comparatively trivial organic disease.

The condition of the tooth, also, must modify the pain. One already disorganized by caries, or rendered rough by exostosis, would be more actually painful than an organ entirely healthy.

Inflammation of the pulp may be either *acute or chronic*.—The first form extends to every part of the pulp and lining membrane, and terminates usually in suppuration. It is more common before than after exposure of the pulp. The second form usually results from exposure of the pulp and is not so severely painful as the acute; indeed it may last sometime without pain, but the susceptibility of the tooth to painful impressions is always increased by it.

This form of odontalgia may be called *inflammatory tooth-ache*.

Closely allied to this is *periodontitis*, or inflammation of the investing membrane of the tooth. The same causes which produce odontitis may produce this. It is, however, more likely to be metastatic than the former variety. The tendency of fibrous membranes to metastatic disease is well known, a tendency which is very much increased by the rheumatic and gouty diatheses.

The pain, in acute periodontitis, is first dull, then acute and throbbing. It is distinguished from the disease last described by soreness and elongation of the tooth, redness and swelling of the gums and sometimes of the cheek. The tooth is sometimes so raised as to interfere with mastication. This affection often ends in alveolar abscess.

In both these forms of odontalgia, we may have all the accompaniments of inflammation, such as constipation, head-ache, dry skin, flushed cheeks, full and rapid pulse, in short, inflammatory fever.

Odontalgia may also proceed from exostosis, or the growth of bony vegetations at the base of the fang. These gradually, yet powerfully and irresistibly, pressing upon the nervous filaments must excite pain. According to Dr. Good, this pain at first is not quite so acute as in the other varieties of tooth-ache, for the pulp is not yet in a state of irritation. But by a continuance of the pressure, it is soon brought into this condition, when the pain will be as severe as on any other occasion, and far less mitigable. This disease is obscure in its symptoms and difficult of diagnosis. Often it is not detected till after the removal of the tooth.

Fungus of the nerve, is a cause of tooth-ache, according to Dr. Hullihen. He says, that it occupies the natural cavity of the tooth and most probably originates from a remnant of the dental blood vessels. "It always attacks teeth in which the nerve has suppurated and is of a deep red color; very soft; bleeds freely upon the slightest touch; is sometimes totally insensible, at others highly sensitive, giving much pain of an incessant character; entirely free from any darting or throbbing sensation; and, when small and painful, is frequently mistaken for an exposed nerve. It varies in size, from that of a pin's head, to a pea and larger. It is sometimes, when small, deeply situated in a fang; but more generally protrudes, filling up a cavity formed by caries."*

So far we have considered those causes of odontalgia, which directly affect the sensitive expansion in the cavity of the tooth. We come now to investigate other and more distant causes of the same affection. Occupying an intermediate place between the local and general causes, is a condition of the nervous system, alluded to by Dr. Harris,† in which the general sensibility is so exalted that ordinary impressions, which, in a state of health, are unattended by any uneasiness, may be productive of severe agony. Analogous affections of other organs are constantly met with in the practice of medicine. In many forms of fever, the cutaneous surface becomes so exceedingly sensitive as to be intolerant of the slightest touch. This is but a lower degree of the same morbid change which produces those severe aching pains in the muscles accompanying fever.

Neuralgic tooth-ache is an affection which may proceed from a vast variety of morbid conditions. Any disease which is capable of producing neuralgia elsewhere may bring it about in the teeth. To enter into the investigation of neuralgic odontalgia, would lead us into the consideration of all the various changes which may induce neuralgia, and would extend

* American Journal of Dental Science, i, 107

† Principles and Practice of Dental Surgery, 5th ed., p. 337.

this little article to a most unwarrantable length. We can, therefore, glance at but a few of these morbid alterations.

We may have intrinsic or extrinsic affections, capable of producing neuralgic tooth-ache. Among the first we may enumerate *neuritis* or inflammation of the nerve, either in its substance or its neurilemma. The morbid change referred to by Cotunnus and others, namely, a deposit of a gelatinous character under the neurilemma, is probably a result of inflammation. So, too, the thickening of the neurilemma, observed by Cirillo and Earle, would seem to be referable to the same cause.

A more common condition is simple engorgement of the vessels of the nerve, an affection which is often only the first stage of inflammation. This engorgement has been frequently discovered, especially in cases of sciatica, and may have been present before death, when no traces of it are found on a post-mortem examination. The experiments of Broussais and Goupil are conclusive on this point; they having found that rabbits, in which intense peritonitis existed during life, as seen by openings in their bellies, exhibited often no traces of this engorgement after death.

Venous congestion is still another cause of this intense pain. This has often been observed about superficial nerves affected with neuralgia. The fact that most neuralgic affections are relieved by heat and aggravated by cold gives additional probability to this hypothesis of venous congestion acting as a cause to produce neuralgia. We might further cite the intimate relations between neuralgia and intermittent fever, a disease marked by intense and frequently recurring paroxysms of venous congestion.

Any pressure of the nerve along its course may be productive of the same affection. An exostosis of the walls of the infra-orbital or inferior dental canal, an aneurism of one of the accompanying arteries, a foreign body pressing upon the nerve, or a tumor affecting it at any part of its course may produce neuralgic tooth-ache. The teeth may be and often are associated in the general anguish which involves the whole face, during an attack of facial neuralgia or *tic douloureux*. This may depend upon any of the above enumerated causes. In a case of this disease, occurring at the Richmond Hospital in Dublin, the Gasserian ganglion was fibro-cartilaginous and as large as a nutmeg. Or the disease may be central and depend upon some disorder of the encephalic mass, affecting the fifth nerve at its very origin.

It must not be forgotten, however, that this general neuralgia of the face may be caused by the irritation of a diseased

tooth. The phenomena of the *transference* and *radiation* of impressions by nervous centres have not been sufficiently studied. That they possess these properties is well known. By radiation of impressions is meant that power of the centres by which they disperse an impression made at one point, over a wide circuit in its neighborhood. Thus, to cite a case directly in point, a diseased tooth, irritating a single ramuscle of the inferior dental nerve, may arouse sufficient central disturbance to be reflected over the whole face and head, affecting every branch of the great trifacial trunk.

Transference of morbid impressions is analogous to radiation, but much more remarkable. In consequence of this property of nervous centres, an impression produced at one point is felt at a remote one. Disease of the hip joint, is a familiar example. This as all surgeons know, is very often first revealed by pain in the knee. Mr. Lawrence's case, is a very striking illustration of this law of nervous centres, for in that, neuralgia of the *thumb* was caused by the pressure of a *pivot tooth on the nerve of an old fang*.

The teeth may also suffer among other organs, those strange, wandering pains which accompany and which sometimes constitute as far as symptoms can constitute a disease, the *febres larvatae* or masked intermittents, so common in malarious districts. These may be easily recognized by the judicious practitioner who is watching for symptoms of this form of disease, but will be certainly overlooked by the careless observer who does not think of anything below the surface. As a general thing, universal *malaise* accompanies this disturbance, though but a single nerve may feel the severity of the pain; and the watchful physician can assuredly detect the characteristic element of periodicity. Sometimes, however, this is very obscure, and hardly to be recognized by the closest scrutiny, or eliminated by the most careful inquiry. As the disease advances, the periodical element gradually retires, and the neuralgic becomes unduly prominent. An analogous affection is the *hepatalgia* of malarious districts, so generally misunderstood, so often confounded with chronic hepatitis, so commonly mismanaged with large doses of calomel. This disease is almost always more or less regularly intermittent at first, though it finally becomes constant, or irregularly paroxysmal. In both affections, at this stage, the history of the case forms our most valuable diagnostic.

Neuralgic tooth-ache, however occasioned, is characterized by very much the same train of symptoms. Dr. Wood, in speaking of it, says: "The pain is usually of the acute character, sometimes mild in its beginning, gradually increasing in

intensity, and as gradually declining; but usually very irregular, at one time moderate, at another slow, and occasionally darting with excruciating violence through the dental arches, not unfrequently it assumes the regular intermittent form. Instead of pain, strictly speaking, the sensation is sometimes of that kind which is indicated when we say that the teeth are on edge, and is apt to be excited by certain harsh sounds, such as that produced in the filing of saw-teeth, by mental inquietude, and by the contact of acids or other irritant substances. Neuralgic tooth-ache sometimes persists, with intervals of exemption, for a great length of time. The diagnosis is occasionally difficult. When, however, it occurs in sound teeth, it is paroxysmal in its character, is attended with little or no swelling of the external parts, occupies a considerable portion of the jaw and especially when it alternates or is associated with pain of the same character in other parts of the face, there can be very little doubt as to its real nature."

Sympathetic tooth-ache cannot by any known means of diagnosis in symptomatology, be distinguished from the last variety. Its history alone can throw any light upon it.

The nervous connections of the teeth are so numerous and so extensive, that we need not be surprised at the wide range of their sympathies. The fifth nerve supplying the whole face and all the organs of the senses brings them in relation with the entire head. But besides this, they have special connections with many of the more important organs of the head. Meckel's ganglion and its branches, forming a sort of sympathetic centre for the entire head, is directly connected with the superior maxillary nerve, just as it is about passing into the infra-orbital canal for the supply of the upper teeth. With the ear they are still more intimately connected through the otic ganglion which rests upon the inferior maxillary nerve, at its exit from the foramen ovale. This union will explain the extreme frequency of ear-ache in children at the period of second dentition and the tooth-ache which sometimes attends it. The same ganglion, by means of the tympanic plexus which is so directly connected with it, brings them in relation with the glossopharyngeal and the pneumo-gastric nerves and so establishes a sympathy between these organs and the whole upper portion of the alimentary canal and with the lungs. Through the sympathetic system these connections are indefinitely extended. With the spinal cord they have not only the union, through the origin of the fifth nerve, with the spinal bulb, but also through the numerous filaments of the sympathetic which are connected with the spinal nerves. Thus we find that, through this intricate web of sensory filaments, the teeth are directly or indirectly united with every organ in the system.

During certain conditions of the system, therefore, it is not surprising that these sympathies should be roused to undue activity. Many diseases have the power of inordinately exciting nerves remote from the organ affected. The various disturbances of the alimentary canal are remarkable for their peculiarity. The anatomical connection between the stomach and bowels and the teeth, already glanced at, sufficiently prepares us to expect tooth-ache as an occasional accompaniment of disorder of the apparatus of alimentation.

Pregnancy is another state which is very liable to excite sympathetic tooth-ache. This is indeed but a part of those numerous remote disturbances which are caused by the peculiar condition of the uterus at this interesting period. All the organs of the abdomen are involved in that intricate net of nervous filaments which are ultimately distributed to the uterus. The two great cords of the sympathetic which descend from the solar plexus and semilunar ganglion, on both sides of the aorta, first unite over that vessel in a plexus which weaves into itself all the various threads of which these cords and the stray fibres which surround them are composed. Having thus knit on the life of the whole abdomen in one intricate interlacement of fibrillæ, the nervous mass then divides into its two hypogastric nerves and their plexuses. These proceed directly to the ganglia of the uterus, sending off great numbers of nervous twigs to all the neighboring organs, and wrapping all the pelvic viscera in their labyrinthine threads. In this manner is the great organ of reproduction brought into relation with the entire frame. During pregnancy, as has been clearly demonstrated by Dr. Lee, the nervous system undergoes the same change to which all the other structures of this organ are subjected. They increase in size, and this increase extends even to the cords which descend from the semilunar ganglia. There are, therefore, both anatomical and physiological reasons for the extensive range of sympathies possessed by the pregnant uterus. The close contact of the semilunar ganglion with the stomach, and the relations between that organ and the teeth, explain how these may be involved.

Rheumatism, with its usual wandering character, may attack the fibrous structures of the teeth. The diagnosis will be made by a careful study of the diathesis, and a thorough investigation of the attendant and antecedent circumstances of the case.

Gout, affecting every fibrous tissue in the body, does not neglect the teeth. Dr. Harris, in a note to the chapter on tooth-ache, in his *Principles and Practice of Dental Surgery*, has an extremely interesting case, which we need not apologise for quoting:

"The subject, Mr. W., a resident of Baltimore, about forty years of age, had been subject to attacks of this most excruciatingly painful affection for more than fifteen years. The paroxysms, for some four or five years previous to the above mentioned time, had occurred at intervals of from three to six months, and from ten to twelve days before each attack, during this period he suffered from pain in the first right superior molar tooth, which was slightly affected with caries in the centre of the grinding surface, but it had not yet penetrated to the pulp cavity. The pain at first was not severe, but it gradually increased in intensity, and assumed a peculiar boring, and at times, a grinding and lancinating character. His attacks of gout were confined to the joint of the big toe of his right foot and as soon as the pain commenced here it subsided in his tooth. But when the paroxysm of gout began to pass off, it again commenced in the tooth, where it continued for about two weeks. At the request of Mr. W. we removed the diseased portion of the tooth and filled the cavity. The operation for about three months promised to be successful, but about ten days previous to the next paroxysm of gout, the tooth began to ache, the pain subsiding with the occurrence of the paroxysm, but commencing again, when it passed off, and continued as it had formerly done for about two weeks. His suffering from the pain in the tooth, during these periods, was so great, that he determined to have it extracted. What the result of its removal has been, we have not yet ascertained."

The *treatment* of tooth-ache must necessarily be as varied as the disease. To talk of administering one remedy for such multiform disease is a manifest fallacy.

The tooth-ache of direct irritation has manifestly two indications—first to remove the cause, secondly to allay the pain. The latter indication is accomplished in various ways. Opiates and other narcotics and the anæsthetics may be used to deaden the sensation. Or this may be accomplished in another way, by so vehemently stimulating the exposed nerve as to exhaust its sensibility. The essential oils, of cloves, of cinnamon, of cajeput, are used for this purpose. Intermediate between the two, is kreasote, which possesses both stimulant and anodyne powers. Combined with morphia in such proportions as will form a thin paste, and applied on a small pledget of lint or cotton, it usually acts very agreeably. Dr. Harris prescribes the following formulæ:

℞ Æther. sulphuric.	℥ j.	℞ Ætheris sulphuric.	℥ j.
Pulv. camphoræ,	℥ j.	Kreasoti,	℥ ss.
Pulv. aluminis,	℥ j.	Ext. gall. Alep.,	℥ j.
Morphiæ sulphat.	℥ j. m.	Pulv. G. camph.	℥ ss. m.

These are to be applied on raw cotton, after having first cleansed and thoroughly dried the offending part, and renewed as often as may be necessary. Relief is soon obtained, but of course it is not permanent. A thick solution of *gutta serena* in chloroform is another favorite remedy. It has the double advantage of allaying the pain by its anæsthetic properties, and of subsequently forming a thin but impervious coating which sheathes the nerve and protects it for a while from the irritating substances about it.

Permanent relief, however, can only be afforded by extraction of the tooth or destruction of the nerve, as there is no other way of fulfilling the indication of removing the cause. Of extraction it is not necessary to speak in this place. Destruction of the nerve may be accomplished by thrusting a sharp stilet down through the root of the tooth and by thus mechanically breaking up the structure of the nerve, or by escharotics. The most powerful of the latter is the actual cautery. A favorite mode of domestic application of this remedy is heating a small knitting needle to high redness and plunging it down into the pulp till its destruction is effected. The only caution to be observed is to introduce it so far into the fang as to destroy the entire nerve. *Potassa fusa* may be used, made into a paste with absolute alcohol, and applied to the exposed pulp. Great care should be taken not to allow this agent to come in contact with the surrounding soft parts, or much pain and troublesome inflammation and ulceration will follow. The combination of a small quantity of pure morphia with this agent will somewhat abate the pain of its operation. *Chloride of zinc* is another very powerful escharotic which must also be used with extreme caution. The most common of all these potential cauteries, however, is arsenious acid. Dr. Foster, of New York, uses this in combination with morphia, sulphas, in the proportion of two parts of the caustic to eight of the narcotic. It is applied directly over the nerve, on a small pellet moistened with kreasote, and covered with a cap so as to avoid pressure. The subsequent treatment for the preservation of the bony shell does not come under our consideration.

In *Inflammatory tooth-ache*, the dentist has to consider which of these courses he will take—whether he will extract the tooth, destroy the nerve, or attack the inflammation with anti-phlogistics. “The propriety or impropriety of extraction will be determined by the amount of pain, the progress made by the inflammation, the condition of the parts with which the tooth is immediately connected, the effect of the local disturbance upon the general system, the situation and importance of

the tooth, and the extent of structural alteration which has taken place in the crown. If the retention of it on account of its location, or the loss of several other teeth, is of great importance to the patient, and the circumstances of the case justify a well grounded belief that it can be preserved and rendered useful, without acting as a morbid irritant, the operation, if possible, should be avoided. In this case, supposing the inflammation to have proceeded too far to be arrested, the pulp may be destroyed and the tooth treated in the manner elsewhere described, as it would be useless to procrastinate the suffering of the patient by instituting other treatment in the vain hope of avoiding an alternative, which, after all, may not enable the dentist to secure the permanent preservation of the organ. Indeed, after the lining membrane has become exposed, this is the only method of procedure, in any stage of the inflammation, which, with a view to the preservation of the tooth, holds out any prospect of success."

If the tooth is not eroded, or if the inflammation is not caused by the direct application of irritants to the exposed pulp, the antiphlogistic treatment may succeed. It is possible that, in an intense inflammation accompanied by high sthenic febrile excitement, venesection might be desirable. As a general thing, however, the application of leeches to the gum, in the neighborhood of the diseased tooth, is the only way in which it is necessary to abstract blood in this painful little affection. Saline cathartics, and rigid abstinence constitute the rest of the treatment.

Suppuration having occurred, the treatment must be modified. This condition may be detected, according to Dr. Hüllihen, by the following signs. "At first the tooth is painful only when hot or cold fluids pass over it, and may continue in this situation for some time. At length a steady gnawing pain supervenes, and then the tooth begins to get sore and appear a little loose and longer than the rest. About this period the pain assumes a very different character, darting from the tooth along the courses of the nerve, to the temple, ear, and side of the head, and to the teeth of both jaws on that side. Within twenty-four hours the face begins to swell, and the pain changes to one of a constant throbbing description which indicate that an alveolar abscess is forming. When a tooth in the manner described, becomes painful, the nerve is about suppurating; when it appears longer than the rest, loose, and is very sore, the nerve has suppurated and the pus is beginning to ooze out at the extremity of the fang where the vessels enter, producing much pressure, hence the sharp darting paroxysm. When the cheek begins to swell, the matter is effusing between the alveo-

lus and its lining membrane, and occasions the throbbing pain which always accompanies the formation of an alveolar abscess. The true abscess of the antrum maxillare, originates in like manner, from the same cause, when the fangs are very near or penetrate into that cavity." The treatment recommended by Dr. Hullihen is the trepanning or drilling of the tooth so as to evacuate the pus.

Periodontitis is treated very much in the same way as inflammation of the pulp. Besides the ordinary antiphlogistic treatment, counter-irritation to the neighboring cheek may be of service. Narcotic agents combined with rubefacients have often been found to be beneficial. A deep incision into the gum has been recommended. Should alveolar abscess be threatened, extraction will be necessary. Should it be an incisor or cuspidatus which is involved, it should be removed only as a last resort. In chronic inflammation the necessity for extraction is more urgent.

Fungus of the nerve must be destroyed by the actual cautery. The pain may, however, be relieved by making the morbid growth bleed freely. Dr. Hullihen narrates a singular case of hemorrhage occurring monthly from fungi in a young lady's teeth. Destruction of the growth and plugging the teeth did not relieve. At last it was decided to be a case of *vicarious menstruation*, and as soon as the catamenia were regulated by constitutional treatment, the local disorder vanished.

Neuralgic tooth-ache, originating, as it does, from a great variety of causes, demands a corresponding variety of treatment. Unfortunately, neuralgia still continues to be one of the *opprobria medicorum*, but we have of late been approximating something like a rational treatment of this obstinate and agonizing distemper.

Thus, if the disease be caused by a general exaltation of nervous impressibility, the indication is manifestly to subdue that irritability. If it be purely functional, the narcotics will accomplish this. If, however, it depends, as it may, upon loss of balance between the nervous and vascular systems, that balance must be restored, either by tonics, in one state of the system, or by sedatives, in another. Arsenic and iron are favorite remedies of the former class, aconite and the antiphlogistic regimen of the latter. Should the affection proceed from disturbance of the nervous centres, they must be attended to.

When neuralgic odontalgia depends upon neuritis of the fifth pair or its dental branches, local antiphlogistic treatment along the course of the inflamed nerve, must be adopted. In simple irritation of a nervous trunk, chloroform applied to the surface

nearest the nerve, and over its course, will often prove eminently successful. If there be any tumor pressing upon the nerve, it must be removed.

Sometimes the disease continues so obstinate, that even when purely local it resists all the remedies we apply. In this case, division of the nerve has been suggested. If the inferior dental nerve be involved, M. Roux recommends trephining the lower jaw, so as to reach the nerve, which is easily recognized by its color and direction. Having exposed it, he then severs it and passes a stick of potassa fusa up and down the canal so as to completely destroy it. If the superior maxillary nerve be affected, an incision is made upon the cheek and the caustic introduced as deeply as possible along the infra-orbital canal. This operation is resorted to in those maddening neuralgias of the face which associate the teeth in their general anguish. When the teeth thus make part and parcel of a number of suffering organs, the treatment must necessarily be general to the affected nerve. The main disturbance being quieted, all the subordinate pains are allayed. As facial neuralgia, however, may depend upon the irritation of a diseased tooth, it is necessary to examine the case minutely, and to extract immediately, if necessary. Here, however, caution is to be observed. Pain may be inflicted unnecessarily; sound teeth may be drawn without at all benefiting the patient, and on the other hand a useless organ may be suffered to remain in the jaw, a perpetual source of irritation, owing to a timid or procrastinating policy. When tooth-ache depends upon that general neuralgic condition which accompanies malarious poisoning, quinine, arsenic and iron are the proper remedies.

In treating sympathetic tooth-ache, the first point to be determined is the nature and seat of the primary disturbance. If this can be reached and overcome, it is manifestly our duty to do it. If not, the determination of the source of irritation will have at least one good effect; it will prevent the dentist from unnecessarily wasting his patient's teeth by profitless extraction. It would occupy us too long to attempt to trace the various morbid conditions of different parts of the system and the many modes of treating them, and then we could not succeed in giving a clear idea of all the circumstances on which this painful affection may depend. The investigation of the cause of the disease, and the selection of the appropriate remedies must be left to the judgment of the practitioner and that in each individual case.

In *rheumatic tooth-ache* the treatment will be leveled at the general disease, while the local disturbance must not be neglected. Colchicum, calomel and James' powders, guaiacum,

nitrate of potash and opium at night, followed by a cathartic in the morning, will answer the first indications, and plasters of opium and belladonna the second.

In *gouty tooth-ache* the treatment must also be addressed to the main disorder.

In *tooth-ache from exostosis*, the remedy is extraction. In some cases, however, Dr. Good's mercurial treatment may succeed, for we may have syphilitic exostosis here as well as any where else. The existence of the exostosis, however, must be first established, for pure neuralgia is usually aggravated by mercury.—[*Amer. Journal of Dental Science*.]

Remarks on the Effects of Sleep, in exhausted states of the system: (read before the Medical Society of Oneida county, New York.) By JOHN MCCALL, M. D., of Utica, Fellow of the College of Physicians and Surgeons of the University of N. York, Member of the Am. Med. Association, etc., etc.

I propose making a few remarks on the present occasion, respecting certain views and practices which have obtained to some extent among physicians, of not allowing such patients as are regarded as much exhausted, from whatever cause, to sleep but a few minutes at a time, from an idea, as they express it, that sleep beyond fifteen or twenty minutes at a time, would exhaust and prostrate the vital forces. That such opinions have been entertained by some of our members, I know, and if nurses and other attendants upon the sick are to be relied on in all cases, it is to be feared that they are still put in practice by some of the profession. At least, I have been credibly informed, that in different parts of our country, some physicians still give directions to the nurses to wake up patients who are in a very feeble state, every few minutes, though they may be sleeping soundly at the time.

Several years since, I was called in consultation in a severe case of bilious fever, occurring in the fall season. The patient was a young man in single life, who had been sick some two weeks, and was in danger of having a typhoid form of the disease; he was so reduced in strength at the time as to faint on being raised from his bed. On my second visit, which was in the morning, I inquired of the attendant how the patient had slept during the preceding night? She replied, "pretty well," but remarked that she awoke him every fifteen or twenty minutes. With some surprise, I asked by what authority she had done so; and learned that it was according to the directions of the attending physician, and upon further inquiry, I found

that such had been the practice enjoined upon the nurse for several days. Regarding such advice as not only singular, but pernicious, I took occasion in the presence of the medical attendant to urge upon the nurse and family the danger of such practice, and advised that the patient be allowed to sleep as long as he wished, even three hours at a time if he could, and that on no occasion should he be again disturbed in like manner. From this time, he was permitted to sleep as long as he desired, and often from one to three hours without waking. With careful nursing, strict attention to ventilation and cleanliness, and a mild supporting course of treatment, our patient gradually recovered, though his convalescence was slow, and it was not until the approach of the cold weather of winter that he fully regained his strength and health.

In another instance, where I was called in consultation, the same course had been pursued. The patient was a young lad about eleven years of age, the son of a clergyman, suffering an attack of inflammation of the bowels. He was naturally feeble and easily prostrated, had been ill six days when I first saw him, and by accident I learned that the nurse and watchers, by direction of the medical attendant, had made it a rule to wake him every few minutes. He was petulant, "nervous," as it is called, and easily disturbed by trifling things; his pulse was very frequent, varying from 120 to 140 in the minute, small and feeble, and he was irritated and annoyed by almost every thing said or done to him. He was now told and enjoined to sleep as long as he wished, and the nurse was forbidden to wake him, even though he should "sleep hard," to use their own words. "Tired nature's sweet restorer, balmy sleep," was now allowed to do her work "in her dire way," and with the aid of suitable anodynes and soothing remedies, the young patient slowly recovered.

In proof that one or more of our profession in distant States or sections of the country entertain like views, and pursue similar treatment, if such a term is admissible, I may mention the case of a merchant from Michigan, who became a patient of mine while suffering a severe inflammation of the middle finger of the left hand, occasioned by a slight wound received from the point of a needle near the first joint. This gentleman's constitution was a good deal impaired, in consequence of a severe course of fever which he had passed through in the preceding fall or winter. In the relation of his illness to me, he stated that by the advice of the physician who attended him, he was not allowed to sleep but a few minutes at a time. On my third visit, I learned that his good young wife had made it a rule to wake him every fifteen or twenty minutes, assuring

me that his medical attendant in Michigan had advised this practice. The patient was now told to sleep all night if he could, and his wife respectfully requested to do the same, and thus give herself no further solicitude about his "sleeping too hard." From this time he was allowed and enabled to rest quite comfortably by taking one or two small Dover's powders in the evening. He is now fully convinced that sleep, even if "hard," is nature's best restorer.

How prevalent these opinions may be, I am unable to say, but I fear there are still a few of the medical profession who entertain them, and it is very certain that in this region such notions too generally obtain among nurses and attendants upon the sick. My object in presenting the subject is to solicit attention thereto, and at the same time to ascertain, if possible, whether such practice is ever advisable in the class of patients above mentioned.

Having never seen or heard any good reason given in support of such opinions and practice, and being unable to reconcile either with any experience of my own touching the physiological and pathological view of the case, I trust I shall be excused in thus briefly discussing the subject. And in order to find out the truth here as elsewhere, (which, by the by, is often a difficult matter, and he who asks, *what is truth?* and endeavors to find it by patient thought, and careful examination, is deserving of something more than the sneers and ridicule of the world,) I shall at once proceed to inquire into the condition of the human economy in a state of sleep.

And in the first place—What is sleep? Is it a state or condition of rest of the mind and body, or simply of some portion of the organism? To arrive at the facts in the case, let us take for example a person sleeping soundly. All the external senses are more or less locked up—in other words, resting. Neither the sense of vision, hearing, tasting, smelling, nor speech, attend to their respective functions in sound sleep. The muscles of locomotion are all quiet and immovable, and all those parts or portions of the brain and nervous system concerned in thought and feeling, no longer take note of time or circumstances, or the objects of earth or heaven. The individual on awakening can rarely give any account of himself, or what occurred during his state of sleep. The circulating system with the secretions and the respiratory movements all move onward regularly during such a state, and yet, on awakening, the person is unable to give an account of what passed during his state of rest. And what is particularly worthy of notice, as bearing on this question, is the fact, that the sleeper is generally, if not always refreshed and invigorated, so

as to be ready and willing to resume again his labors of body and mind. How often, too, have we not witnessed the restoration of the poor patient to health and reason, who only a few hours before was perhaps all but exhausted from the dreadful effects of delirium tremens? In this, as in all diseases and affections of the brain and nervous system which eventuate in some form of insanity, *sleep* is the only certain remedy. These statements require no special proof, as they are facts known to many intelligent persons not of the profession. So long as an individual sleeps well, there is little or no danger of the supervention of insanity. I suppose that all parts of the brain which are concerned in mental manifestations, require rest or sleep, as well as the muscles of locomotion. Not only man, but animals, birds, and all living things require sleep. Plants droop and suffer, when deprived of that great restorative principle of nature.

The opinion that sleep of two or three hours continuance can exhaust the powers of the system when in an enfeebled condition, seems to me unreasonable, and is not founded on any physiological or pathological principle with which I am acquainted. It is a common idea, that the mind may become fatigued, sick, or diseased, and that it is never idle or asleep. I cannot conceive how all this can happen to an immaterial thing, which the mind is said to be. It seems to me, that physiological and pathological facts relating to the encephalon can best explain this matter, and one such fact is worth a thousand metaphysical speculations.

And now let me ask, What is the amount of our knowledge on this subject? What do we know of the use or purposes of the brain in health, and what do we know of its pathology? Certainly if we understood its physiology and pathology, we could talk intelligibly upon the great subject of mental philosophy. Have we any system on that subject recognized as sound in all its points, aside from phrenology? And yet who but the medical philosopher should best understand this matter, as also the subject of sleep and its effects both in health and disease?

If these remarks should elicit one useful idea in behalf of our noble profession, I shall feel satisfied.—[*N. Y. Jour. of Med.*

On the use of Chloroform in Puerperal Convulsions. By B. FORDYCE BARKER, M. D., Professor of Midwifery, &c. in the New York Medical College.

In the report of the proceedings of the Medical and Surgical Society in the April number of this journal, I observe that the use of chloroform in puerperal convulsions was somewhat dis-

cussed. Since Dec. 1847, the period when I first commenced the use of anæsthetics in midwifery,) I have met with fifteen cases of puerperal convulsions. In four of these cases, no anæsthetic agent was used, as the condition of the patient was such that it was deemed inadvisable. Three of them were comatose when I first saw them, and could not be roused from this condition. The fourth occurred subsequent to labor, immediately after the delivery of the placenta. It was retained some hours; the physician in attendance was obliged to introduce the hand into the cavity of the uterus in order to extract it. When the placenta was withdrawn, there was a great loss of blood, and the patient almost instantly became convulsed. I saw her in a half hour afterwards, when she had had four convulsions. These were arrested by the use of opium, but the patient died on the 5th day, from metritis. In two cases, sulphuric ether was used as an anæsthetic, one of whom recovered; the other was seized with convulsions in the seventh month of pregnancy, on hearing of the death of a sister from the same cause: when not convulsed, she was furiously maniacal until the ether was used, which entirely arrested both the convulsions and mania. Four days after, she was delivered of a dead fœtus. The labor was easy and natural, and unattended by any occurrence of either the convulsions or mania; nor had she any unpleasant symptoms, as I understood from her physician, until the day after delivery, when she complained of severe pain in her breast, then of headache, and within an hour after, she again became maniacal. The prejudices of her husband and friends prevented the use of the anæsthetic again, and she died on the 8th day after delivery. This case I saw in consultation, and administered the ether, but I did not see her after her confinement. In a third case I attempted to use the ether, but the patient refused to inhale it; her husband became alarmed, and I was obliged to desist. She was bled largely, and tart. antimoni, calomel, croton oil, &c., were relied upon. She also became maniacal after delivery, but eventually recovered. In eight cases of puerperal convulsions I have used the chloroform, and in all these the effects were most happy, the convulsions being entirely controlled by its use. Two occurred in my own practice—the other six I saw in consultation. In four the labor terminated naturally, three of the children being born alive. Three were delivered by the forceps, and were alive. The forceps were used in these cases, because the stethoscope indicated danger to the child. One, a shoulder presentation, was delivered by turning. All of the mothers recovered. No one, who has not witnessed the total change which the chloroform induces in an eclamptic patient, can conceive with what satis-

faction its effects are watched by the practitioner and friends. It is certainly not too much to say that by no other plan of treatment can such results be gained; and at this day we may be warranted in adding that sufficient experience has already been gained to justify the assertion that the agent is perfectly safe. At least, no authenticated case of death from the use of chloroform in *obstetrics* has yet been reported.

I suppose it will be admitted by all, that a very large majority of cases of puerperal convulsions depends upon sympathetic or functional derangements, and not on organic changes. In a large proportion of the cases of death, autopsy reveals no structural change to explain either the cause of the death, or the cause of the convulsions. And yet, from a careful examination of the reports which have been published, I find that 30 per cent. of the cases have proved fatal, which, it must be conceded, is an enormous mortality, fully justifying the horror which an attack inspires, in the mind of both the physician and the friends. Hunter said, that he feared only convulsions and hemorrhage in labor. Mad. Boivin asserts that at the Maternité, under the most skillful and rational treatment, nearly one half of the cases of convulsions die. Dubois considers convulsions more dangerous than hemorrhage.

The following table will show that the danger has not been greatly magnified:

	TOTAL.	Before and during Labor.	Recovered.	Died.	After Labor.	Recovered.	DIED.
Mauriceau,.....	45	29	13	16	16	11	5
Mad. La Chapelle,*.....	27	23	16	7	4	2	2
Desjardin,.....	7	5	5		2	2	
Velpéau,.....	21	12	8	4	9	5	4
Smellie,.....	10	10	7	3			
T. Clarke,.....	19	17	12	5	2	2	
Lever,.....	14	12	8	4	2	2	
Robert Lee,.....	54	46	31	15	8	7	1
Ramsbotham,.....	25	22	14	8	3	3	
Collins,.....	30	28	23	5	2	2	
McClintock & Hardy,.....	13	10	7	3	3	3	
Total,.....	165	214	144	70	51	39	12

* Mad. La Chapelle reports 67 cases of convulsions in 37,895 labors, but only 27 cases (as in the table) are detailed, or the results stated.

Authors have differed as to the comparative mortality of convulsions occurring before and during labor, and those coming on after delivery. Mauriceau, Velpeau, Dugés, Nægle, Churchill, Murphy, &c., regard those cases which come on after delivery as much more amenable to treatment than those which occur before and during labor; while Smellie, Astruc, Tissot, Ramsbotham, and some others, hold a contrary opinion. The table I have given above shows that 32 per cent. of those cases occurring before and during labor proved fatal, while only 22 per cent. died where the convulsions came on after delivery.

The limits of this paper preclude the possibility of any discussion of the pathology of this affection. Whatever views are entertained on this point, it will be admitted by all, that whether the exciting causes are emotional, or are from irritation of the uterus, or the stomach, the kidney, or the intestinal canal; and whether these be hyperæmia, anæmia, or toxæmia, the development of this affection depends upon some condition of the nervous system peculiar to the eras of gestation, parturition, and lactation. Furthermore, it may be considered as settled, that in all cases of convulsions it is the medulla oblongata which is primarily irritated, either directly by certain conditions of the blood, or indirectly from the terminal branches of the spinal nerves of the uterus or some other vital organ. Chloroform overcomes the influence of the cerebro-spinal system first, and then of the excito-motor or true spinal system. Thus it may and undoubtedly does allay the irritation which induces the convulsions. In certain cases, but these are rare, the convulsions are at once followed by cerebral congestion, effusion or hemorrhage; and here the chloroform will not control the convulsions, if the pressure is direct upon the medulla oblongata. But from the cases which have been reported by Kite, Fearn, Clifton, Wilson, Chailly, Channing, Metcalf, and others, as well as from my own experience, the conviction is forced upon my mind that we have in anæsthetics a therapeutic agent which will control this fearful complication of labor to an extent not before attained. The number of cases reported is yet too few to form a statistical table of value in determining the relative fatality where chloroform is used, but I fully believe it will be diminished 75 per cent.

I have met with but one case of convulsions occurring after delivery, and that was previous to the introduction of anæsthetics. I am inclined to think that in these cases I should still prefer to rely upon the use of opium rather than upon chloroform, for reasons which must be sufficiently obvious.—[*N. Y. Medical Times.*

Upon the Morbid Desire to Kill. By DON RAIMUNDO D. Y. CORREA.

[We select the following for several reasons—because it is by a Spaniard, and because it seems to us as bringing prominently forward several points in aid of the settlement (if ever it shall be settled) of this terrible question. Witness a late case, where the jury acquitted a seduced female of the crime of murder, on the score of insanity, and the next moment the judge gave her a free discharge, because he could see no proof of insanity. The article itself purports to be taken from the *Gac. de Madrid*, and may be found in the *Medical Times and Gazette* of January 29, 1853, and we can hardly gather from it what portion of it belongs to the author or to the translator.]

The author commences with a eulogy on Esquirol's work on *Homicidal Monomania*, 1837, and quotes examples from Pinel, Marc, Gall and Mende. These show the existence of a partial delirium, whether in the form of a fixed idea or an excited sensibility, extravagance in the passions, or error in judgment. In every instance there has been disturbance of the mind, and hence the words addressed by an advocate to Dr. Marc, upon the occasion of a trial of simple barbarity. "If monomania be a disease, its cure is upon the scaffold." The following bit of legal blood-thirstiness also merits being handed to posterity:—"Your so-called homicidal monomania is an hypothesis, a modern and convenient invention to shield the guilty, and to withdraw them from the power of the law."

The author proceeds to say, that from the works of Magendie and other physiologists, he can prove that there are certain powers in man, which drive him in a definite direction, without his possessing a will sufficiently strong to offer opposition. These powers which can be reduced to four, reside in the *corpora striata*, the *cerebellum*, the *crura cerebri* and the *medulla oblongata*. Injuries to these parts in animals, cause different involuntary movements, and the author concludes therefrom that there are in man different impulses stronger than the will. Governed by these impulses, the homicidal maniac commits his crime.

A man who commits murder upon a false idea, with powerful impulse, should be considered as suffering from disease in the same part of the brain. Now we cannot see the application of Magendie or Flourens' experiments to the elucidation of psychical disturbance, nor comprehend why, upon division of the *crura cerebri*, the injured and dizzy animal rolls over and over. We have before complained with justice of the gross ignorance of morbid anatomy displayed by the generality of

"mental physicians" in all countries, and we think it hard to refute statements made upon no foundation whatever. The examination of the bodies of criminal lunatics does not confirm in any one point the loose assertions of Dr. Raimundo. Neither the corpora striata nor the crura cerebri are often found in an abnormal condition; the cerebellum is for the most part, natural in structure; the medulla oblongata unaltered, except that the membranes covering it become thickened, when other parts of the encephalic coverings have undergone a similar change. We have no ground whatever for asserting that these parts are essentially the seat of morbid actions during life. The cause of insanity is to be sought for in a source deeper than that supposed by those philosophers of a somewhat materialistic school. The author endeavors for judicial purposes to found a differential diagnosis between the maniac and the responsible culprit, both of whom have committed murder.

HOMICIDAL MONOMANIA.

The person is one of weak constitution, of nervous excitable temperament, irreproachable character, working in business for the immediate necessities of life.

The monomaniac is alone.

The maniac kills without interest or passion, without motive, making that man an offering who may be unfortunate enough to meet him.

The maniac disdains to fly, and often gives himself up to justice; he often details the particulars of his act, and seeks punishment more than pardon.

CRIMINAL MURDER.

The criminals are mostly persons of strong constitution, sanguineous or choleric temperament, bad education, given to idle courses, and occupied in immoral pursuits.

The criminal is rarely alone; has usually accomplices to share the booty.

The criminal has a motive, has some passion to gratify, and selects his object accordingly.

The criminal withdraws from observation; tries to mislead the judge; to cast suspicion on others, and to do his best to avoid punishment.

We doubt if these aphorisms will stand their ground as unerring tests in this difficult question. The records of the criminal department of Bethlem Hospital would point to many an inmate imprisoned for murder, whose constitution was good and frame powerful and muscular. Many a criminal has had sufficient nerve to take life alone, unassisted by others; even the last who forfeited his life in the Metropolis fell under this class. Should we be justified in asserting that he was mad, because he was *alone* in his wife's chamber when he cut her throat?

Again, the maniac mostly takes life, not by chance or hazard, but in obedience to a fixed, though erroneous idea, sometimes in sudden passion. Who can at all times either discover or appreciate motives? Jealousy, hatred, or revenge, carefully guarded from public notice, would, but for the Law, impel many a ruffian to gratify his passion at the cost of another's life.

The maniac does not always disdain to fly, and can even argue cleverly in his own defence. But what can be said of that class of offenders whose lowly-organized and ill-directed minds are equally under the influence of both fear and evil passions? Place them under restraint, they behave respectfully and with decency; give them liberty and passion soon regains the mastery. Can any aphorisms comprehend the anomalies of this class? We believe not. Each case must be determined by circumstances elicited at the trial, and by the opinions of those in whom the responsibility of the judgment rests.—[*American Journal of Insanity.*

Predisposing Causes of Idiotism and Imbecility.

Dr. Moreau, of Tours, (see vol. 9, p. 78,) read a Memoir on the above before the Academy of Medicine at Paris. The following are extracts from it.

Hereditary causes are the true origin of idiotism. They originate those vices or imperfections of organization, that are incompatible with the regular exercise of the intellectual functions. Dr. Moreau's researches extended to 56 cases, and the relatives of these 56, presented the enormous cipher of 132 pathological conditions, to which the hereditary influence could be referred. Mania, in the form denominated lypemania is the most common hereditary source of idiotism. To these, succeed drunkenness, epilepsy, hysteria. In the collateral lines, we find, in the order of frequency, convulsions, idiotism, scrofula, apoplexy.

The hereditary influence extends equally on the father and mother's side. It is more frequent from grandfather than grandmother. It is transmitted more commonly by the male than the female, in the proportion of 53 to 37. Dr. Moreau presents the following results.

1. Idiotism is produced by hereditary causes, in absolutely the same manner, as insanity, epilepsy, hysteria, and other nervous diseases are.

2. All our therapeutical efforts should be directed to a modification of the nervous system, as opposite as possible from the

conditions that have originated the affection. Without undervaluing the value of education, and which he calls a species of intellectual gymnastics, Dr. Moreau is of opinion, that above all, we should endeavor to renew (*repair*) the moral, by modifying its organic state. Thus, changing, by a species of renovation, the vicious tendencies which the hereditary predispositions have produced.—[*Bulletin De L'Academie Nationale de Paris. Ibid.*]

Case in which Alum was successfully employed to excite Emesis in a patient who had taken a large dose of Opium to destroy life. By CHARLES D. MEIGS, M. D.

Dr. Meigs was called on Saturday the 20th inst., at 2½ o'clock, P. M., to see a young gentleman, between 26 and 28 years of age, who, after a drinking frolic, had swallowed two hours previously, one ounce of powdered opium with the view of destroying his life. A physician of his acquaintance, who happened to be present when he swallowed the poison, immediately sent for 30 grs. of sulphate of zinc, and gave it to him, but without its producing the slightest effort at vomiting. When Dr. Meigs saw the patient he was in a somnolent condition. One ounce of powdered alum was immediately procured, and one-half of it, mixed with a little syrup, was given to the patient, and followed in a few minutes by two or three tumblers of warm water, when copious vomiting ensued, by which a free discharge of the contents of the stomach was induced. After the patient had rested for a short period, the remainder of the alum, followed by two tumblers of warm water, was given, which caused renewed and free vomiting, by which Dr. Meigs presumes the opium was entirely thrown off. The patient continued still to be very sleepy, but entirely recovered from the effects of his rash act. Dr. M. considered this case worth relating, as evincing the very efficient emetic powers of the alum.—[*Trans. Phil. Col. Phys.*]

Vesicating Oil. By E. DUROY.

The solubility of cantharidin in chloroform, as shown by the experiments of Professor W. Procter, suggested to me the idea of using that vehicle in combination with a fixed oil to obtain a vesicating agent, freed from the disagreeable concomitants of the ordinary fly blister, and retaining the cantharidin in a soluble state. I proceed thus:

Powdered Cantharides, one part.

Chloroform, (by weight) one and a half parts.

Castor Oil, (by weight) one and a half parts.

To the powder was added the mixture of chloroform and oil in a close vessel: the ingredients were transferred after some hours to a glass apparatus, and the liquid displaced in the usual way. It amounted to about two-thirds of the original bulk of the liquid employed. A few drops of the vesicating oil applied to the arm of an adult produced a perfect blister in eight hours. Its easy application on any given surface may be of value as a vesicating or epispastic. I would suggest the use of oil silk over the application of it to the skin; by retaining the moisture of the skin it will favor the action of the oil.—[*Transylvania Med. Jour.*

Singular Case of Foreign Substances in the Intestinal Canal.

By D. HAYS AGNEW, M. D., Philadelphia.

The following case I am induced to report from its very singular character:—On examining the body of an individual who, I believe, labored under some mental alienation during life, my attention was attracted to an adhesion between the parietal and visceral layer of peritoneum over the cæcum, upon the separation of which a small opening was perceived through the walls of the intestine, disclosing a dark-looking substance, which, upon examination, proved to be a large mass of straw, little less than an ordinary sized fist, and firmly impacted in all the space below the ileo-cæcal valve. Noticing the transverse colon very much distended, an incision was made into its cavity, where were found a pair of suspenders, three rollers, and a quantity of thread, interwoven with one another. The webbing, which evidently was his suspenders, exceeded one and a quarter inches in breadth, and must be several feet in length, inasmuch as it extended through the ascending, transverse, and a portion of the descending colon, and doubled in several places upon itself. The rollers were of ordinary muslin, over one inch in width and the same in diameter, but which must have been of much greater size when swallowed, as they had, in their progress along the intestine, become unrolled, leaving long ends which were encased within layers of fæculent matter. The peritonitis, which no doubt had been the principal cause of death, was not, however, produced by the escape of any intestinal matter into the serous cavity, no such discharge having occurred, the opening into the cæcum only presented itself after the reflected layer of the peritoneum was separated therefrom. Had life been prolonged, it is highly probable that the ulceration would have extended through the walls of the abdomen, and the cæcal contents passed out by this artificial route.

[*Medical Examiner.*

Miscellany.

Some Observations on Hip-Joint Disease and its Rational Treatment.

By LOUIS BAUER, M. D., Physician and Surgeon (Berlin), Member of the Royal College of Surgeons of England, &c., &c.

Under the above caption, we find in the New York Journal of Medicine for Sept., 1853, a most excellent paper. The importance of the subject, and the ability with which it is treated will justify us in presenting our readers the following extracts:

"It is of great importance in the treatment of hip-joint disease, that its essential origin should be properly understood and appreciated; and be it therefore repeated—

"1. Hip-joint disease is primarily seated in the caput femoris.

"2. Hip-joint disease originates exclusively from tubercular depositions into the cells of that bone.

"3. The object which nature purposes to realize by those depositions, is the elimination of the noxious tubercular substance from within the organism.

"4. The softening of the tubercles begins, either in consequence of some accidental traumatic or other injury, or spontaneously, by the gradual juxtaposition and augmentation of the tuberculous matter, which becomes at last obnoxious to the bone, and sets up reaction and inflammation.

"5. As soon as this process takes place, the tuberculous mass softens, and approaches the surface of the bone, involving more or less the periosteum, cartilage, and synovial lining of the joint in destruction.

"6. If the matter reaches the external surface in the shortest way, by penetrating the neck of the femur—a rare case—the destruction may be trifling, and the cure soon consummated, as far as the joint itself is concerned. But, if the matter enters the articular cavity, a more extensive destruction must necessarily be the result. In this latter case we perceive a thickening and softening of the articular cartilage, which separates into fragments, and, sloughing with its bone, floats about within the articular cavity, adding the new disadvantage of a foreign body to the disease. The synovial sac ulcerates consequently, and the whole joint is more or less transformed into an osseous ulcer. Small fistulæ lead in different directions to the external surface, and the environs of the joint are rather infiltrated, and indurated.

"7. In this state the disease continues as long as there are still tuberculous deposits and cartilaginous fragments to be removed, provided that death does not earlier close the scene.

"8. Nature effects the restoration in various ways, either by establishing a true ankylosis, with deposits of bony matter within and around the acetabulum, with the loss of all mobility of the joint, unquestionably a very rare case, or the two opposite articular surfaces

unite by fibrous substance (spurious ankylosis), which is the usual termination of hip-joint disease. In extremely rare cases, the whole joint is filled up with fibrous formations, interfering greatly with the mobility of the hip-joint; but, in most of the cases, fibrous adhesions exist only partially, while another part of the articular surface is denuded even of its cartilaginous covering and smooth-like, being polished. As a modification of the termination of hip-joint disease, we meet also with luxations of the head of the femur. It is easily understood that cases of the last description must necessarily be of very limited number, and that a most extensive carious destruction is to be premised, both of the femoral head, and the supercilium and labrum acetabuli. The luxation of the femur is, by no means, of such frequent occurrence as Rust and other German authors have stated; but of this hereafter."

The author, after dwelling upon some of the important symptoms of the disease, directs attention to its treatment, and asks the significant question: "*Does the present state of our knowledge afford us the means of shortening the process of hip-joint disease?*"

"Two medical agencies have been put in requisition to realize this object; they are external derivation and the resection of the diseased portion of the femur. During the last century derivatory appliances, near the affected joint, have been used to a great extent, from the simple rubefacient up to the positive destruction of the skin by cauterium actuale and potentiale, and a fair trial has been consequently given to them to prove their beneficial results. The numerous advocates of derivation assert boldly, that nature was doing the same by the operation of metastasis, in depositing obnoxious substances in organs of inferior consequence, thus freeing others of great importance for the maintenance of life, of which, indeed, daily experience furnishes us with many instances. If metastasis were identical with derivation, the conclusion would be correct. But I entirely disagree on this point; for the metastatic deposits of nature are materially of the same quality, whereas artificial derivation merely excites inflammation, and produces a discharge of ordinary character. Thus we may artificially transfer an erythematous inflammation of the pleura upon the skin by a mustard poultice, and put a positive stop to primary disease; in fact, we may succeed in subduing diseases by derivation, in as far as both the disease and the effect of derivation constitute the same character; but it might be an impossibility to influence by derivation the process of hip-joint disease, where the cause of the affection is a specific substance purposely deposited for elimination, while inflammation is the mere effect of the undue pressure upon the osseous texture. And, supposing derivation could even direct the tuberculous matter from the system to the artificial issue, which is by no means evident, it would in no way affect the deposits already formed within the cells of the femoral head. There the inflammatory process will and must go on for the final elimination of the tubercu-

lous deposits. Any positive counteraction can only tend to protract the course of hip-joint disease to an unnecessary length, and place even life in jeopardy. While it is thus exceedingly doubtful, at the least, whether derivation has any influence upon the tuberculous disease in general, and while it is positively impossible that derivation should affect the tuberculous deposits already formed, we know it to be a fact, that extensive issues particularly cause a new irritation, enfeeble the system already emaciated, and may even endanger life.

"Thus we must unavoidably come to the conclusion, that derivation, in cases of hip-joint disease, is not only to be dispensed with, but even to be severely condemned. But what are the results that experience gives us in reference to the usefulness of derivation in hip-joint disease. I need only state, that it has been entirely abandoned both in England and Germany, where, formerly, scarcely one patient escaped the application of the *ferrum candens*.

"As to the usefulness of resecting the caput femoris in hip-joint disease, to answer the indication of speedily removing the morbid substance destined for elimination, I must candidly confess that I have had but little opportunity of witnessing and watching personally the effects of that operation. In as far as statistics afford the premises for correct inferences, I must decidedly object to it, and John Gay, Esq., the highly talented surgeon of the Royal Free Hospital in London, has, in his elaborate paper on the treatment of the ulcerative diseases of the joints, read before the Medical Society of London, stated, with convincing arguments, that very little good is to be expected from so extensive an operation. According to the statistics of this operation, which that surgeon placed under the consideration of that learned and respectable body, the greater number of the patients subjected to the operation in question died from the immediately succeeding inflammatory reaction, while another fraction died from tuberculosis, and a very insignificant proportion only survived. This is a decidedly discouraging statement, and its correctness and truthfulness cannot be questioned.

"In the place of resection, Mr. Gay proposed free incisions into the diseased joint, and even into those parts of the bones filled with tuberculous deposits. This practice he had already followed during a certain period with the best success, although only in the minor joints. A very lively discussion ensued, in which many eminent members of the profession participated. Experience, reasoning, and sophism were brought to bear against the new practice, but with the least possible success. The author in replying at the end of the debate to his antagonists, remarked that a diseased joint was nothing else than an ulcer in the bone, or cartilaginous texture; the joint was partly destroyed, and had thus ceased to exist. The violent reaction which regularly follows the opening of a joint, referred only to healthy articulations, but in no way to articular ulceration. The free access of air to the articular cavity was less injurious than the decomposed matter, tuberculous substance, or the fragments of mortified cartilage.

"To procure in all deep-seated abscesses and ulcerations a speedy

and free issue, was a fundamental principle in surgery, and especially in those cases strictly to be adapted, where fragments of mortified cartilage, identical with foreign bodies, constantly keep up irritation and suppuration, thus protracting the termination of the disease. All *a priori* arguments could not dispute the beneficial results, already attained by his practice.

"These new principles of Mr. Gay opened at once a new era for the treatment of the joint diseases, and although old surgeons shook their heads at the boldness of modern surgery, the younger part of the profession embraced the new chance for success with eagerness.

"In some of the metropolitan hospitals, as well as in county infirmaries, the new measures were frequently put to the test, and as far as I could learn, the results have been favorable. My own experience of Mr. Gay's treatment, both in London and Manchester, obliges me to declare in its favor; it is in my opinion the only direct remedy for the abbreviation of hip-joint disease, and it is not, to my knowledge, followed by any dangerous reaction or fatal consequences. I understand that Dr. John O. Stone, the distinguished surgeon of Bellevue Hospital, New York, advocates the same practice, and has since 1847 derived great benefit from it.

"The next important question is: *Do the modern improvements in surgery furnish us with the auxiliary means of preventing or curing those deformities, which are the constant associates of hip-joint disease?*"

With a brief allusion to the progress of modern orthopedic surgery, the writer adds:

"At about the same time Dr. Buehring, of Berlin, the highly gifted nephew of the immortal Dieffenbach, Dr. Loriner of Vienna, and Dr. Bonnet of Lyons, entered into minute researches on the subject in question, and obtained universally the same result, namely, that the former theory of hip-joint disease was fictitious and arbitrary; that the deformities connected with that disease had a dynamical, and by no means an organical foundation, and thus bringing at once, the most distressing mutilation within the reach of surgical aid. Some difficulties, as to the mechanical contrivances, were soon surmounted, and the new discovery put to a practical test. I am happy to state that already results have been obtained, which leave no doubt as to the efficiency of the new systematic treatment, and I myself can confirm them by my own experience.

* * * * *

"Thus it appears that we possess in permanent extension a sovereign remedy for overpowering muscular reflex action, and ultimately restoring the greatly distorted form.

"The *modus operandi* of mechanic extension is of course still a mere problem; we know at present only the fact. The question, in reference to the general curability of those deformities, originating from hip-joint disease, may safely be answered in the affirmative.

"But it remains yet to be decided, whether permanent extension may effectually be applied to the purpose of preventing those deformi-

ties ; or, whether it be advisable not to interfere while the disease of the hip-joint is progressing, and to have recourse to it only when that malady has terminated its phases.

"Some surgeons will have the orthopædic treatment postponed until the morbid process within the joint is completely terminated; others are in favor of acting immediately. The decision cannot be difficult. It is far better to prevent a deformity than to cure it, especially if there are no contra-indications of immediate extension. The excessive pain has been mentioned as such, and no doubt will set in with injudicious orthopædic treatment. But we have it in our power to avoid it. If in the very beginning of hip-joint disease we place the hip in a firm position, which secures both the rest and extension of the afflicted leg, and the comfort of the patient, as we ought to do, there is no reason why the patient should not be capable of enduring it, provided the mechanical contrivances answer the purpose. On the contrary, the patient will be grateful, as such position saves him the pain, unavoidably caused by accidental movement. In fact, the early use of mechanical treatment has every thing in its favor.

"1. It meets the first rule of surgery, by giving the affected limb a quiet position.

"2. It counteracts the reflex action, and prevents consequently the deformity.

"3. It keeps the caput femoris in the anterior and lower part of the acetabulum, thus preventing any luxation to which circumstances and the extent of caries might tend.

"4. It does not cause, but relieves the pain.

"But if the patient is already in an advanced stage of the disease, it will be necessary to place him under the full influence of chloroform before the apparatus is applied. In this state reflex action and sensation are arrested. We may with impunity and at once stretch the patient, and give him that position in the instrument which we deem proper. When the effects of the chloroform have passed by, and the patient is returned to consciousness, he will only not complain, but on the contrary will feel greatly relieved, and more comfortable than before. At least I have obtained these results in all cases under my personal care, and there is no reasonable ground why it should be otherwise. To keep the patient in the position once assumed, will suffice to prevent or cure the deformity. Should there however remain a slight deviation, I prefer the repeated use of chloroform and the total restoration of the form under its influence, to the gradual increase of the extension, which is extremely painful.

"If, however, the disease of the hip-joint is completely removed, and we have only to contend with a painless muscular retraction, a reflex action of some duration, I prefer the gradual extension to the former, for a sudden extension has not in this case the desired effect, and might bring on a new irritation in the joint, which Dr. Buehring experienced. In point of fact, gradual extension is better calculated to neutralize a long established and powerful reflex action, than the sudden reduction of the deformity."

Dr. Bauer finally proposes an apparatus, of which a diagram is presented, by means of which he secures: "1st. Stretching of the knee and hip-joint; 2d. Abduction of the affected limb; 3d. Restoring the parallelism of the transversal diameter of the pelvis with the horizon; and 4th. Comfort and convenience to the patient."

Spermatorrhœa Rings.—Quite a revolution has been effected in New England, in less than a year, by the use of a mechanical invention, instead of medication, in the treatment of a formidable malady. Heretofore, tonics, accompanied by a long series of auxiliary assistants, such as jaunts, horseback, sea bathing, a regulated diet, besides innumerable preparations of drugs, have been prescribed to arrest the effects of spermatorrhœa, but rarely with any permanent success. It is a condition resulting, in most instances, from the indulgence of a pernicious vice. In prisons, self-pollution is nearly universal, and no ingenuity on the part of wardens or attending physicians has prevented it for any length of time. Some suffer intensely, and even die, from excessive indulgence in this vice. Schools, too, and colleges, are often the nurseries of this degrading habit, which carries many young men to an early grave, often without the true cause being suspected. The weak eyes and continued headaches so common among students at public institutions might in many instances be traced, if effort was made in the right direction, to this perpetual violation of a physiological law. The rings, which this Journal was the first to announce, are a sure remedy for involuntary forms of the disease. Physicians are eminently successful with them. In the State Prison at Charlestown, where Dr. Bemis has given them a thorough trial, we understand they have performed many cures. In private practice, also, testimony from the most reliable sources might be cited to strengthen the medical public's confidence in this simple and only effectual relief in these cases. Dr. Cheever has shown us another improvement of the instrument. It is far lighter than the former patterns, and the middle ring is better balanced in the centre of the large one. The simplicity of the adjustment to any sized organ, makes it more economical, too, which is a consideration not to be overlooked. We admire the ingenuity displayed in the manufacture, and predict, from the great success that marks their application to severe and long-protracted cases of individual suffering, that the rings will be very extensively used in other parts of the world as well as in America.

[*Boston Med. and Surg. Journal.*]

Removal of a Ring from a Young Lady's finger. (Communicated for the Boston Medical and Surgical Journal.) By A. C. CASTLE, M. D., of New York. The following case may not be unacceptable to the readers of the Journal.

An interesting young lady, about 17 years of age, had presented to her a gold ring, which she forced over the joints of her middle finger.

After a few minutes the finger commenced swelling, and the ring could not be removed. The family physician, Dr. ———, was sent for but could do nothing. The family, and the young lady especially, were now in the greatest consternation. A jeweller was sent for. After many futile attempts to cut the ring with cutting nippers, and to saw it apart with a fine saw, and after bruising and lacerating the flesh, warm fomentations and leeches were applied, but all without affording the slightest benefit. Dr. ——— requested my presence, with the compliment that "perhaps my mechanical ingenuity might suggest something." I at once proceeded to the house of the patient, and found the young lady in a most deplorable state of mental agony, the doctor embarrassed, and the family in a high state of excitement. I procured some prepared chalk, and applied it between the ridges of swollen flesh, and all round the finger, and succeeded in drying the oozing and abraded flesh; then with a narrow piece of soft linen I succeeded in polishing the ring, by drawing it gently round the ring between the swollen parts. I then applied *quicksilver* to the whole surface of the ring. *In less than three minutes* the ring was broken (by pressing it together) in four pieces, to the great relief of all parties.

In a similar manner (without the chalk) I some time since extracted a small brass ring from the ear of a child, who, child-like, had inserted it into the cavity of its ear. The operation was more painful and tedious, but was equally successful.

The modus operandi. The quicksilver at once permeates the metals *if clean* (with the exception of iron, steel platina, and one or two others,) and amalgamates with them. It immediately crystallizes and renders the metal as hard and as brittle as glass. Hence the ease with which metals amalgamated with quicksilver can be broken.—[*Ib.*

Saccharate of Lime. By M. TROUSSEAU.—The virtues of this preparation are due to the fact that sugar in solution is capable of absorbing a very large quantity of lime. The compound is made by saturating simple syrup with lime, and then filtering it. A perfectly transparent mixture is thus obtained, which is not troubled by admixture with water, and is characterized by an extremely alkaline taste. This syrup combining with water in any proportion, is a convenient and valuable mode of administering lime. The attention of physicians was first called to the article by Dr. Capitaine, adjunct to the Faculty of Medicine, and it was first employed by myself at the Necker hospital, in the treatment of the chronic diarrhœas of children. The dose for an infant is from a fourth to half a drachm, and for an adult, from one drachm to two and a half. At the Necker hospital, I was in the habit of mixing a small portion of this preparation with the milk allowed each suckling during the day, and it seemed to me to obviate the tendency of the milk to acidity in the stomach, and to prevent the disposition to diarrhœa so common in children of a certain age at particular seasons. In comparing the effects of the saccharate of lime with those of the bicarbonate of soda, I found the former to

possess marked advantages.—[*Trousseau, Traité de Thérapeutique. Virginia Med. and Surg. Journal.*]

Cold as an Anæsthetic Agent.—We lately witnessed, says the editor of the *Lancet*, the employment of cold as an anæsthetic, in St. Bartholomew's Hospital, upon a patient suffering from chronic inflammation and enlargement of the knee joint.—A bag of pounded ice was applied for about four minutes, by Mr. Lawrence, over the swollen parts; then, with a heated knife, six or more incisions were made by the side of the patella, extending through the skin and subcutaneous tissue. The patient suffered *no pain whatever*. The counter-irritant effected a marked diminution in the swelling which was of a character sufficiently grave to suggest the idea of the possible necessity of amputation of the limb.—[*New-Orleans Med. and Surg. Journal.*]

Seidlitz Powders.—The necessity for using two papers may be obviated, and a very satisfactory preparation obtained, by mixing two parts of bitartrate of soda with one part of bicarbonate of soda. The mixture keeps well even in paper, and effervesces briskly when mixed with water.—[*Dublin Med. Press.*]

Yellow Fever at the South-west.—The extension of yellow fever to almost every port in direct steam-boat communication with New Orleans, where it made its first appearance in the United States during the recent summer, should open the eyes of civil authority to the importance of quarantine regulations calculated to prevent a repetition of such calamity. So far as we have been able to learn, it is abundantly established that the pestilential cause of yellow fever can be carried from place to place by steamers and other ships—but more certainly by steamers, in consequence of the rapidity of their travel, than in slow moving vessels; and that this cause, when diffused in an atmosphere susceptible of propagating it, does become domiciliated, if we may use the expression, and spreads over an area more or less extensive, according to circumstances. It appears equally certain that the cause of the malady cannot be carried about by the human body so as to induce yellow fever in those who have not been exposed to the atmosphere of the poisoned district.

With these facts before us, it seems imperative to quarantine *vessels*, but not *individuals*. How far the restriction should operate upon cargoes or other fomites is a question upon which we are not so well informed. It ought to be referred to the medical profession of the ports most interested, for it cannot be expected that persons interested in commerce and navigation will possess either the requisite knowledge or the absence of bias so essential in such an arbitration between traffic and health.

BIBLIOGRAPHICAL.

Hallucinations : or the rational History of Apparitions, Visions, Dreams, Ecstasy, Magnetism and Somnambulism. By A. BRIERRE DE BOISMONT, &c., &c. 1st American, from the 2nd enlarged and improved Paris edition. Philadelphia : Lindsay & Blakiston. 1853. 8vo., pp. 553. For sale by T. Richards & Son, price \$2.50

The work before us, by one of the most erudite and strong-minded philosophers of the age, is remarkably interesting as well as instructive. We know none in which the difficult subject of Hallucinations is so logically treated, and the attention of the reader so agreeably entertained. The tedium of the investigation is prevented by the narrative of upwards of 180 cases in illustration of the subject in all its phases. As we fully agree with the Translator in his estimate of the work, we cannot do better than to quote the following paragraphs contained in his Preface :

“ M. Brierre de Boismont treats the important and hitherto neglected subject of hallucination in various points of view, inasmuch as it bears important relations to philosophy, medicine, religion, history, morality, and jurisprudence. The apparitions of Holy Writ are handled with the reverence befitting a Christian, whilst much light is thrown on the probable origin of the hallucinations of many celebrated personages whose characters and actions were so exalted as to place them apparently above humanity.

“ The book will offer attractions to many classes of readers. The *theologian* will admire the vein of reverence and morality which pervades it ; the *philosopher* will find much food for study and contemplation ; the *practical physician* will avail himself of the knowledge and experience detailed in a great variety of cases ; the *lawyer* will be deeply impressed by the necessity of vigilance, and a close study of the case, before he gives in his verdict of insanity ; while the lover of the marvellous will find ample food for the gratification of his taste, in the number of strange and picturesque authenticated facts thus carefully collected.”

Practical Observations on Aural Surgery, and the nature and treatment of Diseases of the Ear. With Illustrations. By WM. R. WILDE, F. R. C. S., of Ireland ; Surgeon to St. Mark's Ophthalmic Hospital, &c., &c. Philadelphia : Blanchard & Lea. 1853. 8vo. pp. 475. (For sale by T. Richards & Son.)

This is just such a work as we needed, for we possessed no treatise on the affections of the ear, in our language, to which we could refer as a faithful exponent of the present state of knowledge in that department. So far as we have had time to look into it, we have every reason to think that it will be found eminently practical and consequently useful. There are no diseases so often left in the hands of charlatans

as those of the ear—perhaps because of insufficient knowledge on the part of physicians—but mainly in consequence of their intrinsic difficulty and the unscrupulous promises made by those who pander to the credulity of patients. Mr. Wilde dwells particularly upon the importance of a correct diagnosis, by which the enlightened practitioner may discriminate between the cases that may be benefitted by treatment and those which are incurable, and be able to announce at once and honestly to the patient what he has to expect.

We take pleasure in recommending this work to our readers.

A Treatise on Operative Ophthalmic Surgery. By H. HAYNES WALTON, F. R. C. S., &c., &c. Illustrated by 169 engravings. Edited by S. LITTELL, M. D., Surgeon to Wills Hospital, &c., &c. Philadelphia: Lindsay & Blakiston. 1853. 8vo., pp. 598. (For sale by McKinne & Hall—price \$3.00.)

We have to add another to the numerous works recently published upon *operative Surgery*. The *Operative Ophthalmic Surgery* of Mr. Walton is, however, entitled to our unqualified commendation, as a careful compend of the present state of knowledge in regard to the operations that may be performed upon one of the most important organs of the body. We must confess that we can see no good grounds for separating the *principles* from the *practice* of any branch of Surgery, and that a work in which may be found the *whole* pathology and treatment of affections of the eye must be more useful to the general practitioner than one in which he can only find the rules for operating. The work before us is peculiarly valuable to the special oculist.

The Maternal management of Children, in health and disease. By THOMAS BULL, M. D., M. R. C. P., author of "Hints to Mothers," &c. 2d edition. Philadelphia: Lindsay & Blakiston. 1853. 12mo., pp. 424 (For sale by McKinne & Hall—price \$1.)

This is one of those unpretending little works which ought to be read and studied by every physician as well as mother. By the physician, because it treats of details too often considered beneath his notice; and by the mother, because she will be enlightened at every page upon subjects which appertain specially to her mission. The author states that in England and Wales, "*one child in five dies within a year after birth, and one in three before the completion of the fifth year!*" In the large cities of Europe and America the ratio of mortality is probably still greater. There can be but little doubt that such a frightful state of things is in a great measure attributable to ignorance of the commonest principles of infantile hygiene and to

consequent mismanagement. The family physician would add much to his satisfaction as well as to his success, by urging mothers to procure and to study this work of Dr. Bull.

A Practical Treatise on the Diseases of Children. By D. FRANCIS CONDIE, M. D., &c., &c. 4th edition—revised and augmented. Philadelphia: Blanchard & Lea. 1853. 8vo., pp. 732. (For sale by McKinne & Hall—price \$2.75.)

American medical literature may justly lay claim to great excellence on the subject of the diseases of children. The works of Dewees, Eberle, Stewart, Condie and Meigs would do credit to any country. The fact that Dr. Condie's treatise has now reached its fourth edition, shows conclusively the high estimate placed upon it by the profession. The present edition has been carefully improved by the learned author.

The Microscopist; or a complete manual on the use of the Microscope, for Physicians, Students, and all lovers of Natural Science. 2nd edition, improved and enlarged; with illustrations. By JOSEPH H. WYTHES, M. D. Philadelphia: Lindsay & Blakiston. 1853. 12mo., pp. 212. (For sale by McKinne & Hall.)

Every man of science has now (or ought to have) a microscope. The object of the work before us is to facilitate the acquisition of those details without the knowledge of which the use of this beautiful and valuable instrument cannot be satisfactory. It is concise, to the point, and therefore peculiarly valuable.

Cox's Companion to the Sea Medicine Chest, and Compendium of Domestic Medicine; particularly adapted for Captains of Merchant Vessels, Missionaries and Colonists. With plain rules for taking the medicines; to which are added directions for restoring suspended animation, the method of obviating the effects of poisons, a plain description of the treatment of Fractures and Dislocations, and a concise account of Asiatic or Spasmodic Cholera. Revised and considerably enlarged. By R. DAVIS, M. R. C. S., &c., &c.—1st American, from the 33d London edition. New York: S. S. & W. Wood. 1851. 12mo., pp. 216. (For sale by McKinne & Hall.)

This is certainly the *multum in parvo*. The title page leaves nothing for us to add.

The Physician's Visiting list, diary, and Book of Engagements, for 1854. Philadelphia: Lindsay & Blakiston. (For sale by McKinne & Hall.)

This is quite a convenient pocket book for physicians in full practice.

SOUTHERN MEDICAL AND SURGICAL JOURNAL.

Vol. 9.]

NEW SERIES.—DECEMBER, 1853.

[No. 12.]

PART FIRST.

Original Communications.

ARTICLE XXXVII.

Collodion in the Treatment of Various Affections. By JURIAH HARRISS, M. D., of Augusta, Ga.

Since 1848, the year of its discovery, Collodion has been used in the treatment of various affections, particularly in diseases of the skin. No new remedy has been more generally used or more lauded as a topical application than collodion. The influence of authority comes to us from all quarters, in praise of its efficacy in a variety of diseases. Although we should grant a proper amount of force to the influence of authority, we should not be wedded to it, but only accept its dicta when sustained by reason and corroborated by experience. I shall endeavor to give the rational indications which it would seem to fulfil and the experience of the profession in Europe and this country. I have been induced to collect this widely scattered testimony, both for and against its utility, into a condensed form, in order that we might see at a glance, the various affections to which it has been used, the mode of its application and the success which attended the cases. Another inducement has been, the great plausibility of its application to many affections.

I would here state that I have not been able to have access to all the articles upon the subject, but think that I have condensed most of the matter written upon it up to this time. I

will also add, that the eminently practical Southern Medical and Surgical Journal contains a great amount of our information upon the subject.

The first indication which collodion fulfils is the formation of an artificial cuticle. In any abrasion of the cutaneous surface, nature makes an effort immediately to throw out in some form a covering for the delicate tissues, to protect them from external and injurious influences. Following up this indication so manifestly shewn us by Nature's teachings, collodion has been applied, and the result has proved satisfactory. M. Latour, believing that atmospheric air was a great stimulus to inflammation even in deeply imbedded tissues, says that collodion acts, not only by excluding air directly, but indirectly. From this belief, he was induced to apply it to rheumatic joints, and to the abdomen in peritonitis. This excluded the parts from the injurious effects of cold air and currents. His success confirmed him in his opinion.

Collodion must, it would seem, act beneficially in this way in erysipelas. Who does not know that the latter disease is frequently *caused* and yet more *often aggravated* by currents of air? If then you secure this affection from such a contingency a great point is certainly gained. If, in addition, you compress the capillaries, force the blood from them, you employ a local antiphlogistic, and rid the affected part of all the characteristics of inflammation, redness, swelling, heat and pain, and the disease must necessarily yield. The second indication, which collodion fulfils, is then done by its power of contraction.

The transparency of the article enables us to see the action of the diseased part beneath the coating, and to note its progress or retrogression. Its cleanliness is an advantage which ointments, &c., do not present when applied. The relief which it affords in obviating distension, pain, itching, &c., are desiderata which should not be overlooked.

I have not space, however, to enlarge upon the plausible advantages of this remedy, and shall therefore proceed to give the experience of the profession in its use.

Mr. G. P. Maynard, of Boston, in 1848, then a medical student, first prepared and used it in surgery, as a substitute for

plasters and sutures in dressing wounds. Dr. Whitney, his preceptor, also testified to its applicability to wounds, particularly when plasters or sutures are impracticable or inadmissible. "In most instances," he says, "it was applied in conjunction with straps of cotton and sheep-skin, and with raw cotton, forming with them strong, unyielding, adhesive straps, bandages and encasements." During the same year it was applied to the relief of tooth-ache.—(*Ranking's Abstract*, 1849, from *Am. Jour. of Med. Sciences*, 1848.)

In Nov., 1848, Erasmus Wilson employed this agent topically in the treatment of various affections of the skin: chronic erythema, chapped nipples and hands, herpes labialis, herpes preputialis, and herpes zoster, lichen agrius, lupus, acne vulgaris, and several other affections. He found that in chapped hands it was not only a protective covering, but promoted the healing of cracks more rapidly than any other remedy. He certainly speaks rationally, when he says, "in chapped nipples, it was even more efficient in its protective and curative action—the gaping cracks were instantly drawn together and almost obliterated by the contracting power of the remedy, and were effectually shielded from the influence of moisture and the pressure of the gums of the infant, and all in consequence of the rapid evaporation of the ether." During this year collodion continued to be used and its application generalized.

In 1849, M. Hairon employed it successfully in ophthalmic affections. "To be enabled," he remarks, "to protect the inflamed cornea from contact with the air, to prevent the movements of the eyelids over its surface, and retain topical applications long in contact with it, are objects which, if realized, would much diminish the severity of keratitis and conjunctivitis." It appears to me that this would prove an admirable agent to prevent friction, and consequent inflammation and ulceration of the cornea in granular inflammation of the eyelids, and to relieve the cornea from the offending cause when the disease exists. A granular lid will necessarily produce inflammation of the cornea, if not soon relieved (and it is an obstinate affection) or some means taken to prevent friction. M. Barrier, of Lyons, has suggested an improvement in the application of collodion to the lids. M. Hairon simply closed

the lids and put a coating of collodion over them. This was objectionable, because the eye could not be examined without tearing or cutting loose the coating of collodion. M. Barrier proposes applying linen dipped in collodion, parallel with the lashes upon the superior and inferior lids, with threads beneath them. These threads are to be tied, and when the eye is to be examined, they are simply untied and the globe of the eye can be easily reached.

In the *Gazette des Hopitaux*, for 1849, there is a notice of the application of collodion to bed-sores, in which the author states that it proved more serviceable than ordinary remedies; it adheres closely and is exempt from moisture.

Dr. T. R. Mitchell, of Dublin, recommends the application of this remedy in ulcerations of the os and cervix uteri: he thinks it necessary to apply it every forty-eight hours, in consequence of its being detached by the fluid accumulation beneath it, and that the results amply repay the physician for the trouble. The *modus curandi* is the same as in abrasions of the skin.—(*Dublin Med. Press*, 1849.)

The use of collodion in erysipelas was, doubtless, first made by Dr. J. W. Freer, of Illinois, during the latter part of 1849. It was in an epidemic form of erysipelas that the Doctor first applied it and found it successful. All will admit that when epidemic, this disease is more obdurate and intractable than it is in sporadic cases; yet in his hands it proved adequate to the occasion, by relieving pain and expelling the superabundance of blood from the capillaries. He also found it very successful in the treatment of burns. I shall refer to the latter application again.—(*N. Western Med. and Sur. Jour.*, Jan. 1850.) Dr. Dugas, in his report to the Georgia State Medical Society, 1852, upon "Certain New Remedies," mentions a case of erysipelas, which he treated with collodion. "The collodion was persevered in to the last without appearing to exercise any controlling influence whatever." The parts affected were the face and scalp. The patient recovered, but not through the agency of the collodion. We shall see that this is not the only unfavorable report of it. It has, however, been extensively used in erysipelas of the face, in the London hospitals.—(*London Lancet*, July, 1850.)

Collodion has been used to a considerable extent in mammary inflammations, by Prof. Evans, of Rush Med. College, who says, "Disheartened by the general want of success in preventing suppuration by the ordinary means of treatment, and satisfied that the most prominent indication of cure, was to overcome the freedom with which the blood was forced into the mammæ, and by compression cause the absorption of the lymph, as in some, by the roller-bandage applied on the extremities, in various forms of inflammation, I determined to use a complete coating of the collodion to obtain the benefit of its contraction." He made the application morning and night and the results were very satisfactory, as shown by his report of cases. There was but one in which suppuration took place. this is certainly a very beautiful result—far better that can be obtained from bandage, tinct. iodine, &c.—(*N. W. Med. and Surg. Journal*, Sept. 1850.) Dr. J. H. Murphy, in the March No. of the same journal says, that he was induced to try this article in mammary inflammations, from the report of Prof. Evans: his success was so great that he ventured upon its use in Small Pox. In the first case he proceeded with laudable caution and applied it to one of the lower extremities. Pitting was the sequel of the eruption every where, except where the collodion had been applied. The extremity to which the application had been made, presented a "uniform smooth surface." In another case, he says, "I brushed the surface of the face, neck and hands of the patient thoroughly, three or four times a day, for four days. The result was as in the first case, a perfect prevention of the disfiguring effects of the disease, although both patients had the affection in a severe confluent form." The Dr. playfully remarks that the first patient, "hinted ominously malpractice," because his face had not been anointed instead of his leg.

I would here state, that in a conversation with Dr. Dugas upon the use of collodion in mammary inflammations, he said that in his practice it had acted like a charm; that it not only prevented suppuration when early applied, but hastened wonderfully the cure after the abscess had been formed and opened.

In the same No. of the journal, Prof. Evans recommended highly the use of collodion in chilblains; it prevents the con-

tact of the oxygen of the air, which but feeds the flame and produces the intolerable itching and burning sensations characteristic of this affection. He says, the sufferings of the patient were promptly relieved and the progress of the diseased action speedily arrested.

M. Aran, gives his testimony in reference to the use of collodion to prevent pitting from small-pox. He had frequently tried it in varioloid and found it satisfactory; so he determined to try it in the confluent variety of small-pox. He relates the case thus: "It occurred in the person of an unvaccinated young man, and the collodion was applied to all parts of the face but the lips and ears. Through this transparent covering the progress of the pustules was observed to become at once arrested while those uncovered continued enlarging; moreover, a part of the covering having been detached without being observed for some hours, the pustules thus exposed began to develop themselves until again arrested by a reapplication. The ears too, were now covered and the progress of the pustules stopped there."—(*Southern Med. and Surg. Jour.*, April, 1851.) After the detachment of the collodion, no cicatrices were observed, though in other parts they existed in abundance, the eruption having been very confluent.

The July No. of this Journal, for 1851, contains an ingenious plan devised by M. Cunier for the treatment of symblepharon after the adhesions are severed. Every one is doubtless aware that these adhesions are almost certain to reunite after their dissection has been made. M. C. proposes collodion to prevent the re-union of the lids to the globe of the eye. In adhesions of the lower eyelids, a narrow strip of linen moistened with collodion is applied just below the eyelashes and parallel with them, so as to secure two bits of threads placed transversely beneath the linen, with their free extremities hanging down. A similar one, with threads beneath, is fixed over the malar bone; when entirely dry, the threads from above are tied to those from the cheek, sufficiently tight to depress or evert the lid, and maintain it so. "In adhesions of the upper lid the second strip should be placed upon the forehead, for the purpose of drawing up the eyelid when the threads are tied." The eye should be protected from dust by a thick veil or shade. If this treatment is

as successful as M. Cunier affirms it to be, it will rid the oculist of very frequent and embarrassing accidents.

An extract is contained in the *Southern Med. and Surg. Journal*, May, 1851, (from the *Bull. Gén. de Therap.*) on the treatment of Ingrowing Toe-nail, by M. Meynier. "The flesh being pressed aside, a little collodion is poured between it and the nail, which soon dries and keeps the parts asunder until cicatrization is effected." M. Larrey succeeded in four cases out of six.

The same *Journal* (October, 1851) gives the report of a case, by M. H. Gassier, of Hemorrhoidal Tumours, treated with collodion. This was applied after all other remedies had failed, and before the tumor was reduced; and in twenty minutes the patient was very considerably relieved. He was soon cured, and six months had elapsed without a return of the disease.

The *Medical Examiner*, for March, 1851, gives, in a very condensed form, the experience of Dr. Liman, of Boston, on the use of collodion in burns. He states that it allays the smarting, forms a protective covering which excludes the action of the air, and is so exactly adapted to all the parts that no after dressing is required. The Doctor adds, that in one case of extensive burn, there were no contractions of the integuments, after it had cicatrized. This is rather an unexpected result, for we might very readily expect contractions in these cases, from the peculiar action of the collodion, producing, as it does, a contraction of itself. During 1852, collodion was applied with success to *Nævi* and *Entropions*.

Collodion has also been employed to subdue the swelling and relieve pain in *Orchitis*, by spreading it over the scrotum. M. Dechange says that it acts by compression and excluding atmospheric air. This was doubtless employed for the same purpose as adhesive strips and bandages. If the contractile power of collodion is sufficient, it will answer the indications quite as well as the bandage, besides being more convenient to apply. The bandage I have found far more efficacious than any other remedy. Dr. Rossignol, of this city, informs me that he has used it very successfully as a substitute for bandaging. I have not myself tested the virtue of collodion in this disease. It doubtless alleviates pain by preventing farther dis-

tension, for the pain is dependent upon this, and ceases when the distension ceases, but its contraction may be sometimes insufficient to subdue the swelling, which existed before its application.

Dr. Claiborne, in the *Stethoscope*, (March, 1853,) speaking of the application of collodion to buboes, uses the following language: "I use the collodion solely for the purpose of discussing or scattering the bubo, and in my hands it has proved incomparably superior to any other discutient." In this case it acts by compression, subduing any inflammation that may exist. It would seem to be more applicable to indolent, than acute buboes.

A synopsis of the experience of Dr. Christen, of Prague, with collodion, in Small-pox and Erysipelas, is presented in the *American Jour. of the Med. Sciences*, (April, 1853.) The experience of the Doctor is diametrically opposed to that of all others in reference to its preventing pitting in small-pox. In his hands, it not only did not prevent pitting, but was often injurious, by confining the pus and giving great pain. It was frequently necessary to remove it and allow the inflammation to proceed unchecked. "He says, "the further development of the variola was neither prevented nor lessened, but only modified, by the papulæ under the layer of collodion forming even points of pus, which, as the suppurative process advanced, became confluent, and in severe cases formed a nearly uninterrupted layer. The evaporation of the liquid parts was so impeded that fluid pus was still under the collodion, when the pustules on other parts of the body had dried and crusted." Dr. Christen's experience with it in erysipelas is scarcely more encouraging than in variola. In symptomatic, erysipelas, he found it of no advantage whatever, but admits its efficacy in this disease when arising from local causes. These results are rather surprising, inasmuch as the reports of most others concur in testifying to its efficacy in these affections.

It appears that M. Guersant has employed, with considerable advantage, collodion and castor oil as an application to erysipelas. *Formula*—Collodion, 30 parts; Castor oil, 2 parts. Mix. This was applied every day. The idea of mixing these materials is due to M. Robert Latour.—(See *Braithwaite's Retros.*, July, 1853.)

Seeing the good effects of the salts of iron in erysipelas, M. Aran was induced to combine them with collodion, forming a preparation which united the compressive and astringent effects. This preparation consisted of equal parts of collodion and ethereal tinct. of the perchloride of iron. He states that it forms a somewhat thinner pellicle than ordinary collodion, but is much more supple and resisting, so that the limb can be moved in any direction, without cracking. Its adhesion is also more prolonged.—(*Bull. de Therap.*)

A rather singular application of collodion was made by Dr. Doring, but with the desired effect. In an obstinate and unyielding case of priapism, accompanying gonorrhœa, he enveloped the whole of the penis of his patient in a coating of collodion; from that moment, he says, the patient had no erection, and suffered only from a slight scalding. It was necessary, however, to renew the application, as the erection returned when the collodion was taken off, but not so severely. Two applications only were made.—(*Med. Examiner*—and *South. Med. and Surg. Jour.*, July, 1853.)

My own experience with collodion has been very limited. I have attempted to arrest the development of furuncles with it, but have entirely failed. Whether my patients carried out the instructions or not I am unable to say. The true cause of failure is, doubtless, owing to the late hour at which physicians are consulted. Patients usually come to us when the boil has suppurated and is ready to be opened. This is also the case with whitlows; though in this affection I have but little confidence in the power of collodion to prevent or rather subdue the inflammation. The disease is usually too deeply seated. The contraction of the collodion would perhaps only increase the pain, inasmuch as it is known to be caused by the resistance of the dense fasciæ of the fingers. In cases where boils have been caused to ulcerate, by friction of clothes, or by pressure, collodion has acted well—healing the sore in a short time.

I have applied collodion to but one case of hemorrhoidal tumors, and in this it proved beneficial. The tumors came down after each evacuation of the bowels, which occurred once or twice a day, and the collodion was applied each time, but after the tumors were cleansed with water. The first applica-

tion gave some pain, but in a few moments great relief was experienced. No succeeding application caused pain. The collodion was persevered in for three days, when relief was complete. I do not consider this, however, a permanent cure. The tumors will probably return.

I used collodion in one case of burn of the hand from sulph. acid. The burn was, however, very superficial, the cuticle only having been removed. The hand was placed in a weak solution of chloride of soda for a few moments, which relieved the pain, and then a coating of collodion was placed upon it, and the young man resumed his work without discomfort. Collodion has also relieved, temporarily, tooth-ache, in my hands; doubtless by excluding air. When the coating has been removed, another should be placed upon the tooth. A bit of cotton dipped in collodion has been recommended, and used by dentists, as a temporary filling for carious teeth, with advantage.

ARTICLE XXXVIII.

A Case of Femoral Exostosis; with Remarks. By L. A. DUGAS, M. D., &c.

Mr. George B. . . , of Newton county, Ga., came to this city on the 7th of February last, and placed himself under my professional care. He was 28 years of age, of slender habit, and, although rather delicate, did not appear to be in bad health. He bore, however, an exostosis, the origin of which he attributed to a fracture or other injury sustained when about one year of age, and since which time he has been troubled with a tumour upon the anterior surface of the lower third of the right femur, and occasional pains in this region. He stated that the tumour and the pains had increased very much during the last five years. Any exercise now caused pain and swelling of the soft parts adjacent, which would subside in a week or ten days, by repose and emollient applications. For the last twelve months he had been thus incapacitated for business or enjoyment of any kind.

Upon examination, I found a considerable tumefaction at the junction of the lower with the upper two-thirds of the thigh,

painful when grasped, and evidently occasioned by a considerable exostosis, which bruised the muscles whenever these were used. The crureus and rectus femoris appeared to have lost much of their thickness, and the exostosis was consequently separated from the skin by a very thin stratum. The vasti seemed larger than those of the other limb, the affected thigh being on the whole much larger than the sound one.

As the patient had come for the purpose of submitting to what he thought a comparatively trivial operation, I deemed it proper to undeceive him, and at once told him that I regarded the removal of such an exostosis as so serious that nothing but the most urgent necessity could induce me to attempt it; that his constitution was evidently not robust and would perhaps yield sooner than he expected, &c., &c. He replied, that his life was a burden to him under existing circumstances, and that he would rather take any risk than remain as he was. I then requested him to remain in bed and to apply emollients, in order to subdue the acute symptoms induced by travel; adding that I would ask some of my professional friends to see him. Professors Newton and Miller, and Drs. Campbell, Harriss and Rossignol accordingly visited him; all of whom concurred with me as to the seriousness of the case, but thought an operation proper under the circumstances.

The temporary aggravation having subsided, and the patient being fully prepared, mentally and physically, the operation was performed in the presence of the above-named gentlemen and some others on the 17th February, as follows:—The patient was placed horizontally upon his bed, with the affected limb extended and the foot resting upon a chair; chloroform was administered until insensibility was induced, but not carried so far as to produce profound coma; a longitudinal incision about eight inches in length was made upon the anterior surface of the thigh and down to the exostosis. This gave issue to a large quantity of black blood, mixed with grumous matter, apparently resulting from the contusion of the tissues, which were found to be quite dark and ragged; the wound being cleansed, the knife was carried around the exostosis so as to bring its entire volume fully into view; the flaps were now drawn asunder with bent spatulas and the exostosis sawed off on a level with the shaft of

the femur. The hemorrhage was comparatively slight, and no vessel required a ligature. The patient regained his consciousness as the operation was completed, and the wound was allowed to remain open some time to detect any tendency to bleed. A roll of lint the size of a man's finger was now placed upon the sawed surface and projected out at the lower end of the incision, in order to secure a free issue of matter; adhesive strips brought the flaps in apposition and the patient was put to bed.

Thus far every thing seemed to promise well, and the patient was highly gratified at having undergone the operation without pain. But in a few hours he began to complain of a jerking in the limb; for which an opiate was ordered, and repeated several times during the night without much relief.

18th. Passed a bad night; pulse excited; no appetite; still much annoyed with jerking of the muscles of the affected limb; opium and camphor ordered at stated intervals.

19th. Has had another bad night; pulse more excited than heretofore; looks pale and takes no nourishment; the wound discharges freely a dark, blackish matter; thigh considerably swollen. Adhesive plasters loosened to facilitate the discharge; wound cleansed with chloride of soda and water; complains of nausea, for which *ess. peppermint* and brandy toddy were occasionally given.

20th. Evidently much worse; pulse very frequent; face pale; nausea increased to vomiting; refuses all food, but craves cooling beverages; the matter discharged is very offensive; tumefaction increased; is more disposed to sleep. Adhesive plasters again raised; no adhesion of flaps; wound cleansed as heretofore—and brandy continued, with broth.

21st. Died at 10 A. M. very quietly, having been delirious through the night whenever awake.

The exostosis was carefully examined after its removal, and found to consist of an exceedingly compact osseous tissue, which, however, passed gradually into the cartilaginous state as the surface was approached. Beneath the periosteum, the surface was cartilaginous and mammillated, especially about the summit of the exostosis. Upon a careful inspection of the specimen, with the aid of the microscope, by Dr. Harriss and myself, we found the structure to resemble very accurately that of

healthy bone, and cartilage undergoing ossification near the surface. The tumour was evidently growing at the time of its excision. There was no appearance whatever of cancerous nor of tubercular matter about it.

REMARKS.—The above case is both interesting and instructive; painfully interesting because of its issue, and eminently instructive because of the paucity of light thrown upon such affections and operations by systematic writers. Indeed, it is truly surprising that so few surgeons speak of femoral exostoses from actual observation. Those who treat of exostoses at all, do so in such general terms as to leave the reader without any valuable information in cases similar to the one under consideration.

That our case was one in which excision was warrantable, can admit of no doubt; yet we are free to admit that, under similar circumstances we would now prefer resorting to amputation than to excision.

In his First lines of the Practice of Surgery, Samuel Cooper does not treat of Exostosis, but he devotes several pages of his Surgical Dictionary to the subject. With regard to the treatment of exostosis, in general, he remarks that "our ignorance of the pathology of exostoses, particularly their causes, accounts for the imperfection of our treatment of them. With the exception of the venereal exostoses, or nodes, there is no species of this affection, for which it can be said that we have any one medicine of efficacy." After alluding to certain cases in which they have disappeared, he adds: "When an exostosis is hard, chronic, and free from pain and alteration of the structure of the bone, it is a much more common thing for it to cease to enlarge, and remain stationary during life, without producing inconvenience, provided it be so situated as not to impede the functions of any vital organ." (Dict. of Pract. Surg., N. York, 1842, p. 363.) Again (at p. 364) he states that "when exostoses merely occasion a deformity, and no pain nor inconvenience from the pressure which they produce on the neighbouring parts, it is certainly most advisable not to undertake any operation for their removal;" but adds (p. 365) "*when exostoses are productive of much pain, and injure the health, and their situation*

admits of their being safely removed with the aid of suitable saws, or even with that of a gouge and mallet, the operation may be undertaken."

Richerand's views are very similar to those of S. Cooper:—
"L'ablation de la tumeur est le seul moyen efficace dans les éxostoses bornées et locales; mais l'opération est très douloureuse, et le mal est bien préférable à un aussi cruel remède. Néanmoins si la tumeur comprimoit quelque organe, ou causoit une difformité dont le malade voulut être guéri à tout prix, voici quel seroit le procédé dont on devroit faire usage." (Nosog. Chir., Paris, 1815, tom. 3., p. 133.)

According to Chelius, (System of Surgery, by J. M. Chelius, translated by J. F. South, Philadelphia, 1847,) "The dispersion of a true *exostosis* is never to be expected from the various dispersing remedies which have been recommended." "When these remedies have actually dispersed bony swellings, they were doubtless only inflammatory swellings of the *periosteum*." v. 3, p. 411.

"If the exostosis be void of pain, if it do not enlarge, nor inconvenience the patient, or if situated on any part where mechanical treatment is improper, it is advisable to let it alone. *But if the swelling be very inconvenient to the patient, and if its situation admit of mechanical treatment, the only remedy is its removal.*" p. 412.

Velpéau, (Oper. Surg., translated by Townsend & Mott, N. York, 1847,) in a special chapter devoted to exostoses, says, "the lower third of the femur is perhaps the region of the osseous system where pediculated exostoses acquire the greatest volume, and are most frequently met with," and adds, "this is a kind of exostosis which is scarcely mentioned by authors." He then states that he had only seen six or seven cases of it, in neither of which did the patient experience much inconvenience, nor therefore require any operation. After speaking of the treatment, he adds: "The conclusion, therefore, in my own mind is, that I would not decide upon the removal of tumors of this description, *unless, notwithstanding my representations, the patients should find themselves so much incommoded or annoyed as to make an urgent demand for relief, or unless such tumors should threaten to acquire too large a volume, or to*

undergo degeneration, or cause, in fine, actual pain, or serious functional derangement in the part. As for the rest, (*in short?*) there is no other treatment for them but extirpation." (p. 141.)

Velpéau advises the use of a tent to give issue to pus, &c.

Miller (Principles of Surgery, Philada., 1852,) observes that "This kind of exostosis seldom occurs but in the long bones of the extremities; and is most frequent in the femur at its lowest part." (p. 449, with diagram.)

"In the majority of cases, this variety of exostosis may be left undisturbed. Interference is only warrantable, *when bulk and position are such as to interfere with important functions*—as of muscles, vessels, cavities, canals, or internal organs: then an incision may be made, the neck of the growth severed by a saw, or bone-pliers, and the exostosis carefully removed. Cases demanding such treatment, however, are comparatively rare." Miller urges the importance of leaving the wound open (p. 450)—cites no case of operation by himself nor any one else.

Nelaton (Pathol. Chir., Paris, 1847,) recommends excision, under circumstances similar to those mentioned by the above authorities. On looking over the works at hand, I find, as above remarked, that very few speak from personal observation of the form of Exostosis under consideration—and the number of reported cases in which excision has been practised is so small that I feel disposed to append such as I have found to this narrative.

Sir Astley Cooper relates the two following cases:

"CASE.—Miss E. O., 11 years of age, was brought to my house in July, 1817, by Mr. Prior, Surgeon of Clapham, on account of having an exostosis on the thigh-bone: it was seated a little above the inner condyle of the os femoris in the line of the insertion of the triceps muscle. The account she gave me was, that it was accidentally discovered about eight months before she applied to me. At first, it did not interfere with her daily exercises, and it produced no pain on walking; but from the month of May last she found some difficulty in bending the limb. Mr. Mortimer, Surgeon, of Bristol, wrote to me, that he was requested to examine Miss E. O.'s thigh in the beginning of the year, and found a small tumor about two inches above the inner condyle of the femur, directly under the seat of the garter. This tumor was evidently an osseous enlargement, insensible to pressure, and by no means painful. The skin was

perfectly free from inflammation, and there was no reason to suppose that the tumor had acquired any addition to its bulk for years. Its origin was unknown; it occasioned no pain or inconvenience whatever, nor did it in any way impede her walking or dancing, or produce any degree of lameness. Mr. Mortimer's view of the case was perfectly correct. In the month of May last she first found that on going up stairs, she was under the necessity of advancing her right foot on each stair; and the same in descending, when she was obliged to proceed sideways, being unable without great inconvenience to bend the limb. Whilst sitting down, and more especially on a low seat, she felt considerable pain in bending the knee: and after having sat for some time, she experienced pain and difficulty on rising. Her lameness, just before her journey to London, had considerably increased, and her leg had become painful down to the heel. When she attempted to run, she felt a snap upon the swelling, as if a cord had slipped out of its pulley, which was owing to the tendon gliding over the projecting part of the bone.

"On Monday, the 21st of July, an operation was performed for the purpose of removing the swelling, in the presence of Mr. Prior and Mr. Plowman. An incision was made over the projecting portion of the bone, and the muscles drawn aside from its surface: the periosteum, which was much thicker than usual, was divided by the knife, and turned aside from the exostosis, which was covered by a bed of cartilage, in which bony matter had been deposited; and this was about the thickness of one-eighth of an inch. The exostosis was next cleared of the soft parts from the surface of the thigh-bone, upon which it grew, and a spatula was passed down to confine the muscles from interfering with the saw. The saw was then attempted to be used without the forceps, but it could not be well fixed: the forceps were therefore added to it, and the bone was sawn through. Some irregularity remained upon the thigh-bone, which I endeavored to remove by means of a saw recommended by Mr. Hey, but the muscles interfered with its employment, in consequence of the depth at which the bone was seated; and I removed it readily, by means of a pair of bone nippers,

"No vessel of any importance was wounded in this operation, nor was there any necessity for applying a ligature. The edges of the wound were approximated, and retained by means of adhesive plasters; and an evaporating lotion was applied.

"On the 22d she had some fever, which on the 23d had subsided.

"On the 24th she was carried to the sofa, after which she experienced no further inconvenience from the operation; and before leaving town had entirely lost the painful sensations which had previously existed."—(*Surg. Essays*, Philada. 1821.)

"CASE.—James Aris was admitted into Guy's Hospital, August 13, 1817, with an exostosis occupying from one to three inches of the thigh-bone, above its internal condyle, which felt, through the integuments and muscles, about the size of the finger, and which was directed rather upwards, not being exactly at right angles with the thigh-bone. His age was 24 years; and 14 years ago, while jumping over a post, he first discovered pain in the part which afterwards formed the seat of the disease. The pain lasted but a very short time, but it led him to examine particularly the part, when he perceived a small and hard swelling. This tumor gradually increased, and at length began to interfere with the motion of the limb, so as to render him anxious to have advice respecting it. When walking, he felt what he described as a snapping in the part, like a cord slipping from a pulley, which probably arose from the extension of the sartorius muscle, and its sudden slipping over the swelling. When he placed the limb quite straight, he found a difficulty in bending it; and when bent it was almost equally difficult to extend it; each flexion and extension producing a snapping noise, which could be distinctly heard.

"On considering the inconvenience which the swelling had produced, and that the disease was obviously on the increase, I recommended to him the operation which I had performed in the preceding Case, and advised him to become a patient in the Hospital, and to submit to one of a similar kind. He procured admission immediately.

"On the 22d of August I performed the operation. The man was lying upon a table with his thigh slightly bent, and I made an incision through the integuments over the swelling, and thus exposed the sartorius muscle, which appeared to have gained an increase in its breadth, and to be incapable of being sufficiently drawn aside to completely expose the tumor without considerable violence; I therefore made an incision through it in the direction of its fibres, sufficiently large to allow the exostosis to pass through the opening. The periosteum which covered the swelling, was then exposed, and being cut through, and turned aside, a surface of cartilage was laid bare, and beneath it a large process of bone. Mr. Machell, the inventor of the saw, who was present at the operation, himself applied it, and with a very few turns of it removed the osseous tumor. The edges of the wound were brought together, and union attempted by adhesion.

"In the evening of that day he had some symptoms of constitutional irritation, and some blood was taken from his arm. On the following day, he took a brisk purging medicine, and after this time no unpleasant symptoms appeared.

"My dresser, Mr. Humble, informed me that the wound was

as nearly as possible cicatrized on the 12th of September, and he was discharged from the Hospital a few days afterwards, and continues free from the inconveniences which he had experienced prior to the operation."—(*loc. cit.*, p. 160.)

The late Dr. George McClellan (*Prin. and Pract. of Surgery*, Philad., 1848, p. 345) thus expresses himself in reference to a case upon which he operated :

"It is by no means so often necessary to perform the operation for extirpating an exostosis as is generally supposed. By far the majority of cases become stationary after they have attained a moderate size, especially if the patient is kept on an antiphlogistic regimen, and attention be paid to his regular secretions and exhalations. Those who have been occasionally compelled to operate for the removal of exostoses from deep-seated positions, where they interfere with important motions, or functions of any kind, know too well the difficulties and embarrassments to resort unnecessarily to a repetition of such attempts. I have seen patients suffer more from the extirpation of prominent and troublesome ledges of bone from the linea aspera than from any other surgical procedure, and afterwards undergo more local and general irritation as well as fever. I think I never was so much perplexed and worried as in an effort which I once successfully made to cut away a long and flat prominence from the inner fork of the linea aspera of the right thigh in a muscular young man, whose comfort and business had been much interfered with by the growth of the tumor. A large hook of the excrescence extended upwards along the inner side of the thigh directly over the femoral artery and vein, where they passed the adductor tendons, and rendered it very difficult to avoid them. The collateral vessels were also very greatly enlarged, and bled furiously under every stroke of the knife. A small narrow saw, however, finally enabled me to cut away the whole bony mass, and the patient recovered with severe inflammation and fever."

Mr. Edward Stanley (*Diseases of the Bones*, Philad., 1849) establishes the following rule :

"After the removal of an exostosis, especially when deep-seated, it is not expedient to approximate the divided edges of the integuments, in the view of obtaining their adhesion ; since, in the exposure and removal of the tumor, so much injury is usually done to the surrounding cellular tissue, that suppuration through it will almost certainly ensue. The wound should be simply covered with folded damp linen ; and, by this means, the healing of it will be completed in a shorter time than if

the adhesion of its sides had been in the first instance attempted." (p. 137.)

He then relates two cases in which he performed excision.

CASE 1.—"A man, aged twenty-five, had an exostosis growing from the lower and front part of the femur, which commenced five years previously. The tumor, increasing, had of late interfered with the knee-joint, occasioning pain in it, and distension of the synovial membrane by fluid. The tumor was oblong, the long diameter of its base being in the axis of the femur, and measuring about four inches, the breadth of its base about two inches. The increase of the tumor, in such a direction that it projected into the knee-joint, induced the man to solicit its removal. Its connexions were carefully examined, with reference to the question of its being possible to remove it, without injuring the joint; and it was decided that the operation might be safely undertaken.

"An incision was made through the rectus and cruræus muscles to the base of the tumor, on the side most remote from the knee-joint. But in doing this, though with all possible caution, the joint was opened, and it was now discovered that the synovial membrane, in yielding to the accumulation of fluid in the joint, had extended upwards upon the tumor, and some way beyond it upon the front of the femur. With much difficulty, on account of the deep situation of the tumor, the breadth of its base, and the hardness of its texture, I effected its removal by means of a chisel and cutting forceps. The inflammatory fever, immediately consequent on the operation, was not severe; but as this subsided, there was no return to health. Constitutional derangement of another kind ensued; its prominent features were rigors, collapse, pain in the head, cramps in the limbs, and extreme prostration of nervous power; and it soon became evident, that suppuration had taken place through the subcutaneous cellular tissue of the whole limb, from the hip to the foot. At the end of the third week from the operation, the patient died." (p. 138, 139.)

CASE 2.—"Subsequently, in the hospital, I removed an increasing exostosis from the lower and inner part of the femur; but here there had been no accumulation of fluid within the knee-joint, and consequently no extension of the synovial membrane beyond its natural limits." (p. 140.)

M. Roux, the Nestor of French Surgery, remarked, in 1847, (*Medico-Chirurg. Review*, July, 1847, p. 263, quoted from *Revue Méd. Chirurg.*) that he had seen only seven cases of pediculated exostosis on the lower third of the femur. The

following are his conclusions with regard to excision of exostoses in general:

"1. With the exception of some whose position renders them inaccessible, it is almost always possible to remove these tumours. 2. In almost all cases such ablation is indicated, either for the removal of a great deformity, the relief of continual suffering, or the re-establishment of the deranged functions of certain organs. 3. In the great majority of cases we can proceed to the ablation without a preliminary exposure of the tumour, and without having any great difficulties to overcome. 4. Such an operation may lead to unfortunate results, in consequence of some peculiarity in the seat or relations of the tumour, or from the bad constitution of the patient; but generally it is crowned with success."—(*Revue Médico-Chirurgicale*, Nos. 2 and 3.)

In such operations M. Roux prefers making a lateral incision upon each side of the tumour, through which he passes the saw and divides the pedicle of the exostosis. We are disposed to think that by such a method the danger of purulent infiltration and the difficulty of the operation must be materially increased. M. Nélaton observes that "in a case of exostosis of the anterior and inferior portion of the femur operated upon by M. Roux, I saw the Professor compelled to unite the two lateral incisions by a third and transverse one, which extended through the skin, the rectus cruris and a portion of the triceps, so as to form two square flaps which he had to dissect up in order to disengage the tumour."*

I have looked over the extensive library of the Medical College of Georgia, and have been unable to find any notice of more than the six cases above cited, in which an exostosis of the femur has been excised. The disease is admitted to be of rare occurrence,† and the cases in which an operation may be

* "Je dois dire cependant qu'après avoir scié le pédicule, il est quelquefois très difficile d'isoler l'ostéocèle de toutes les parties qui l'entourent; dans un cas où M. Roux opérât une exostose placée à la partie antérieure et inférieure du fémur, j'ai vu ce professeur être obligé de réunir les deux incisions latérales par une troisième incision transversale qui dut comprendre la peau, le muscle crural antérieur et la partie inférieure du triceps crural, de manière à former deux lambeaux carrés qu'il dut disséquer pour dégager la tumeur."—(*Nélaton, Pathol. Chir.* Paris, 1847. T. 2, p. 20.)

† In the Museum of the Medical College of this city, there are two specimens of exostosis of the lower end of the femur among the collection of diseased bones. In both, the exostosis is a few inches above the inner condyle and upon the anterior aspect of the bone.—[Ed.]

come necessary must be exceedingly uncommon. Operations of the kind have doubtless been performed which were never reported; and this would most probably be the case with such as terminated unfavorably.

As to the result of the cases reported, we find that the first, by Sir A. Cooper, was successful; and we may infer that the wound healed by first intention, although this is not distinctly affirmed. In this case, the existence of the tumour had not been *known* more than eight months and the patient was only eleven years of age.

In Sir Astley Cooper's second case the patient was 24 years of age, and the tumour had existed fourteen years. The wound was upwards of twenty days in healing; but the result was successful.

The third case, alluded to by Prof. McClellan, recovered; but the details are not given, and we cannot therefore determine the length of time the wound required to heal.

The fourth case is reported by Mr. Stanley. The patient was 25 years of age, and the tumour of five years' standing. Death occurred at the end of the third week, apparently from purulent infection.

In the fifth case, Mr. Stanley merely adverts to the operation, but does not furnish us with the result. It was probably favorable.

The sixth case is that of M. Roux, referred to by M. Nélaton. The result is not stated. From the severity of the operation, as practiced in this instance, we may well doubt that the patient recovered.

If we add our own case to the list, we have seven in all, of which three were successful, two fatal, and the two others of uncertain result. The immediate cause of death in our case, was evidently infection induced by the rapid decomposition of the tissues that had been so long exposed to contusion, and which were found lacerated and filled with black blood and grumous matter. Notwithstanding the free issue which had been provided for the offensive matter, its copious discharge, and ther repeated cleansing to which the wound was subjected, the poisonous absorption took place rapidly and carried off the patient.

PART II.

Eclectic Department.

Hemorrhage in Operations Performed with the aid of Chloroform. By KIRTLEY RYLAND, M. D., of St. Louis.

In the last number of the St. Louis Medical and Surgical Journal, there is an article by S. Wolff, M. R. C. S. L., in reply to one published by me, in a previous number of the Journal. As the article of Mr. Wolff places me in a false light, in more respects than one, I feel myself called upon to reply to it.

Mr. Wolff has, in the beginning of his article, taken a wrong view of the intention of mine. One would infer from his preliminary remarks, that I had commenced a crusade against the use of chloroform, which he felt it his duty to defeat; because, as he says, the patients might (some of them at least) meet with my damnable heresies, and refuse to take chloroform when it was desirable they should, and thereby greatly annoy the surgeon. But as I stated at the time, my only object was to call the attention of the profession to the circumstance, that chloroform might cause hemorrhage, and ask for the experience of the older medical men concerning it.

Mr. Wolff thinks I should not attribute the hemorrhage, I reported to chloroform, "for," says he, "it would be but reasonable to suppose that hemorrhage would continue if the surgeon occupied himself with ligating small arteries and overlooked two or three large ones with their mouths gaping open," and he might have had manliness enough to have quoted entire, and said, "with no blood issuing from them." He says, he cannot conceive what arteries they could have been. I firmly believe he cannot, and I am convinced he would have as much difficulty in conceiving of a standard of comparison or an arterial anomaly. The facts of this oversight which Mr. Wolff makes so much of, are these: I was taking up arteries myself at the time, and, on the tourniquet being loosened, I observed near the inner edge of the flap and close to the surface two or three arteries the size of a young chicken's quill; I watched them for a moment, and although their mouths were uncontracted, they did not bleed, I placed ligatures upon them and went on. This occurred early in the operation, and yet Mr. Wolff thinks it sufficient to account for the hemorrhage, and still these stray arteries did not bleed; and if they had bled, how that could affect the small arteries and capillaries and make them bleed I cannot see.

Mr. Wolff next takes hold of the supposition I advanced to account for the hemorrhage. He says, the theory will not stand investigation, but does not prove it, thinking, as he appears to do throughout his article, that his bare assertion will be sufficient to convince all. He says, there is a more satisfactory way to account for the hemorrhage than by the theory I advanced, and proceeding to show what it is, he says, as confidently, as if stating a fact which had come to his own knowledge: "there had been extensive chronic inflammation in the leg, and where there is chronic inflammation, the hemorrhage is increased." Mr. Wolff appears to have forgotten his previous brilliant and satisfactory solution of the question, or peradventure he wished to make assurance doubly sure, and therefore brought in this one, which unfortunately enough fails in one particular—the fact upon which it rests is no fact at all. The injury had been received in the foot, and on account of the inflammation above the ankle joint, Dr. Wood amputated the leg just below the knee, in order that there might be no danger from organic mischief. The rapid and satisfactory recovery of the patient, proved his judgment to be correct. And if there had been chronic inflammation, the argument amounts to nothing; for, like the first, it fails to apply to the other case. It is a poor and pitiful argument that will not reach around the subject.

Mr. Wolff next proceeds to give his experience in chloroform, which he does in the following remarkable sentence;

"Sir George Ballingall reports a case of amputation where twenty ligatures were necessary, occupying some twenty-five minutes. Fergusson says, that two or three ligatures, more or less, are not going to interfere with the result; and for myself, I can only state that, since I have lived here, now nearly three years, I had used *chloroform* very extensively, and always with good results." Now, what connection has Sir G. Ballingall's ligatures with Mr. Wolff's experience in chloroform? Perhaps he merely wished to quote some author, (every one knows how popular foreign quotations are latterly, they look learned,) and seeing no other crack at which they could be introduced, dragged them in where they certainly appear most wofully out of place. Mr. Wolff appears to think this experience of his quite enough to settle the affair forever, but I imagine that Dr. Wood's experience upon the same subject, (mentioned in my former article,) will have quite as *much* weight in the part of the State where they both live as Mr. Wolff's. The concluding paragraph of Mr. Wolff's communication begins thus: Dr. R. states, that he has never met with any account of this increased hemorrhage in any author, and

I am not surprised at it, &c. Now, how Mr. Wolff could have made such a statement as the above I cannot see; for I distinctly mentioned a case, reported by Dr. J. B. Porter, U. S. A., where ether had been given in an amputation, and the hemorrhage was violent and almost uncontrollable.

Lastly, in reply to Mr. Wolff's opinion, that if I continue my investigations, I will soon find reason to change my opinion, I have only to reply, that I *intend* to pursue them; and if he or any one else convinces me that I am wrong, I shall certainly acknowledge it and thank him besides. With another case which has come under my observation, and a few remarks, I will close.

In March, 1853, Prof. C. A. Pope amputated the middle finger of Richard Genery, a stout, healthy Irishman, aged twenty-eight. The patient took chloroform, which I administered. A large quantity was necessary. After the operation Dr. P. left the hospital, directing me to stop the bleeding and dress the hand. It is found sufficient, ordinarily, to elevate the hand and apply cold water, which I did, applying pressure to the artery at the same time; but after a considerable time it was found insufficient, and I attempted to stop the bleeding by other means, and after placing ligatures upon three small arteries, twisting or performing torsion upon several others without yet checking it, I had a common field tourniquet applied to the artery, and pressure maintained, until the cause of the bleeding disappeared, which was two hours or more. Dr. Stone, of Knoxville, Mo., assisted me greatly. A few days ago, Prof. Pope amputated the finger of a man at the hospital, which was affected precisely like the one in the foregoing case. No chloroform was used, and no hemorrhage took place.

In the London Med. Gazette, for Sept., 1850, is an article from the Boston Med. Journal, giving an account of a case where ether was used in an operation on the face near the mouth, and the hemorrhage was so considerable, that the actual cautery had to be applied, which set fire to the ether vapor.

Since I wrote my first article, I have ascertained some facts which enable me to account more satisfactorily for these bleedings than I had done—and at the same time, show that chloroform probably caused them. M. De Chaumont, in a paper, read before the Edinburgh Physiological Society, says: "I have found that when blood is agitated with chloroform it does not coagulate." *Secondly*: In the London Medical Gazette, 1850, it is stated that chloroform enters the circulation in full integrity and without any resolution of its component parts. The facts taken in connection go to show, that when a large quantity of chloroform is taken into the circulation and

the elimination from any cause, retarded hemorrhage would probably ensue. Moreover, the blood is nearly always found fluid in patients, poisoned by chloroform, which favors the idea somewhat.

I propose to repeat the experiments of M. De Chaumont upon the various ethers, the results of which, together with others upon the cause of death from anæsthetics, I hope soon to be able to lay before the profession.—[*St. Louis Med. and Surg. Journal.*

Effects of Chloroform on the Blood. MR. DE CHAUMONT read before the Edinburgh Physiological Society the following communication :

Much controversy has arisen as to the real nature of the effect of chloroform on the blood. Amussat, Sedillot, etc., have contended that the blood is darkened by it, and that the effect is quite analogous to asphyxia. Gruby, on the other hand, contends that not only is the arterial blood not darkened but that the venous blood is even rendered as bright as the arterial. I have found that when blood is agitated with chloroform it does not coagulate, but becomes quite transparent ; and when the blood is venous the color is rendered as brilliant as that of arterial blood. Sulphuric ether, on the contrary, does not effect the color of venous blood. Chloroform seems thus to have a special effect of brightning colors, not only in this but in other cases ; as, for instance, when it dissolves iodine, the solution has not the deep-brown color obtained when water, alcohol, etc., are used, but has the beautiful violet hue of the vapor. In some cases chloroform restores the color of old blood-stains on linen, etc., but the effect is not constant. When blood, acted upon by that agent, is examined by means of the microscope, the red globules are found completely dissolved, the field presenting only a homogeneous colored expanse, in which the white globules float unaffected. Chloroform acts on pus very similarly to acetic acid, as it brings into view the double and tripartite nuclei of the globules, although the effect is not so speedily accomplished as by acetic acid. When the blood of leucocythemia is acted upon, the red globules dissolve, and the white present the reaction not of the ordinary white globules of the blood but of pus. With the view of observing the effects of the administration of chloroform on the blood in the living body, I attempted to anæsthetize a rabbit by the rectum, so as not to interfere with the respiration. Although two drachms were injected, no other effect was produced ex-

cept an intoxicated appearance, which did not give immunity from pain. This corresponds with what was observed by Flourens in the early days of etherization. Another rabbit was anæsthetized by inhalation in the ordinary way, and died suddenly during the operation. On opening the body the lungs were found florid, and without trace of asphyxia; the auricles of the heart continued to contract for an hour after death; the peristaltic action of the intestines and bladder continued also with tolerable vigor. Venous blood from this animal presented the same reactions as before mentioned. The next experiment was upon mice. Four of these animals were placed alive in a wide-mouthed bottle, and a few drops of chloroform poured into the bottom of the vessel. This speedily caused a great excitement among them, and they soon after died. The examination of their bodies showed the same signs as in the rabbit. The brain was also normal. Another rabbit was anæsthetized by the lungs, and the crural vessels of the right leg exposed and carefully watched during the whole process of inhalation, but without there being any evidence of change in the color of the venous or the arterial blood. In this animal also an attempt was afterwards made to induce insensibility by the rectum, but without success. The chloroform was in this case mixed with water, and the intoxicating effect produced was greater and continued longer than in the former. The temperature was also apparently lowered, although no exact observation by the thermometer was made. Dumeril and Dumarquay have stated that the temperature is lowered in anæsthesia, and under the influence of ether and alcohol, distinguishing the effects of these agents from common narcotism, as by laudanum, in which the temperature is raised. This last rabbit died after a few hours, and the *post mortem* appearances showed that death had been occasioned by acute peritonitis. Here, too, there were no signs of asphyxia, the lungs being of a bright rose color. In the liver was found a cyst, containing a white putty-like mass, which, under the microscope, showed beautiful oval-shaped cells containing a round nucleus, with several, generally three, oval nucleoli. These, Dr. Bennett informed me, were entozoa. The action of chloroform on them was curious; the nucleus swelled up so as to fill the cell, while in another part of the field were seen cells floating empty, and nucleoli floating about in threes, joined by filaments. Hence, it seemed as if the nucleus had alone been dissolved, a curious fact when taken in connection with the solution of the red globules of the blood, these being also considered as nuclei.

The above experiments were all subsequently confirmed by

a committee of the Royal Medical Society, by whom it was also observed that chloroform dropped upon the intestines and bladder caused violent peristaltic action, and that dropped upon the heart, while that organ was contracting, it caused a cessation of the pulsations until it had all evaporated, having a temporary paralyzing effect on that organ.

From the above experiments I am inclined to think that chloroform does not act by asphyxia, and that, when this does occur, it results from the manner of the administration; that its action is directly upon the nervous centres, first upon that part connected with the perception of sensation, and lastly on the spinal cord, or generator of motive force; that although it arterializes the color of the venous blood out of the body, I have not yet been able to observe the same effect in the body; and that on the other hand, the arterial blood is in no way affected in color by its inhalation.

Dr. Bennett referred to a report on the action of chloroform drawn up by himself, and printed in the *Transactions* of the Edinburgh Medico-Chirurgical Society about the time when chloroform was introduced as an anæsthetic. (Monthly Journal, Jan., 1848.) The results of the experiments there detailed were similar in some respects to those of Mr. De Chaumont.

Dr. Mathews Duncan remarked, that rabbits were not good subjects for experiments with chloroform, on account of the rapidity with which fatal effects succeeded the anæsthesia. From recent trials, he was convinced that chloroform introduced into the stomach could produce anæsthetic effects.

[*Monthly Jour. of Med. Science.*

Iodide of Potassium as an Antidote to Mercury. By J. W. CORSON, M. D.

In common, doubtless with many others, I have for years been in the habit, especially in scrofulous constitutions, of always terminating a mercurial course by the protracted administration of the iodide of potassium, both as a remedy for the immediate effects of mercury, and as a prophylactic against its future injurious consequences. And I have gradually ventured to be far less apprehensive of the ill effects of the mercurials in cachectic habits when judiciously managed, than formerly.

These views came as quiet practical teachings from the bedside. But the peculiar mode of action by which the iodide of potassium neutralized the slow poison of mercury in the system, was to me, as doubtless to others, a mystery, till the recent clear and satisfactory experiments of M. Melsens, of Paris,

explained the whole matter. The observations of M. Melsens were first embodied in a remarkably interesting paper, published not long since in the *Annales de Chimie et de Physique*, and recently translated by Dr. Budd, of Bristol, and republished in the *British and Foreign Medico-Chirurgical Review* for January, 1853. Those who are not already familiar with it we would refer to the original paper as a master-piece of medical observation, strengthening the patient deductions from the experience of the bedside, by the most rigid chemical analysis.

In the hope of directing more attention to these researches and somewhat confirming them, we have selected the above cases out of many others that might have been quoted. Some of them have the advantage of having been watched for several years, during which time they have exhibited no wandering pains, or undue sensitiveness to cold, and no cachectic appearances or other signs of injury from the constitutional effects of mercury. Four out of five cases had legitimate evidence of scrofulous or tuberculous taints and were therefore, specially liable to injury, and better proofs of the efficacy of the antidote. In the single exception, too, there was a chronic renal affection, and it is well known that in Bright's disease, and other chronic difficulties of the kidney, mercury is particularly apt to act at times with fearful violence. The suffering in this case from pytalism, was most severe of all, and the prostration amounted almost to that of mercurial erethism.

Yet with these constitutional tendencies to hinder us in the administration, they all belong to that desperate class of cases in which, from change of structure, or effusion of lymph or serum, life itself was immediately jeopardized, and apparently could only be saved by the resolvent effects of mercury. The simple enumeration of the cases would tell the practical physician that when menacing life, he had, in duty to his patient, no other choice. They consist it will be recollected, of extensive dropsy with hepatic congestion, threatening obstructive inflammation of the throat, immense pleuritic effusion, suffocative laryngitis, and severe puerperal peritonitis. In all these cases mercury appeared the *only remedy* capable of meeting the exigency. It succeeded most happily; and through the singular, and as we firmly believe efficient counteracting agency of the iodide of potassium, in a class of subjects most liable to injury, it left no sting behind. How the iodide of potassium acts in thus neutralizing the slow poison of mercury, the varied experiments of M. Melsens seems clearly to explain.

He lays it down as an admitted principle, proved by the well-known fact, that years afterw'ard, persons who have once freely taken mercury find gold coins discolored by the mercu-

rial in the perspiration of their bodies, and that mercury has been sometimes detected in the body after death; *that mercury as well as lead combines with the animal tissues*, and remains so to speak, fixed in the system for years. Secondly, that in the body as well as out of the body, the iodide of potassium acts as a *powerful solvent to the compounds of mercury and lead; disengages them readily from the animal tissues*, and drains them off through the kidneys. Many ingenious chemical and clinical experiments are given to establish these propositions. M. Melsens first proved that the iodide of potassium passes off principally in the urine, by taking large quantities himself, and then analyzing the different secretions of the body. The *feces* contained scarcely a trace, while the urine was loaded with it. It passes off through the kidneys with great rapidity. A person took 77 grains of the iodide of potassium, and in a few minutes the urine was charged with it. The compounds of mercury and lead, with the iodide of potassium, pass off by the kidneys in the same way.

An extraordinary cure of a looking-glass maker with severe mercurial paralysis, is given, in which the patient took the iodide of potassium in very large doses for several months, and repeatedly during this period the iodide of mercury was detected in the urine by chemical tests.

The great efficacy of the iodide of potassium as an antidote to the slow poison of lead and mercury, were proved by M. Melsens, by experiments upon several dogs, which were fed with the carbonate and sulphate of lead till paralyzed, emaciated, and nearly dead, and then in a short time restored to health and flesh by the administration of the iodide of potassium.

Three cases of severe lead paralysis among house-painters and workers in lead, were entirely cured, and a fourth greatly relieved by the same remedy. In five cases of mercurial paralysis and severe suffering among gilders and workers in quicksilver, the iodide of potassium in a few weeks, accomplished great relief, or a perfect cure.

It happened in some cases that the poison seemed to be liberated so rapidly by the remedy that it was badly borne. Sometimes profuse resalivation was the consequence. I had under my own care a gilder, aged 65, a few weeks since, suffering from mercurial tremors and paralysis, in whom eight grains of the iodide of potassium, three times a day, produced distressing ptyalism, and so added to his sufferings that he refused to continue the remedy.

Does the iodide of potassium ever salivate, except by *liberating* mercury?

We believe not. In the few cases in which salivation oc-

curs from preparations of iodide, we think it will always be discovered that mercurials have at some previous time, in the patient's life, been taken.

From the experiments of M. Melsens, it appears that the iodide of potassium when taken with mercurials, sometimes acts as a preventive to injury from the latter. We have for years been in the habit in strumous, syphilitic cases, of giving blue pill at night, and the iodide of potassium by day. Or we have *neutralized*, as we imagined, the too severe effects of the prot. iodide of mercury, syphilitic and scrofulous throat affections, by combining with it the iodide of potassium. We have very lately witnessed excellent effects from this combination, in a case of tubercular syphilitic eruption.

Might not the exhibition of the iodide of potassium, in seasons of special exposure, be a protection to painters and workmen in lead, against lead colic, and paralysis?

It is curious to notice what immense quantities of the iodide of potassium may be often safely borne. M. Melsens took himself for two months, from half a drachm to a drachm and a half per day, or more than two thousand grains in the whole period without any inconvenience, except temporary coryza, and a few pimples, and with a decided increase of appetite. One of the most severe cases of mercurial paralysis, related as cured by him, took 2,314 grains of the iodide of potassium, between the 21st of March and the 23d of June.

Of the cases of our own, one took five grains, three times a day, for eleven months, with the greatest benefit. M. Melsens recommends, in cases of mercurial, or lead poisoning, to begin with fifteen grains in solution, three times a day, and to increase the dose as the patient will bear it.

Dr. Budd thinks such large doses require two conditions—*First*, to be given on an empty stomach; and *Secondly*, in a state of large dilution.

The cases we have narrated above seem to prove, that in milder forms, where mercurial paralysis is not induced, and the system is not highly charged with the noxious mineral; smaller doses of the iodide of potassium, if continued sufficiently long, are highly efficacious.

In conclusion, these researches of M. Melsens explain, we think, why the iodide of potassium is so serviceable in certain broken-down syphilitic patients, in whom the quantity of mercurials previously taken, finally form an important element in their disease. We have not sufficiently tested the iodide of potassium in lead disease, to speak as yet, with confidence from personal reference. We may, however, as a supplement to this paper, at a future period, report a few cases of lead paraly-

sis, now under treatment, and hope in the mean time, to promote the principal object of this paper by eliciting from the profession further observations on this important subject, and exciting a deeper interest in the original memoir, from which we have so freely quoted.—[*New York Jour. of Medicine.*

Chloroform.

The introduction of anæsthetic agents to alleviate the sufferings attendant upon surgical operations and child-birth, was hailed as a discovery which not only robbed the knife of its terror, but also so far thwarted the operation of the primitive curse, as to cause certain clerical grandmas to enter their protest against its heaven-defying use. Experience, however, has proved that the rapture of the surgeon, and the pious fear of his clerical brother, are alike premature; that the use of the most popular as well as powerful of the several anæsthetic agents is calculated to awaken no little anxiety on the part of the cautious physician, and fear on the part of his patient; and while our profession must feel that their hopes and expectations are not yet fully realized, our clerical brethren may rest assured that their wives will continue to bring forth in sorrow, for though the pangs attendant may be relieved, the chances of a sudden exit will serve to keep in force the spirit of the original penalty of mother Eve's transgression.

By the use of chloroform, we are occasionally reminded that Death's advent may most emphatically be like a thief in the night, when least anticipated. The officers of one of the London hospitals boasted of having exhibited it in over nine thousand cases, without a single death; and, as if in mockery the grim messenger claimed its own in almost their next attempt at its exhibition.

The mode of death, too, is almost as diverse as the cases are numerous, hardly any two presenting exactly similar symptoms. This observation will be corroborated by consulting the table contained in the May No. of the *New-York Journal of Medicine and Collateral Sciences* for the present year. At times death is instantaneous, at others, as in Dr. Warren's case, in the Massachusetts General Hospital, it occurred after a lapse of three hours. Sometimes the respiratory movements continue after the heart has ceased to pulsate, and at others, we observe as one of its earliest manifestations, a paralyzing of the muscles of respiration. We once gave it to a parturient woman who strikingly verified this observation. The use of the agent was suspended, the patient revived, and again upon its renewal the same symptoms appeared. Several unavailing

attempts were thus made in this case, and its use finally abandoned. In another case, one of amputation of the thigh, its exhibition was attended with the same result; here we persisted in our efforts for an hour, without avail till at last, our knife acting as a stimulus to the respiratory motions, the chloroform was freely given and its full and happy effect obtained.

The amount too required to produce death varies; sometimes drachms, at others drops only. Twenty-five drops have proved fatal. (See table above alluded to.) One of our colleagues (Professor Denton) was about giving it to a nervous patient previous to laying open a felon on one of her fingers. To accustom her to the odor, a very few drops were put upon a handkerchief and handed her to smell of; he had hardly turned around, when he looked at her again and saw that her face was blanched, her eyes set, her muscles rigid, and respiration ceased. This case would undoubtedly have proved fatal, had not the Dr's admirable coolness prompted the application of a powerful stimulus. A scalpel, by no means distinguished for the keenness of its edge, was drawn slowly and firmly over the inflamed finger, cutting its way down to the bone. Just as the incision was completed the patient screamed, respiration was thus restored, and the patient and her friends considered it a beautiful exhibition of the agent in question; and should this patient ever have occasion to have a pustule picked, she will probably insist upon taking chloroform.

We do not indulge in this train of thought from any ill effects we have ever experienced; on the contrary, no accident has ever occurred with us.—[*Peninsular Jour. of Med.*]

Inhalation of Chloroform in Pneumonia.

The late journals of Germany publish more than 200 cases of pneumonia treated by inhalations of chloroform. Far from being contradicted in pulmonary phlegmasia, as had been thought up to the present time, chloroform on the contrary would seem, according to these facts, to modify favorably the inflammatory process of the lung. From among the observations published, out of 193 cases treated by Drs. Wachner, Baumgartner and Schmit, only nine died. Of twenty-three cases reported by Dr. Wawentrapp, of Frankfort, nineteen were treated exclusively by chloroform, and only one died. Every two or three hours the patient is made to inhale the vapor from fifty drops of chloroform, during ten or fifteen minutes, so as never to let the effects reach a loss of consciousness. All the patients were of adult age, and the disease upon an average had reached the fifth day. In every case it was

observed that the chloroform had a diaphoretic effect, which was sometimes produced by the first inhalation, and never failed to manifest itself on the third or fourth day. It gradually diminished the local pain, and caused it to disappear; it calmed the thoracic anxiety, brought back the respiration to its normal type, always appeased the cough, facilitated the expectoration in rendering it less abundant; and lastly it reduced the febrile reaction and induced a refreshing sleep three or four days after the inhalations were commenced.—[*Gazette des Hopitaux. Western Journal of Medicine and Surgery.*]

Upon the Difference of Temperature between Venous and Arterial Blood. By G. V. LIEBIG. (Inaug. Diss. Giessen., 1853.)

Former investigations upon this subject have led to conflicting results. Most of the older observers, such as Haller, Crawford, Kramer, Seudamore, &c., having declared that the arterial blood (in the left cavities of the heart) is warmer than the venous blood of the right cavities by 1° ; while A. Cooper, Coleman, Mayer, Autenrieth, etc., either pronounce the temperature of both kinds of blood equal, or give to the venous blood an excess of temperature over the arterial of 0.5° . The author proceeds, in his attempt to explain these differences, to inquire, whether the higher temperature be found in the blood flowing from the lungs, or in that from the capillaries, and he reverts to some of his former observations upon "muscular respiration."

The experiments were conducted both upon living and recently killed animals. That death might be caused quickly, and without loss of blood, the medulla oblongata was severed in some instances, and narcotin was administered in others. The thermometer was introduced, either by the jugular vein or the carotid artery, to the requisite depth to the heart; or it was inserted into the crural vein, or the vena cava abdominalis. Respiration was prolonged after death, when necessary. The experiments were performed upon dogs, and with every possible precaution to avoid errors.

The results were—1. That the temperature of the blood in the right cavities of the heart exceeds that of the left cavities by 0.05° — 0.16° ; 2. That the blood flowing into the heart by the vena cava descendens (probably because the blood which the former vessel contains comes from parts which present to the atmosphere a very great extent of surface in which a process of cooling takes place).

In the venous system, there go on changes of temperature corresponding with the respiratory act. In the vena cava su-

perior it was remarked that at the end of each inspiration the temperature of the blood rose; between inspiration and expiration it attained its maximum; towards the end of expiration it fell; and was at its lowest point after expiration. When animals breathe, or rather expire, shortly and quickly, and then inspire deeply, the variations of temperature are greater than usual. When the thermometer is in the right auricle, the highest degree corresponds with inspiration, and the lowest with the beginning of expiration. When the breathing is very short and hurried, these changes of temperature become diminished, or even cease altogether. In the vena cava abdominalis the same phenomena were not observed; in the vena iliaca they were reversed, the maximum occurring after expiration, and the minimum after inspiration.

The author then remarks upon the mechanical influence exerted by the respiratory movements upon the circulation in veins. The enlargement of the thoracic, and the diminution of the abdominal cavities, coincident with inspiration, cause the blood of the vena cava ascendens to flow into the right auricle, during and especially at the end of inspiration. The contents of the vena cava descendens are less than those of the vena cava ascendens, and they are cleared out at the beginning of inspiration. During expiration, the abdominal cavity is widened, and the vena cava abdominalis gains both in space and contents. Towards the end of inspiration, the auricle will therefore be filled by the warm blood of the vena cava abdominalis; and at this moment is observed the highest degree of variation in the heart's temperature. In expiration the blood streams into the auricle chiefly from the vena cava descendens, and then is remarked the minimum of temperature. As regards differences of temperature in different parts of the same system, the author found the blood of the vena cava sup. 0.16° , and that of the auricle 0.20° . Between the auricle, where the blood from two vessels of different temperature is not completely mixed, and the ventricle, the difference amounted to $0.1-30.20^{\circ}$.

In the arterial system there are no variations dependent upon the movements of respiration, or they are very slight, amounting to $0.01-0.06^{\circ}$, and connected with the changes in the right cavities of the heart, and the cooling of the blood in its passage from the lungs.

In dead animals, the temperature of the blood of the right ventricle exceeds that of the left by 0.16° . In two experiments, however, the temperature was found the same in both cavities. The temperature of the blood of the vena cava abdominalis was once, upon opening the chest, found 0.72° higher than that

of the thoracic portion of the carotid during life. The same observations were repeated in several experiments. The blood of the vena cava abdominalis, which comes from the capillaries of the lower extremities, and of some of the abdominal viscera, is the warmest blood in the body, even in animals where the heart has been emptied by bleeding, and to this point the author attaches great importance.

It would be desirable to know the amount of cooling which the blood undergoes in its passage through the lungs; but the subject is fraught with difficulty. The fact, however, if established, is most important, inasmuch as the source of animal heat has been referred by many to the organs of respiration. As regards the warmth of the blood in the vena cava abdominalis, it must be remembered, that the vessel lies imbedded in soft, highly organized parts, where, under all circumstances, heat is retained for a longer time than in other parts of the body; and perhaps this fact will explain the cause of the warmth of blood in that vessel as satisfactorily as any theory of changes going on in the capillary system.—[*Schmidt's Jahrb.*, 1853. *London Lancet*.

On the Treatment of Ascites by Intra-peritoneal Injections.
By M. VELPEAU.

A question of great importance from its novelty and the limited number of statistical data which we as yet possess to test its validity, and cases to which it may be curatively applicable is the late treatment of ascites by intra-peritoneal injections of iodine. The use of ioduretted injections, first introduced into practice by M. Velpeau, in the treatment of hydrocele, has proved their utility in the removal of serous effusions, and their superiority over various injections; and seems on the eve of becoming generalized as a treatment applicable to all serous effusions of a chronic character. One would *à priori* think that the startling proposition to inject a fluid so irritant as the tincture of iodine into the cavity lined by so extensive and delicate a membrane as the abdominal, would never be tried; we would at once anticipate, as the direct and inevitable consequence, a general and fatal peritonitis. Yet since facts are beginning to multiply on this subject, although they do not prove the utter harmlessness of the procedure, they demonstrate the radical cure of cases wherein all other treatment had failed; objections, however plausible and urgent they may theoretically appear, must give way to experiments and facts. As a matter of course, this treatment cannot be applicable to all cases; as yet, statistical observation has not been sufficiently

extended to point out accurately those cases to which this mode of treatment might be specially applicable. Ascites being but a symptom various are the organic lesions of which it is the effect; diseases of the heart, especially of its right chambers, offering impediments to the ingress of venous blood; diseases of the liver, particularly those causing atrophy of its tissue, interfering with venous return, as cirrosis; diseases of the spleen, especially those of a miasmatic nature, the sequela of protracted intermittents, and renal diseases; where the dropsy is a consequence of cardiac and renal disease, it is of a general nature. I do not believe that ascites, the consequence of those organic lesions, can receive any benefit from ioduretted treatment, inasmuch as the cause is usually irremediable and permanent, and therefore so must be the effect. Yet there are unquestionably cases of ascites, which might be designated simple, as that resulting from general anemia; that the effect of a peritonitis, acute or subacute, where the inflammatory peritoneal action having subsided, the serous effusion yet remains, and that sometimes after all treatment for its relief had proved nugatory. In the former of these cases, where a well tried tonic treatment with chalybeates shall have failed associated with drastic and saline cathartics and diuretics, the injections of iodine may succeed, as also in ascites, the result of an acute or subacute peritonitis; here inflammatory action may have been completely subdued, and we have to do with its consequences, its effused products. In these cases, as well as in ascites, the result of prolonged and invincible anemia, it would seem in the generality of cases, that the pathological action which maintains the dropsy is merely the predominance of exhalation over absorption, there being probably no organic alteration persisting otherwise to account for its continuance; here I can see from the analogy of its action in hydrocele, that from ioduretted injections, a new action being originated in the peritoneal surface, an impetus is given to absorption, gradually the effused fluid observed, exhalation by the same rests in abeyance, and thus an equilibrium being established between exhalation and absorption a radical cure is effected. The opinion that the irritant influence of iodine necessarily produces, when injected into the abdominal cavity, an extensive and fatal peritonitis appears to be more apparent than real. In corroboration of the truth of this statement I herewith furnish you some interesting facts, taken from published cases, treated in Hotel Dieu, of Lyons, by M. Teissier, one of the attending physicians.

"I have practiced," says he, "up to the present time, the intra-peritoneal injection of iodine upon six female patients af-

fectured with ascites. Of this number, two have been cured; with the third, the result is yet uncertain, because the injection is recent; but there have been no accidents, and the consequences of the injection are to the present moment favorable; with two others, the operation has been *innocent* though *unsuccessful*, the ascites having been produced a short time after; with the sixth patient, symptoms of peritonitis rapidly followed, and the patient died in forty-eight hours. In this fatal case I had to do with a young woman, twenty-two years old, in the last stage of scrofulous cachexia, and whose case offered no chance of success by any other method. Thus: two patients cured, their ascites having resisted all ordinary means—purgatives, diuretics, vesicatories, mercurial ointment; one patient, result doubtful; one operation perfectly *innocent* but *unsuccessful*; one operation followed by death. I ought to say, in this last operation, it was impossible to make the injection run out after having been thrown in. The injection in these cases was composed of iodine, iodide of potassium, and water. It appears, then, evident to me that this operation is destined to render great service, but it will not always be innocent. MM. Gromier and Teissier."

The paragraph which concludes these statistical results of MM. Gromier and Teissier, in my estimation, imports the true appreciation which should be accorded to the use of these injections in cases of confirmed and invincible ascites. In the last case of M. Teissier the ascites may have been the consequence of a tubercular peritonitis, as he remarks that the patient was in the last stage of scrofulous cachexia, though he makes no mention of this lesion as being the dropsy. These injections of iodine have been successfully used in certain cases of ovarian dropsy, as the unilocular form, and have been advised, as you are aware, in chronic and invincible diseases of joints, particularly in chronic synovitis.—[Dr. E. F. SMITH, *European Correspondent of St. Louis Med. and Surg. Jour.*

Faults of Medical Writers.

[In the discourse by Dr. Samuel Jackson before the Philadelphia County Medical Society at its last annual meeting, we find the following remarks on a subject which deserves the attention of the profession generally—especially those who are in the habit, as all should be, of writing occasionally for the press.]

Let the young doctor do his very utmost in acquiring a habit of writing with *perspicuity, propriety and precision*. Let him

seek no other ornament, for medical language is, like Thompson's loveliness, when "unadorned, adorned the most." No merit will make amends for the want of perspicuity. I can show whole paragraphs in our American books which have no meaning whatever, being similar in this respect to those verbose letters that Queen Elizabeth used to write when she had pre determined to say nothing. Medical diction ought to use as few words as possible, thus going the shortest way to the end of a thought. An English writer on morbid poisons, wishing to describe the daily progress of the variolous pustule, uses the following verbosity: "You receive from a long distance, from Dublin or from Edinburgh, a lancet, on the point of which there is a little dry animal matter. This lancet has pricked the pustule of a patient suffering with smallpox, and the contents of the pustule have been suffered to dry on the lancet. Now with this lancet you make a single puncture in the arm of a healthy person, not previously defended by vaccination or otherwise, and what results?"

Now suppose this author, Dr. Simon, had wished to describe also, the effect of a rattlesnake's bite, he might have begun thus: You receive from a long distance, from Utah or California, a rattlesnake, which Linnæus calls *crotalus*, it may be the species *horridus* or *durisus*; this dreadful animal has a sacculus of poison at the root of each fang, and when he bites, these sacculi pour forth their deadly contents along a groove in each fang. Now you permit this animal to bite a horse, for an experiment, or perhaps it bites one of you, and what results? In this multiplication of useless verbiage, a great amount of time is wasted without any compensation.

In a celebrated medical journal, we have this circuitous way of saying that a certain medicine was probably useful in rheumatism; the disease was cured in eleven days; "and lemon juice, if it was not the principal remedy, certainly exerted an important influence toward the production of that end." What think you, gentlemen, of *producing* or *leading forward* an end or a cure? One might suppose that the writer was a cobbler, and that he was talking about the *producing* or the *pulling forward* of his waxed-end. And then he has lemon-juice *making an exertion*, and *exerting an influence*.

Why should a writer say, "I had recourse to a medicine," if he had not previously used it in the same disease? This word means a running backward. The simple English word to *give*, is often supplanted by the Latin word to *exhibit*; that is, to make a show of the medicine. A shopkeeper *exhibits* his goods, a physician *gives* or *orders* his medicine. Celsus took nearly all his ideas from the Greeks, but he did not copy their

words. I believe he never uses the word *exhibere*, but *dare et uti*. Sometimes he says *adhibere*, but this does not mean *to make a show*; moreover, it is pure Latin. His own language was sufficient for him, except in the mere naming of diseases; and hence one reason that his style and manner are universally approved.

It is of no little importance that our young author should not practise the coining of words. A new idea may require a new word, but old ideas will always be most intelligibly introduced by known terms; hence the great English lexicographer, whose head might well be fancied as swarming with words, introduced only four in all his writings. His rule was, "to admit only such as may supply real deficiencies, such as are readily adopted by the genius of our tongue, and incorporate easily with our native idiom." If a little license be granted, how will you define its limits? How will you definitely measure the old vulgar phrase *too much*? A little liberty will prove like moderate drinking, and lead to intemperance. If every writer of the present times should coin words at his pleasure, and the next generation should adopt them and add to them, what odious gibberish would then fill the air! It is told of Sir John Mandeville that, when far in northern Asia, with his retinue, their words were all frozen before they could be heard, and that, on coming south, they were suddenly thawed, and filled the air with their liberated voices. I can hardly credit this fact, as the amiable author does not relate it himself, and yet something similar may happen to the jargon of the present generation; while confined to books it may pass without much notice, but our successors may find the accumulated vocabulary to become a clattering of unmeaning voices, the mere echoes of our vanity, and as unintelligible as Sir John's thawed vocables.

In the Transactions of the American Medical Association you may find some animating specimens of these important additions to our deficient language. *Numerism, socialism, sensationalism, subjectivity, progressionist, therapeutication, truths eliminated, annexes of the heart*. A writer in vol. iv., p. 59, calls impressions "*intuitively-felt relations*," and then inquires, "Are not all the felt relations based on immediacy and intuition, and not on representational and transmitted impressions." Truly, if men in high places continue to pour forth such floods of impurity, men in low places may well complain; hence I have ventured to notice the subject; it pertains to *self-education*, which is our present topic.—[*Boston Medical and Surgical Journal*.

[Extract of a Letter from Paris, addressed to the Editor of
"The Stethoscope and Virginia Medical Gazette."]

Lithotomy per Rectum.

I witnessed, on the morning of the 25th of July, an interesting operation for stone in the bladder, by Prof. Nélaton of the Hôpital de la Faculté. The subject was a man, about 45 years of age. A calculus of considerable size (as large as a hen's egg) was extracted, together with several others of smaller volume, which M. Nélaton supposed to be *scales* from the larger stone, which had been cast off spontaneously some time before the operation. The circumstance of interest connected with this case is, that lithotomy was performed through the "dilated rectum." The urethra, into which a grooved director or sound had been previously passed, was punctured, and Depuytren's "double concealed lithotome" (lithotome caché,) introduced along the director, to the bladder. It was then opened and withdrawn, in the track by which it had entered, dividing the neck of the bladder and the prostate so as to make an incision continuous with the puncture that had already been practiced in the urethra, below or in front of the prostate gland. In this way an opening was created sufficiently large to admit of the extraction of the stone, the relative size of which I have given above. This was accordingly accomplished, as in ordinary cases, with the forceps. The smaller calculi, be it observed, M. Nélaton removed with great facility, simply by the introduction of the fingers, proving thereby the ease with which the bladder is reached "per rectum." No vessel of sufficient diameter to require the ligature was wounded in this operation. I shall refrain from any lengthy remarks upon this operation, leaving it to your readers to draw their own conclusions relative to its advantages. When we reflect, however, upon the extreme dilatability of the rectum; the ease with which the bladder may be attacked through it; the tenuity of the tissues necessarily incised; the comparatively little danger of wounding the urethral "bulb" or vessels sufficiently large to create anxiety in regard to hemorrhage; lastly, the ease with which stones of ordinary or even large diameter may be brought to light—we are naturally inclined to look favorably upon it. A "rectovesical" fistula will of course result; and the hope that it will close by primary intention, can scarcely be entertained, but it may be expected that the "granulating process" will cause its obliteration as much sooner than the vesico perineal fistula resulting from the bilateral operation in the perineum as the extent of surface implicated in the one case is less than in the other. Particularly may this be antici-

pated, if by proper injections and lavations, fœcal irritation be avoided. The introduction of this operation into practice belongs, I believe, to M. Maisonneuve, the bold surgeon of Hôpital Cochin. I say I believe, because I am not aware that it has been performed in the U. States or elsewhere, though the incision of the fundus of the bladder, where the stone, making for itself a vesical pouch, projects into the rectum, has, I am told, been often practiced. It is difficult to foretell the absolute termination of any step in operative medicine, especially if regions be concerned whose inflammatory accidents are to be dreaded. Yet, when we consider "appearances," (too often and particularly deceptive in medicine I know) and the anatomical simplicity of the incision requisite in this operation, we are led to believe it *less* replete with danger to life than any other method of lithotomy now employed. This, however, is for "experience" to prove. Permit me, in conclusion, to add, that the patient who submitted to this operation has, during the several subsequent days that have now elapsed, presented none but favorable symptoms.

Orchitis.

For the radical cure of this affection, M. Velpeau not unfrequently punctures the testicle with the lancet. In the several instances in which I have seen him employ this method, he did not apparently spare the true substance of the seminal gland, but passed the lancet freely and in different places through both the proper tunica-vaginalis and tunica-albuginea. In a clinical lesson given by him, he spoke of the rapidity with which cures of this sufficiently troublesome affection are sometimes accomplished by this means, and of the little danger resulting to the testes from wounds of that organ. This last fact has long been known here and in America, and any one may be convinced of the fact by giving that method of resolving an inflamed testes a trial. It undoubtedly acts benignantly, by discharging whatever serosity may be contained in the tunica vaginalis propria, and by relieving the tension of the tunica albuginea. This operation is productive of but little and momentaneous pain, which is followed by an amelioration of previously distressing symptoms. M. Velpeau, however, mentioned one case, where an aggravation of pain existed for several days after the punctures. This was, in all probability, due to a portion of the tubercular matter of the testicle forming a hernia in the small openings that had been punctured in the tunica albuginea by the lancet.

Cataract.

I have seen at the Hospital of the Faculty, several operations

upon cataractous eyes, involving a principle which, I confess was a novel one to me, and which I insert here, with the hope that it may interest those of your readers to whom it may also be *new*. These operations consisted neither in extraction, depression nor reclination, but in simply dividing the lenticular capsule so as to admit the free penetration of the aqueous humor to the crystalline lens, which is left in situ to undergo the changes that are impressed upon it by that fluid. These changes are interesting. At first it increases in volume. Small mushroom or fungous-like portions of it proceed from the incisions of the capsule. These finally separate and fall into the anterior chamber of the eye, where they are ultimately dissolved and disappear. It goes on in this manner until the whole lens "breaks up," is removed from the field of vision, and sight is restored. I need not say that some time is required for this dissolution and absorption of the lens, but it will as surely arrive as that the aqueous humor has the property of dissolving that body when exposed to its action. There is also but little danger of inflammation (so much to be dreaded in others) from this operation, which may be repeated, I might almost say, "*ad libitum*;" for every surgeon is aware of the comparative harmlessness of punctures of the cornea, and the rapidity with which the aqueous humor is reproduced after having been evacuated. Indeed, it would not be unwise to "draw off" this fluid several times, at proper intervals, during the process of absorption of the lens; for it exists in but very small quantity, and becomes rapidly saturated with the crystalline matter, and then of course refuses to dissolve a larger portion of it. It is scarcely necessary to add that this process is most applicable to lenticular cataracts, and that the rapidity of its success must in a great measure depend upon the greater or less solubility of the crystalline body. Of course, secondary cataract, and those which are the result of fibrinous depositions or false membranous exudations upon the capsule, can hardly be supposed to fall within the pale of the remedial agency of this operative process.

It would give me pleasure, Mr. Editor, to mention other subjects connected with the science of our profession; but, conscious that brevity is the sauce of communications, unenlivened by brilliancy of style, even when questions of interest are involved in those communications, I am induced to forbear until another time.

With the heartiest wishes for the continued success of the "*Pioneer Medical Journal*" of Old Virginia,

Paris, August, 1853.

I am yours truly,
SAMUEL C. GHOLSON.

Brief account of a Wound in the Head from the bursting of a musket, the breech lodging in the cells of the ethmoid and sphenoid bones, for a period of eight years. (Communicated by a Military Surgeon.)

An officer, 32 years of age, serving in the island of Ceylon, in the year 1828, while in the act of firing at an elephant with a cut-down musket of the old description, was knocked down by the bursting of the piece. He lay insensible for some time; but, being alone, was uncertain how long; and, on the return of consciousness, found himself wounded in the forehead, the *débris* of the musket lying about him. He was, however, able to get on his feet; and, on assistance arriving, and search made, most of the shattered fragments were forthcoming, but the breech, which was nowhere to be found, until after the lapse of several days, when it was ascertained to have been the cause of the wound, and that it actually remained embedded in the skull. In the course of about three months, the patient had recovered sufficiently to resume his duties; the wound in the forehead remaining open, but being protected by a covering of black plaster. So matters rested for a few months more, when the pointed portion of the iron breech made its way through the palate, together with the head of the screw by which it had been secured to the stock, and which was still loosely inserted through its proper hole in the iron, as they had together been torn from the wood-work. They had continued to descend gradually lower and lower, so that the point of the iron was in almost constant contact with the tongue, by which its edge was worn smooth and polished. As will be naturally concluded, a profuse secretion of pus was kept up; this was generally most offensive, requiring the greatest attention to cleanliness, and a consumption of lint and tow somewhat enormous; despite all which, the discomfort of the sufferer was occasionally added to by the engendering of maggots within the wound. It was on one of these occasions, eight years after the accident, that I first saw the case, and had from the officer himself a statement of which the preceding is a summary. The worms were at this time got rid of by the use of stimulating injections, but not without great pain. The wound in the forehead was triangular, with a base about half an inch long, just above the *ossa nasi*, and from its centre to the apex was a full inch. Within was seen, horizontally placed, and level, or nearly so, with the base, the circular end of the iron; while, as before mentioned, the other end, with the head of its connecting screw, had now descended very low through the opening in the roof of the mouth, the small end of the screw being situated in the right nostril,

and easily twirled by the thumb and finger, applied to the opposite one in the mouth. There had been great desire to get away this screw; but, though so loose, there was not space to admit of its being withdrawn; to obtain which, an ingenious attempt had been made, some months previously, by Mr. Elliott, a young surgeon, to saw off about half an inch, thus reducing its length to an inch and a half; and he had nearly succeeded, when obliged to desist, from the suffering caused to the patient, but who expressed his intention to submit to a further trial at some subsequent date. The attempt, however, was never repeated. On the evening of March 25, 1836, after the excitement of a mess dinner, with much conversation, and some rather loud singing, in which this officer bore a conspicuous part, while leaning over to one side, his chair slipped from under him, and he came to the floor in a sitting posture, but did not at the time appear to have sustained any hurt; though it was scarcely to be expected that such a succussion, with a large foreign body in the head and in such close proximity to the brain, could take place without consequences of a serious and dangerous tendency. The next day, headache and general pyrexia came on, and rapidly increased with evident vascular determination to the brain, delirium ensued, and death closed the scene on the 2nd April,—the seventh day after the fall. Eight hours after death the head was examined. The brain and its membranes were in a high state of vascularity, and a small abscess was found in the anterior lobe of the right hemisphere, very thinly separated from the orbital plate of that side, and connected with a dense membrane that supplied the deficiency occasioned by the absorption of a triangular portion of the cribriform plate of the ethmoides, this adventitious membrane being, of course, the only medium of separation between the brain and the cavity containing the iron, which, on removing the membrane, was brought into view, but was only fully exposed by a horizontal section of that part of the os frontis, below the superciliary ridge and orbital plate, with the necessary vertical ones to complete the separation. The cavity formed of the ethmoidal and sphenoid cells, the inner bony structure of which had been removed by absorption, had a black appearance, and a fetid sulphurous odour; the iron was now removed without difficulty by the bullet forceps; had a black charred appearance, with a strong odour of gunpowder, and, when cleared of its adherent impurities, weighed (including the screw) within a drachm and a half of three ounces; its length $2\frac{3}{4}$ inches; and its greatest transverse diameter, $1\frac{3}{8}$ inches. The screw exactly 2 inches in length.—[*London Lancet*.

External Stimulus in Cholera.

To the Editor of THE LANCET :

SIR,—The resources of professional judgment in the treatment of cholera are not so numerous and efficient as to render a somewhat novel remedy unacceptable to those whose lot it may be to undertake the charge of many cases. I am indebted to a lady, of heroic mind and great intelligence, for the hints which led me to adopt in the collapse of cholera the powerful external stimulus which I shall presently describe. It appears that in the most desperate cases, even when life has appeared extinct, the native Indians are accustomed to apply the actual cautery freely to the abdomen, not unfrequently with the happy result of restored vitality. The remedy, based on the same principle, which I have employed in three cases with complete success in the Borough Gaol of Newcastle, is precisely similar in kind, though somewhat less harsh and formidable in degree. A piece of linen dipped in brandy is placed over the epigastrium or abdomen, and ignited; the brandy burns away in a minute or so, producing a considerable feeling of pain, which renders it necessary to secure the hands of the patient. Slight vesication will probably follow, and, if successful, in a short time heart and pulse begin to return, and the feelings of the patient are greatly improved, vomiting will also generally be put a stop to. It is probable that the application may soon require repetition, the situation being somewhat varied. In one case, now convalescent, in which death was apparently close at hand, the most complete effect was produced by a third application along the spine in the lumbar region.

Time will not permit me to give details of cases; but having now tried the *brandy blister* in seven or eight cases of total collapse, I repeat that in three it has been entirely successful, and in others has had the effect of temporarily rousing the patients; and if, as experience has since taught me, in some of these it had been repeated with more energy, greater success might possibly have resulted. The patients, now convalescent, say that it is a *severe* remedy; but are quite conscious of its beneficial effects, and attribute to its use, without any hesitation, their restoration from impending death.

I have the honor to remain, Sir, your ob't serv't,

September, 1853.

T. M. GREENHOW.

P. S.—When leisure permits I shall possibly give some details of the cases which have occurred in the Borough Gaol, exceeding considerably 100 in number, of whom 8 are dead. The small number under treatment (less than 20) are all likely to recover. About a week ago more than 40 patients were ill at the same time.

On the Application of Nitrate of Silver in Acute Tonsillitis.

By M. HERPIN.

M. Herpin states that he finds the application of nitrate of silver in substance to be a most excellent mode of abridging the duration of acute tonsillitis, preventing suppuration in persons liable to this occurrence. Even in the most intense cases, accompanied by great febrile action, he has not had to make more than three applications. If suppuration has already occurred, the application is of less avail, and is then, on account of the closure of the jaws, often impracticable. The application must be carefully and methodically made opposite a window. If the velum is inflamed it should be touched in passing from one tonsil to another, as also may be the uvula—but as a spasm of the fauces is often then induced, this should be left to the last. If the application is made within the first twenty-four hours, a single one often suffices: and this happens in persons who are liable to relapse of this affection, and have already derived benefit from the caustic. If seen later, two applications at the interval of a day, or even three, are required, although the first at once checks the progress of the disease. More than twenty-four hours should never be allowed to elapse between the applications. Since he first recommended this practice, many of M. Herpin's colleagues at Geneva have adopted it, and with the best effects, in securing the rapid dispersion of a disagreeable though not a dangerous disease.—[*L'Union Médicale*, and *Med. Chir. Review*.]

Statistics of Operations for Cancer. By PROFESSOR PAGET.

In a recent note to *The Lancet*, Mr. Paget qualifies a remark which he had made in his lectures at the College of Surgeons, "that persons operated upon for cancer die upon an average thirteen months sooner than those upon whom no operation has been performed," by stating that he referred only to scirrhus cancer of the breast. He says—

In such cases, I believe that the general average duration of life, after the patient's first observation of the disease, is forty-nine months; that the average life of those whose breasts are removed, and who survive the effects of the operation, is forty-three months; and that the average life of those in whom the disease is allowed to run its course is about fifty-five months.

In the second lecture, I said that the general result of operations for medullary cancers is very different; and that, although they are so seldom long survived, that they are generally considered to be less beneficial than the operations for scirrhus

cancers of the breast, yet on the whole they are more so. The general average of life of persons affected with medullary cancer of the eye, testicle, breast, bones, or other external organ, may be reckoned at about twenty-four months from their first notice of the disease; but I believe the average for those from whom the primary disease is removed, and who do not die in consequence of the operation, is about thirty-four months; while the average for those in whom the disease is allowed to run its course is scarcely more than a year.

In the third lecture I expressed the belief that, on the whole, the operation for epithelial cancers is even more effective in prolonging life than the operation for medullary cancers; but that the wide diversities in the duration of life amongst those affected with this form of cancer makes it very difficult, at present, to deduce such an average as may be relied on. And I would repeat what I said in one lecture respecting all these averages—namely, that such general results serve only general considerations in the treatment of particular cases of cancer. They may justly determine a general rule of action, but it can be only such a rule as must admit of numerous exceptions. In many cases of scirrhus cancer there are sufficient reasons for operating; and, in many cases of medullary and epithelial cancers, reasons as sufficient for refraining. The right course must, in each case, be determined by a just appreciation of all the conditions each presents.—[*London Lancet*.

A Bean extracted from the Bladder by the Lithotrite.

M. Maisonneuve, Surgeon to the Hôpital Cochin, in Paris, has just removed a large bean from the bladder of a man, 27 years of age, who had wilfully introduced this foreign body into his urethra. The bean, which, on the introduction of the lithotrite, was felt floating on the urine, was luckily caught, and withdrawn without crushing. The urethra must have been considerably stretched by the passage of the lithotrite partially open, and holding a foreign body more than half an inch long. It is, of course, advantageous that the bean should have been extracted whole; but in supposing that the surgeon had crushed it, the fragments would probably have been passed, as well as a portion of the bean, which was evacuated the day after the operation.—[*Ibid*.

Practical Remarks upon Polypi of the Uterus. By HELFFT.

The fact that uterine polypi grow rapidly during the period of gestation, and constitute a very dangerous complication, makes it advisable to remove even small polypi, when they are dis-

covered, although there may have been neither hæmorrhage nor symptoms of pressure. When a polypus is seated at the neck of the uterus, and protrudes into the vagina, it can give rise to hæmorrhage during pregnancy; or it may lead to abortion, or, at a later period, it may interfere with delivery. If it be in the uterine cavity, dangerous symptoms occur, for the first time, after the birth, when hæmorrhage takes place; or the uterus becomes inverted; or the polypus forms a swelling, which closely resembles, and may even be mistaken for, an inverted uterus.

A polypus discovered in the early months of pregnancy, of moderate size, and attached by a narrow peduncle, can be removed by torsion. Usually this operation has but little effect upon the uterus. When the polypi are larger, and are attached by a broader base, the ligature is unnecessary; but, in this case, inflammation of veins is a possible occurrence, or there may ensue a direct absorption of the putrid matter caused by the strangulation of the ligature; abortion, too, is to be feared.

Polypi within the uterus do not occasion any serious inconvenience during pregnancy, in spite of the great size which they sometimes acquire. It is only after the removal of the placenta that their presence becomes known, by the swelling which still remains. In lucky cases, the polypus comes down of itself into the vagina a longer or shorter time after delivery, either without hæmorrhage or any unfavorable symptom, or attended with severe pain and copious loss of blood. When the presence of an intra-uterine polypus has been made out, it is best to leave the organ to itself, should it be disposed to contract; in which case there is no fear of hæmorrhage. When, however, severe bleedings come on, the polypus must be removed, either by torsion, or by the ligature and the knife.—[*Ibid.*

New Method of Inducing Premature Delivery. By SCANZONI.

The author was induced, by observing the active sympathy between the breasts and the other parts of the sexual apparatus, to try to produce premature delivery by irritating the nerves of the mammary glands. The first experiment was made upon a young woman, aged 24, who, two years ago, had been delivered by perforation, in consequence of contraction of the pelvis. In the thirty-second week of utero-gestation, apparatus constructed of caoutchouc, forming sucking-pumps, were put upon the nipples. During three days they were used about seven times, the process going on upon each occasion for two hours. After the third application, the neck of the uterus be-

came shortened; after the sixth, severe labor-pains came on; after the seventh, the child was born.

The only danger likely to ensue from this very simple method of treatment is inflammation of the mammæ; this can be met with proper treatment.

A second case, of similar kind, occurred to the author. A young woman, *enceinte* for the first time, suffered so severely from dyspnœa, connected with organic disease of the chest, that premature delivery was necessary for the preservation of her life. After the third application of the sucking-pumps, an apparently dead child was born; respiration, however, was soon re-established. The author remarks, that this case was not quite conclusive, because premature delivery occurs often in connection with severe dyspnœa, independent of other influences.—[*Verhandl. der Med. Phys. Ges. zu Würzburg*, 1853, and *Ibid.*

On the Use of Ext. Belladonna in the Treatment of Obstinate Vomitings in Pregnant Women. By R. L. SCRUGGS, M. D., of Louisiana.

It is not a little surprising that an article capable of promptly arresting so grave a disease as the obstinate, and even dangerous vomitings, which often supervene in the course of pregnancy, should have been so entirely neglected or overlooked by the profession generally; particularly when it is remembered that M. Bretonneau, more than eight years ago, announced the important fact to the profession in Europe, and pointed out the circumstances under which it ought to be used, the manner of applying it, &c. In the many recent discussions and papers read upon the subject of the propriety of inducing premature labor for this disease, I am surprised to see no allusion made to this remedy whatever. Even in that excellent and unique work, published in 1851, by Chas. D. Meigs, "upon woman and her diseases," no mention is made of it, notwithstanding he says that the affection is so untractable as to justify the induction of premature labor. M. Trousseau, in a clinical lecture, delivered at the Hospital Necker, in January, 1848, thus alludes to M. Bretonneau's theory and practice in these cases.*

"Five years ago," remarked the Professor, "a lady pregnant for the first time, who, for six weeks, had vomited both liquids and solids, called in M. Bretonneau. He found the patient in a most alarming state—the affection progressed rapidly, and threatened to become inevitably fatal. This woman, when questioned, complained of sharp

* Yandell's Letters from Paris.

uterine pains. In a primipara, the fibres of the uterus are not broken in, if you will allow the expression, and not habituated to the process, and allow themselves to be distended with difficulty; and it is this which causes the pain. M. Bretonneau thought that the uterine pains were the cause of the other symptoms, and that if he succeeded in mastering them, he would overcome the sympathetic vomitings of the patient. Acting upon this idea, he covered the hypogastrium repeatedly with a mixture of belladonna; the vomitings ceased the same day, and recovery ensued. Sometime afterwards, he had occasion to observe another case, where the pains of the uterus did not exist; but he thought that even if the brain did not perceive the pains of the uterus, the ganglia might take note of them, and reaction occur. To modify these accidents, he believed it to be sufficient to prescribe the belladonna mixture, and was again gratified with complete success. The result of these and similar cases justifies him, he thinks, in laying down the following principle:

“Whenever, in a woman, pregnant for the first time, or many times, vomitings supervene during the course of gestation, frictions should be made upon the hypogastrium with a mixture of belladonna, and the vomitings will cease.”

The Professor then asks, “In what manner does the belladonna act? I confess it is impossible to determine. Can it be supposed that the fœtus, in being developed, painfully distends the fibres of the uterus; that the vomitings are sympathetic, like those which supervene in cystitis, for example? This is possible. Whether it be this or something else, it is upon this hypothesis that M. Bretonneau has employed his remedy. He has promulgated his theory, and has endeavored to confirm it by facts. The fœtus distends the uterus, the nervous ganglia take cognizance of it, and sympathetic vomitings are the consequence. This is the theory, which you may adopt or not, but which must be admitted to conform, with marvellous exactness, to the therapeutical results.”

I had but just seen these opinions of M. Bretonneau announced, when I had an opportunity of making a practical application of them. My first patient, however, presented other symptoms than those described by him, for the relief of which he prescribed the belladonna mixture with such confidence and success. The result in this instance was equally fortunate.

Called in consultation, July 14th, 1848, to Mrs. L. W. D., æt. 24. This lady had been married about two years, and had miscarried once during the time, at about the fourth month of utero-gestation. She had been attended for several days before I saw her, by an experienced and scientific physician, who, failing in his efforts to relieve her of a most distressing cough, solicited my assistance.

Pregnancy, at the time of my visit, had not been suspected;

but upon a more thorough examination of the case, assisted by the answers elicited from her by questions in reference to this condition, we satisfied ourselves of the existence of pregnancy. I immediately suggested to my colleague the theory of M. Bretonneau, and asked, if this theory be correct, might not the sympathetic irritation produced by the distended and fretted uterine fibres react as well upon the bronchial mucous membrane—thus producing cough—as upon the stomach? He caught at the idea at once, and we directed equal parts of ext. belladonna and lard to be rubbed together, and frictions made with the mixture, every four hours until our return. The next morning we were much gratified to find that the cough had entirely disappeared, and the patient feeling, of course, greatly relieved. She got up in a short time, and continued to enjoy moderately good health until she removed to Memphis, when we lost sight of her, but understood she was taken ill some months afterwards, and after suffering for several days, was delivered of a dead *fœtus*, at about the seventh month of utero-gestation. Having repeatedly seen the vomiting return after having been arrested by the application of belladonna over the hypogastrium, and again arrested by the same means, as promptly as at first, I am inclined to think now, that had the belladonna been used again in her case she might have gone to her full term, and possibly borne a living child.

Since the occurrence of this case, I have had repeated opportunities of testing the virtue of this article in similar cases, and in no instance has it failed to relieve the patient. It may be proper to remark, however, that any complications that may be found to co-exist with this condition, such as gastritis, gastro-enteritis, constipation, &c., ought to be treated with their appropriate remedies; and when the vomiting has continued for a considerable time, I have usually applied cups, fomentations, &c., under the impression that the excessive vomiting itself had excited inflammation of the gastric mucous membrane. But this has probably been an unnecessary proceeding, since it would appear from the observations of some of the most distinguished physicians of Europe, that no such condition of the mucous membrane of the stomach has been found to exist in subjects examined after death from this disease. My own observations tend also to establish this fact. At least, I have repeatedly found that the most active means that could be used for the subduction of the supposed gastric inflammation, proved altogether unavailing until the belladonna was applied over the hypogastrium, when the vomiting has invariably ceased. Very recently I delivered a young married lady of a healthy female child, who about the middle of December last, was taken with ex-

cessive vomiting, attended with such violent straining, that when I arrived, I found that the matters ejected from the stomach were streaked with blood. The stomach being also tender to the touch, I proposed at once, the application of the cups. But no persuasion could induce her to allow scarifications, nor even dry cupping. Failing in this, I ordered a purgative enema, a stimulating foot bath, a mustard cataplasm over the stomach, and used a variety of anti-emetic mixtures, but all to no purpose. I then applied a belladonna plaster over the hypogastrium, and very soon she was relieved of her nausea and vomiting, and had no return of it for eight or ten days, when the plaster was again resorted to, which relieved her as promptly as at first and she had no return of it afterwards.

I have now under my charge a young married lady, pregnant about six months, who suffered for a considerable time before she applied to me for relief. The belladonna here, as usual, was prompt and effectual in stopping the vomiting. She made use of it once or twice afterwards upon feeling slight nausea, but she is now, and has been for several weeks, perfectly healthy and free from any trouble of that sort.

M. Dubois, while upon the subject of the "induction of abortion in the vomiting of pregnant women," during a recent discussion in the *Academie de Medicine*, "stated the results of his experience in relation to obstinate vomiting in pregnancy. In proof that this is oftener a more dangerous occurrence than is usually supposed, he stated that in the course of thirteen years he had met with twenty cases in which it had proved fatal. That obstinate vomiting is but the exaggeration of the natural sympathetic vomiting of pregnancy, and not due to any special lesion, is proved by the facts that at the autopsies nothing is found, and that when the process of gestation becomes arrested, whether spontaneously or artificially, the vomiting is ordinarily put an end to, although the woman may not be delivered until several days after, of a dead child, and may yet die of the effects of what she has undergone." (*Amer. Journal of the Medical sciences*, Jan. 1853.)

The observations of Dubois, Bretonneau, Ems, Duclos, Trouseau, and others, seem to go to establish the fact, that, no matter how violent or continued the vomitings are in these cases, there is no real inflammation of the stomach produced by them, and consequently any anti-phlogistic measures resorted to in view of this condition of the stomach, would appear to be, to say the least of it, unnecessary. Notwithstanding my own observations tend to establish the same fact, yet I cannot recommend an entire neglect of such adjuvant measures as would naturally suggest themselves to the intelligent physician. The bowels

of course, ought to be attended to, and the cups, fomentations, poultices, &c., may, I think, be justifiably resorted to upon a mere suspicion of gastric inflammation, for the patient is but slightly inconvenienced by them, and they will certainly relieve any inflammation that may exist. But I must protest against the blister. It will do no good at the time, and prove a source of great annoyance to the patient afterwards.

I have also used the belladonna ointment in cases of painful menstruation, with apparent benefit, but my experience with it in the treatment of these latter cases, is too limited to justify me in recommending it with any great confidence.

I have used it recently in a very violent case of dismenorrhœa, and it appeared to assist in relieving the pain; but so many other measures were resorted to, at the time, for the relief of this young lady, that it is impossible to determine what part, if any, the belladonna acted in giving the relief. I think however it is worthy a still further trial in these cases.

In conclusion, I would suggest that it may be applied much more conveniently, and with equal efficiency, to the hypogastrium, by spreading the extract, undiluted, upon soft leather, in the manner of using the exp. cantharides, than by the plan originally suggested, of rubbing it on with the hand. This plan has the advantage, first, of being more cleanly, and secondly, may be re-applied by the patient herself, at any time when pain or nausea is felt.—[*Southern Journal of Med. and Physical Sciences*.

Different Modes of arresting Hemorrhage from the Extraction of Teeth. By BENJAMIN WOOD, M. D., Nashville, Tenn.

Dr. A. Saltonstall, of Columbus, Ohio, Miss., reports a case (American Jour. Dental Science, Oct. 1852) of hemorrhage from the extraction of a tooth, which, having resisted the usual means—astringents, escharotics and compression—was arrested by an artificial fixture acting both as compress and actual cautery. He “took a piece of pure silver plate, and cut it in shape to fit between the teeth and cover the lips of the orifice about the eighth of an inch on each side. This was bent to fit the parts, and heated to a white heat, and suddenly applied to the place, where it remained several days. When it was removed the coagulum came away with it. The orifice was examined, and a very delicate covering, resembling tissue paper, had formed over it.”

Dr. Levison, of England, in an article published about a year ago, says, that in cases of excessive hemorrhage, where the ordinary styptics cannot be depended upon, “we may arrest

the dangerous hemorrhagic flow with certainty by destroying the vessels with the bi-chloride of zinc," and gives cases where this agent, as a last resort, had been successful in his hands. In alveolar hemorrhage, pieces of cotton dipped into the bi-chloride were forced down to the alveolar cavities. It was attended, however, with great pain.

It may be remarked that in some cases where success is ascribed to the last remedy employed, the result may have been owing to a natural stasis of blood from exhaustion of the patient; such hemorrhages sometimes continuing for hours, until after fainting, and then ceasing altogether without any intervention. An interesting case of the kind was related to us a few years ago by a reliable lady who was herself the subject. The bleeding had continued with but occasional and partial intermissions for three days. On the night of the third it ceased, and she retired, but about midnight she was awakened by a renewed flow of blood. Exhausted by the loss of blood and sleep, she merely arranged a wash-bowl upon a chair so as to receive the blood as it flowed from her mouth, and with her head supported by a pillow, she soon fell asleep.—In this position she was found early the next morning, in a state of unconsciousness. The bleeding had effectually ceased.

It is fortunate these cases rarely occur. We have had but few that were troublesome. Besides the use of nitrate of silver (which as a styptic we have found more reliable than anything else that we have used,) and the application of pressure, we have in two or three instances resorted to a partial *torsion of the bloodvessels* at the bottom of the alveolar cells. This depends upon the principle that the mouths of the vessels contract more readily when lacerated than when divided with a smooth cut or broken short off as may happen in extracting a tooth, and that mechanical irritation has a tendency to induce contraction. *The modus operandi* (as we received it while under pupilage, from our brother, Dr. J. S. Wood) consists in passing a stylet or an ordinary excavator of the proper shape, to the bottom of the socket, until a twinge of pain is felt, and then giving the instrument a sudden turn, so as to twist or lacerate the artery—its situation being indicated by the impression made upon the nerve which it accompanies.

We know of but one instance in this vicinity, of death having occurred in consequence of the kind of hemorrhage under notice. This was in Russellville, Ky., about two years ago. The patient's tooth was broken in extracting, leaving a portion of the fang which could not be gotten out. Pressure, as well as styptics, &c., was tried, but without arresting the hemorrhage, the man dying, according to the recollection of our in-

formant, in about fifteen hours after the operation. We would like very much to be favored with a report of the case in full.

In case a tooth is broken and the bleeding proceeds from the pulp, cavity or nerve canal, the obvious means of arresting it would be to plug the orifice with a metallic or wood stopping. A hickory peg or sliver would perhaps be as good as anything. If the orifice be too small to receive a stopping, it should be enlarged by means of a drill.

Pressure applied directly to the bleeding vessels and retained in its place is reliable in such cases of hemorrhage; but there is sometimes considerable difficulty experienced in its application. A ready and effectual means is to roll up pellets of cotton firmly in the fingers, of a size to suit the alveolar cells, and introduce them with considerable force, notwithstanding it be attended with considerable pain, as it always is, we believe when the hemorrhage has continued for some time. They may be wet with some styptic solution, or coated with powdered lunar caustic. After the first pellet has been introduced, we usually fill the remainder of the cavity with one of a larger size, and if it be a molar tooth with two or three bifurcations, cover the whole with a third, sufficiently large for the purpose but no larger, crowding the edges under the margins of the gums, which in ordinary conditions, where the blood possesses its due amount of fibrin, and is of a plastic character, will be found to adhere to the cotton with sufficient tenacity to retain it in its place. It will be safest to let this stopping remain until loosened by the suppurative process. If not thrown off, however, or removed in the course of a few days, the pellets thus introduced are apt to prove the source of great suffering in the sockets, bespeaking the inflammatory action preparatory to suppuration; but when this occurs we think they may be removed at once, regarding it as evidence that active reparation has commenced.

The "waxed cones" recommended by Dr. B. B. Brown, which are made by cutting a piece of linen previously coated with melted beeswax, into tapering strips and rolling these in a form to suit the sockets to which they are to be applied, may be used to great advantage in many cases.—[*Ibid.*

Poisonous Dropsical Inoculation.

An accident of a singular and dangerous nature recently befel the celebrated surgeon, Prof. Langenbeck, in Berlin. Having been called in to attend a lady of high rank, in a most advanced and perilous stage of dropsy, Dr. L. deemed it necessary to proceed without delay, to puncturation, and this with-

out waiting for other assistance. The operation was, therefore, instantly and successfully performed, and the patient, previously at death's door, relieved and saved. During the operation, however, some of the acrid discharge fell upon his hand, and was of course washed off when the work was completed; but, ere long, the hand, arm, throat and neighboring regions began to swell, and all the febrile and inflammatory symptoms of animal poison ensued. Vigorous remedies were forthwith employed, and the danger averted, but the Professor is not yet so entirely recovered as to enjoy the full use of the side affected, whilst the venom has shown its lurking agency by causing eruptions on other parts of the body.—[*Boston M. and S. Jour.*

Miscellany.

OUR JOURNAL.—With this number we close the Ninth Volume of the New Series of the SOUTHERN MEDICAL AND SURGICAL JOURNAL, and the third year of our Editorial supervision of its pages. The duties of an editor are so onerous that nothing short of enthusiasm in the cause of Science can make them tolerable for any length of time. The consciousness, however, that the work, when done, meets with the approbation of those for whom it is intended, lends to the task the charm of success, which is always a source of much gratification. The evidence of this appreciation is to be found in the continued and increasing patronage of the publication; and, judging by this standard, we have every reason not only to be satisfied, but also to continue at our post with increased diligence. That any periodical can be so conducted as to please every one, is not to be expected. Some will become offended because what they may write is not always deemed admissible; others because they are dunned by the printer when they forget to comply with his terms. Some censure the publisher for the irregularities of the mails, and others find fault with the editor for the opinions of contributors to the original department of the work. There are even some who never forgive a refusal to admit personal controversies into a publication exclusively devoted to Science. We are happy to say that we have long since ceased to be annoyed by such small minds, and that the intelligence and respectability of the patrons of the Southern Medical and Surgical Journal furnish abundant testimony in favor of the manner and spirit in which it is conducted. This is now the oldest Medical Periodical south of the Potomac, and one of the oldest in the Union: its list of subscribers is probably inferior to very few, if any, in the country: it is and has always been

independent of sectional feeling or party bias of any kind, unless the preference of American to foreign contributions be regarded as savoring of partiality. This preference has been acknowledged on a former occasion, and the reasons for it then advanced. We can see no reason why American Journals should be vehicles for the spread of foreign fame to the almost entire neglect of the just claims of domestic talent.

In conclusion, we would say to our Patrons: aid us in doubling the number of our subscribers and contributors, and we will use our best endeavors to duplicate the value of the Journal.

Vesicatories.—A cotemporary states that "When it becomes necessary to use blisters to inflamed joints, as in some forms of rheumatism, the previous application of a mustard cataplasm will be advisable, as, after full vesication, when there is a redundance of fibrine, the vesicle will be filled with it, and the vessels thereby relieved to a much greater extent than without the previous use of the mustard."

Our experience has taught us, that the previous application of mustard plaster will retard the action of the ordinary ointment of cantharides, and sometimes render it almost impossible to produce vesication.

A Curious Case—the man who has slept five years.—We called yesterday to see the man who has been asleep for five years, and whose case was detailed in the Times some weeks since. We found him in what seemed like a sound sleep. He was lying in bed, his eyes nearly closed, his respiration rather slower than is usual, his breathing a little stertorous, pulse some seventy-five strokes in a minute, soft and weak. On attempting to open his eyes, he firmly closed them, and when, by force, the lids were opened, the eyes were rolled upward, so that it was impossible to see the pupils. The mouth was slightly opened; on attempting to open it wider, the jaws were instantly locked. There was a constant tremor of the eyelids, and from his mouth there was some drivelling. His body was extremely emaciated; his arms were folded upon his breast, and any attempt to remove them was strongly resisted. The muscles seemed rigid and tense when the effort was made, and indeed it was impossible, without violence, to change at all the position of his limbs. Once during our stay he drew a long breath, like a man who is about to turn in his sleep. At another time he hitched himself up a little in bed. He was lifted up bodily and seated on the side of the bed; his head was still bent forward on his chest, his legs crooked under him at the same angle, and his arms folded as when he was lying. There was nothing to indicate that he would not retain the same position for weeks. We lifted one foot, the other came up with it. There was little or

no bending at the knee, or at the hip ; the feet were raised only as the upper part of the body was carried backwards. He was placed standing upon the floor. It required a few moments to balance him exactly ; after that he stood in the same position so long as we remained ; there was nothing to indicate that he would not maintain the same posture for a month.

This certainly is a most marvellous case. There is not the slightest chance for any collusion or deception in the matter. Many of our best physicians have examined him,—none, so far as we can hear, believe any deception in the case to be possible. From physicians in the western part of New-York, and from men of the highest standing, we are assured that the story which is told of him is perfectly true.

Though we have given a history of it before, a brief re-statement is warranted by the interest created by his presence here. His name is Cornelius Vroman ; he was born in Schoharie county, but has lived since he was seventeen years of age in Clarkson, Monroe county, not far from Rochester. He was a hard-working man, a good worker, temperate, trusty, and at the time when this strange sleep came on, he was working on the farm of Mr. Moses Jennings. His mother is dead ; he has a father and two brothers living in Clarkson. On the 19th of June, 1848, he felt unwell enough to call in Dr. John S. Cole, who found him complaining of some pain in the stomach and in the head, for which he prescribed. After this, without becoming any more sick, his sleep each night grew longer, until at last it was found impossible to wake him. Out of this sleep he has never come, to remain wakeful for more than sixteen hours at a time ; and the aggregate of all his waking hours since the seizure is not over three days. At first they were oftener, but now the waking intervals recur about every six weeks. The last time he awoke was while he was in Rochester, some ten weeks since, which gives us a hope that his waking-hour now approaches, and that we may see him in his wakeful condition. When awake he seems totally unconscious of his peculiarity, and has said some things which indicate that he remembers matters as they were before his change. They say that he straightens himself up then, and walks as limberly as others. Yet, now, to handle his limbs, we fear that they must be partially ankylosed ; but on this point we are not satisfactorily informed.

His diet consists principally of milk, sometimes with a little bread soaked in it. It is with some difficulty that it can be administered. The jaws must be forced open as in tetanus, and the liquid poured in between his teeth. Once he went without any food for five days ; but his friends objected to any farther conduct of the experiment, though there was no change in his symptoms during that time. When the seizure occurred he is said to have weighed 160 pounds ; now, he cannot weigh over 90 pounds. His height was six feet two inches. The secretion of the kidneys is discharged once or twice a day ; it is very high colored, and not much diminished in quantity. Possibly it is from habit, possibly from some remains of consciousness, that in this matter he is subject to the wishes of his attendants. The

alvine evacuations are very scanty, occurring not oftener than at intervals of from six to twenty days.

Once he was left standing for three days ; there was no change in his position during that time.

We are not informed definitely as to the medical treatment to which he has been subjected. We are told, however, (and he has scars that attest it,) that he has been blistered and bled, subjected to issues, setons, and counter-irritations, of almost every sort ; that tonics and sudden stimulants have all been applied ; but medicine has exhausted its resources in vain. Once he was thrown into the water, but it produced no change.

His personal appearance is any thing but prepossessing. The beard which covers his face and lips, stands erect, and the hair of his head also stands on end. We are assured that this is no peculiarity in the family, and that nothing of this sort, no tendency to epileptic or kindred diseases, has ever been noticed in the family.

Medical men regard this case with the profoundest interest. It is entirely without a parallel in medical history. Catalepsy it has been called, but the rigidity of the muscles is quite uncommon ; indeed the limbs of the cataleptic generally seem to be plastic, maintaining the position in which they are placed—yielding readily, however, to any counterbalancing power.

We have seen another form of it, in which, though the head, the neck, or the limbs would take any posture given them, after the lapse of a few moments they would begin gradually to return to the position occupied at the time of the seizure. The maintenance of the cataleptic state for even a few days is extremely rare. The tetanic spasm of the mouth upon attempting to open it, the forcible closing of the eyelid, and the other slight indications of consciousness, are not common in catalepsy.

The ecstasy of nosologists recognizes a lack of all consciousness and recognition, and great muscular rigidity, but more or less mental excitement is supposed to be indicated by that term. Strange ecstatic seizures connected with hysteria are on record.

Cases are recorded where lethargy—or cataphora, as some prefer to call it—and coma have been greatly prolonged ; but in these we believe the voluntary motions of which we have spoken were entirely absent. No case, however, of either has been protracted for years as has this. This curious case seems both to baffle all medical skill, and to defy the history of medicine to adduce a parallel.

[We copy the foregoing article from the N. Y. Daily Times, one of whose editors, Dr. F. Tuthill, is the writer, and to whom the readers of that paper are indebted for frequent medical contributions to its columns, and which are appreciated by the profession, and cannot fail to promote its circulation and patronage. We have seen the singular case alluded to, and can vouch for the accuracy of the description here given.

We suppose the man to be laboring under that form of Insanity,

denominated Dementia, dependent upon a pathological condition of the brain and spinal cord, involving the nervous centres, and producing the remarkable rigidity and fixedness of the muscles of the limbs, here referred to. It cannot be regarded as Catalepsy, nor is he asleep, as has been alleged, nor can he be called comatose, at least when we saw him. The quivering of the eyelids, and resistance to opening them, the voluntary action of numerous muscles, even those most rigid, demonstrate that he possesses consciousness, and employs volition, always however so morbidly developed as to preclude the idea of feigning or deception. He is certainly an object of deep commiseration, and ought to be placed in the Lunatic Asylum for curative treatment, for, of even such a case, we have no warrant to despair. There can be little doubt that the cause of this sad spectacle of human woe has been masturbation, another warning against this horrible vice, and which should be heeded by the young.]—*N. Y. Med. Gaz.*

“*The Rochester Democrat*,” of the 24th ult., announces the death of the “Sleeping Man,” CORNELIUS VROMAN, who “died at his brother’s residence, in Clarkson, on Monday, the 17th inst. While on exhibition in New York, he was taken sick, which seemed to induce a wakeful state for a short period, and then a stupid condition, with intervals of wakefulness, until he was brought home on the 14th. He talked but very little, inquiring after his mother, who had been dead two years, and his father and brothers, whom he seemed partially to recognize. He complained of great internal heat, and soreness of his throat and stomach. On the morning of the day of his death he called for food, and ate a hearty meal, and from that time he seemed to be in pain until 2 o’clock P. M., when he died without a struggle. His age was some 34 years.”—[*Ohio Med. and Surg. Journal.*]

Duty of Medical Men.—Dr. Todd, in his farewell address, on resigning his professorship, made the following remarks:—

“It appears to me, that when a man proposes to devote himself to the practice of an honorable profession, he has a twofold duty to perform; first, to fit himself to the utmost of his ability for the practical duties of that profession, and, secondly, having done so, it is incumbent on him to divest himself, as far as possible, of every engagement which may interfere with his bending his thoughts and attention to the various anxious, difficult, and often perplexing questions which are continually arising in the course of his professional practice. Every member of a liberal profession should keep it constantly in view that he exercises his calling not simply for his own personal benefit, but for the public good, and for the good of his profession at large. So every practising physician or surgeon, whether the sphere of his labor be within wide or narrow limits, should bear in mind that in the successful application of his art, by fair, honorable, and truthful means, is involved the repute and estimation in which his profession is held by the public at large. Let each of us act under the feeling, that to himself specially is committed the keeping of the honorable character

and the scientific credit of our common profession, and he will have the strongest motives, not only to eschew everything that savours of charlatanical pretence, but to seek for and insure the highest means of moral and intellectual culture.”—[*Med. Gazette.*]

Adulterated Cream of Tartar.—A very large portion of the cream of tartar used for domestic purposes, and, what is even worse, much of that used for medicine, is badly adulterated. A writer in the Boston Journal says that an examination lately made of six specimens showed in the purest sixty-two per cent. of foreign matter. The consequences are unwholesome bread and inoperative medicine. An extensive dealer in the article states that three barrels of alum and three of flour were lately sent to a mill in Boston, with instructions to be manufactured into cream of tartar, and to be labelled with the name of the manufacturer. The man refused to place his label upon a spurious article, and the raw material was sent to some more compliant person. Ground cream of tartar is sold in many of the shops lower than the crystalized can be afforded, and is of course adulterated. Grocers should be very careful in their purchases, and house-keepers should be quite as careful. The writer in the Boston Journal says of the specimens analyzed :

“The added ingredients consisted of white sand, ground pumice, ground rice, and flour. This is a vile compound to offer to a sick child or an adult invalid. If the purchaser will provide himself with a small phial of the solution of iodine, and place a drop in connexion with the suspected article, the presence of flour, or any article containing starch, is instantly shown by the blue tint which follows. Let him place a few grains in boiling water, and if it is not entirely soluble, let him reject it. An experienced dealer can always judge with a great degree of accuracy by the appearance of the article. Pure cream of tartar is intensely white, and has a degree of moisture and cohesion about it entirely unlike flour, or any of the articles used to adulterate it.”

It is a wonderful evidence of ingenuity—to say nothing of the rascality of it—the great extent to which adulteration of the most common articles is carried. Articles so cheap that it would not seem an object to cheat in their preparation are mixed up with something a little cheaper, and often very deleterious to health. Stringent laws should be passed for the inspection of all substances intended for food or medicine; the spurious articles should in all cases be destroyed without mercy, and severe penalties should be visited upon those who manufacture them or knowingly sell them.—[*Providence Journal.*]

Cholera Infantum.—The efficacy of sub-nitrate of bismuth in this affection is incontestable. During the past summer we have had occasion to employ this remedy in a considerable number of cases, and have had every reason to be satisfied with its effects. The bismuth is best administered to children in suspension; the following formula is convenient: Sub-nitrate of bismuth, 5ij; gum-arabic in powder, 3j;

orange-flower water, and simple syrup, each, $\mathfrak{z}\text{j}$; water, $\mathfrak{z}\text{ij}$. A tea-spoonful every hour. We have found the anti-vomitive powers of this mineral to be very remarkable.—[*Virginia Med. and Sur. Jour.*]

Gleet.—M. Bourgeois has found the following formula of service in this obstinate affection, Balsam of tolu, $\mathfrak{z}\text{ij}$; liquor of sub-acetate of lead, $\mathfrak{z}\text{ij}$; linseed oil, $\mathfrak{z}\text{iv}$. Reduce the balsam to a fine powder; add three drachms each of the lead-water and oil; then make an emulsion of the whole by adding gradually the remainder of the oil.

This injection is intended for blennorrhœa; the amount of balsam and of sub-acetate of lead being diminished, the remedy may be advantageously employed in gonorrhœa.—[*Rev. Médicale. Ibid.*]

Otalgia.—Dr. Delioux announces (*Bulletin de Thérapeutique*) a simple remedy, which he has found efficacious in relieving a large number of cases of neuralgia of the ear, and also in curing those cases of erythism in which the chief symptom consists in distressing humming and ringing in the ears. This is local etherization. A few drachms of ether are placed in a bottle, the mouth of which is adapted to the external meatus. The bottle is grasped in the palm of the hand, and the animal warmth suffices to volatilize the ether. Dr. Delioux states that the effects of this treatment are rapid and permanent.—[*Ibid.*]

Homœopathy.—The following extract is taken from a clever letter in the Leicester Journal, signed "Chirurgicus."

"Homœopathy has had its day. Excepting in Vienna, it is now comparatively little heard of in Germany, and notwithstanding Leipsic is the head quarters of the doctrine, the homœopathic hospital of that city, a small house in the suburbs, contains only eight beds, of which Mr. Lee, who lately visited it, found only two or three occupied. In Paris, M. Andral put it to the test of experience in one of the general hospitals, and the result was a total failure. It was therefore discontinued. He treated 130 to 140 patients in the presence of the homœopathists themselves, adopting every requisite care and precaution, yet in not one instance was he successful. In Russia, a German homœopathist was invested by the Grand Duke Michael with full powers to prove, if possible, its advantages over ordinary treatment on a certain number of patients in the wards of a military hospital. In two months, however, he was not allowed to proceed further. The Russian Government tried likewise the comparative treatment in two hospitals of a number of patients with homœopathic globules, and a number of other patients with no drugs of any kind; and the results were found very similar in both instances. A trial was given at Naples by Royal order. The results of the observations were:—1st. That the homœopathic treatment produced no effect; 2d. That it had the serious inconvenience, in several of the patients, of preventing the employment of remedies by which they might be cured. In London, there are two homœopathic hospitals, both of which are in a declining

state, and one I hear is now about closing, if not closed, for want of funds. A celebrated physician speaks of it thus :—‘ Everything in it, and out of it, fully confirms the view, that, as to practice, homœopathy is truly a nonentity ; it is literally, as your author Huc says, (see p. 86,) the swallowing of names only.’ To swallow the name of a remedy, or the remedy itself, comes (say the Tartars) to precisely the same thing. Homœopathists cannot point out a single medical authority now authorizing homœopathy. So much for its decline. The gradual progress of medical science sufficiently shows that we do not let our patients die, as homœopaths do. Witness such facts as the following :

IN LONDON.

		Died under 5 years.
From 1730 to 1749, out of every 100 children born		73
“ 1750 to 1769,	“ “	63
“ 1770 to 1789,	“ “	51
“ 1790 to 1810,	“ “	41
“ 1810 to 1829,	“ “	31

Regarding the capability of the public to judge the value of medical remedies in curing disease, I quote Archbishop Whately :—Nothing is more common than to hear a person state confidently, as from his own experience, that such and such a patient was cured by this or that remedy ; whereas, all that he knows is, that he took the medicine and recovered. A termination and a cure are two vastly different things.”—[*London Lancet*.

BIBLIOGRAPHICAL.

The Obstetric Catechism ; containing 2347 Questions and Answers on Obstetrics proper. By JOSEPH WARRINGTON, M. D. 150 illustrations. Philadelphia : E. Barrington & Haswell. 1853. 12mo., pp. 445. (For sale by T. Richards & Son.)

A Manual of Obstetrics. By THOS. F. COCK, M. D., Physician to New York Lying-in Asylum, &c., &c. N. York : Samuel S. and William Wood. 1853. 12mo., pp. 250. (For sale by T. Richards & Son.)

These two works are evidently intended to facilitate the study of Obstetrics by the omission of all matter deemed by the respective authors as more or less superfluous. Our opinion of all Manuals on medical subjects is the same—viz., that they may be read with advantage by *beginners*, as an *introduction* to knowledge, but should then give place to the more elaborate treatises, without which no one can be qualified to enter upon the duties of so responsible a profession as that of Medicine.

The works before us, especially the “*Catechism*,” are well adapted to the purposes of young students.

The Prescriber's Pharmacopœia : containing all the Medicines in the London Pharmacopœia, arranged in classes according to their action, with their composition and doses. By a Practising Physician. Revised, with additions, by THOS. F. COCK, M. D. N. York: S. S. and Wm. Wood. 18mo., pp. 180. (For sale by T. Richards & Son.

A very convenient remembrancer or suggester. The inexperience of young practitioners in the details of prescription, or recipe making, will render this little book quite useful to them.

WORKS RECEIVED.

The Obstetric Catechism. By J. Warrington, M. D.

The Manual of Obstetrics. By Thos. F. Cock, M. D.

The Prescriber's Pharmacopœia. By Thos. F. Cock, M. D.

Transactions of the 4th Annual Meeting of the Medical Society of the State of Georgia, held in the city of Savannah in April, 1853.

Transactions of the Medical Society of the State of Pennsylvania, at its annual session held in the city of Philadelphia in May, 1853.

The American Journal of Science and Arts, by Professors Silliman and Dana.

Proceedings of the American Pharmaceutical Association, at the annual meeting held in Boston, August, 1853.

Yellow or Malignant Bilious Fever in the vicinity of South-street wharf, Philadelphia, 1853. By Wilson Jewell, M. D.

On the treatment of Vesico-Vaginal Fistula. By J. Marion Sims, M. D., of New York.

We are happy to learn that Dr. Sims has been restored to health, and is devoting his fine abilities to the special treatment of Vesico-Vaginal Fistula and similar accidents of parturition. He has removed from Montgomery, Ala., to the city of New York, where he will doubtless take a high position among the surgeons of that place.

Fracture Tables. By Frank H. Hamilton, M. D., &c., &c., with a supplement by John Boardman, A. B., comprising an analysis of 461 cases of Fractures.

Also, a large number of Medical College Circulars, Addresses, &c.

We hope to cull valuable materials from these works as opportunity will permit.

Obituary.—It becomes our painful duty to announce the early and unexpected death of Dr. BRICE T. GAITHER, late of Oxford, Ga. He died on the 21st Sept. last, in the 26th year of his age, of a pulmonary affection which he contracted during his attendance upon the Lectures in a Northern institution, and which he attributed to the severity of the winter climate. He was a man of spotless character and high promise, whose loss will be lamented by all who knew him. M.

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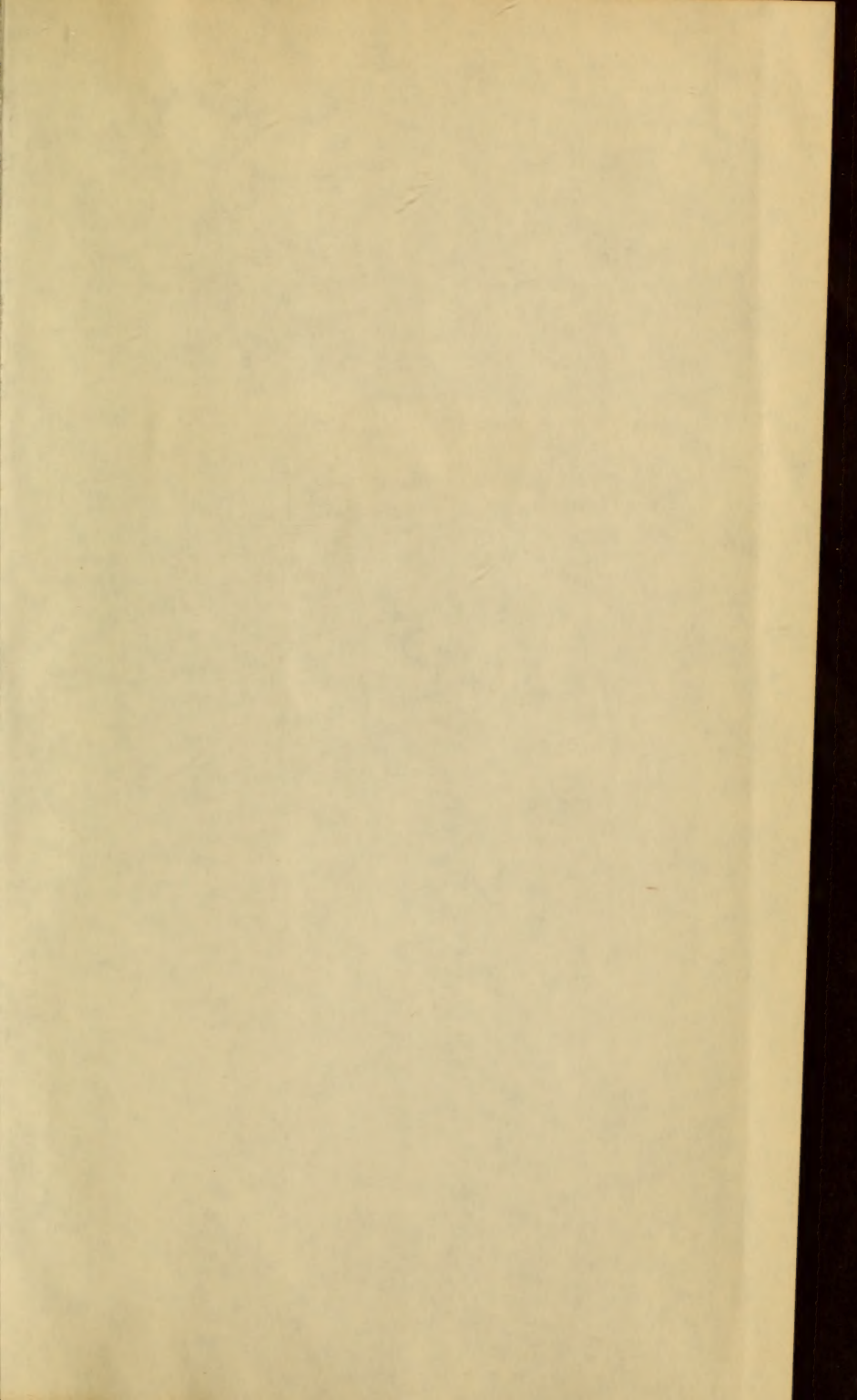
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